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PART FIRST.

Analytical and Critical Reviews.

Art. I.

Clinique Chirurgicale de l'Hôpital de la Pitié. Par J. Lisfranc. Tome premier.—*Paris*, 1841. 8vo, pp. 696.

Clinical Surgery of the Hospital of La Pitié. By J. Lisfranc.—*Paris*, 1841.

It is generally understood that M. Lisfranc, in his own estimation, is the first surgeon of this or any other age. To the accuracy of this sweeping result, the profession at large wisely demurs; yet is not unwilling to admit that, since the death of Dupuytren, he has certainly no superior among the professors of operative surgery in the French capital, and that to his professional opinions much respect must be at all times due. He has been for many years eminent as an hospital surgeon; it seems generally admitted that the extensive field for observation afforded him in la Pitié has not been uncultivated; and when the results of such experience are laid before us by his own will and deed—not by the furtive or at all events subordinate pen of an *Interne*—they will deserve no hasty or slight perusal, and are likely soon to engross a large share of public attention. We have, accordingly, lost no time in carefully investigating the volume; and shall now endeavour to lay before our readers a practical digest of its contents, with such commentaries as the subjects naturally suggest.

Throughout many a page of this book there is entwined an amusing vein of self-sufficiency and importance; not seldom diversified, however, by snarling and sneers directed against those who venture to disagree with the author in opinion. But there is one redeeming circumstance, to which we gladly advert. Dupuytren, alive, was often loaded with reproach and abuse, as little measured as merited; dead, his name is ever associated with honour and commendation. Why should a bright spot such as this be obscured and dirtied by others of a darker and less pleasing hue? May we not hope that reform thus well begun against one blemish may be followed by removal of them all?

In the preface, after having well asserted for clinical instruction a

more prominent place than is too frequently allotted to it in the curriculum of medical study,—yet admitting the necessity of its being accompanied with a sound knowledge of anatomy, physiology, pathology, and therapeutics,—he justly reprobates "concealment and exaggeration" as two most dangerous and deadly foes to the advancement of medical science; and if report speaks truly, this objurgation falls not without especial cause upon his fellow countrymen. The hint, coming from a friendly hand, will, we trust, be improved as it ought: and they will not then have to blame themselves for assisting to retard the event with an aspiration towards which he concludes his preface—"Medicine counted among the exact sciences!"—a consummation devoutly to be wished, doubtless; yet, according to present seeming, scarcely removed from the category of Utopianisms.

On the uvula. The first chapter of the Clinique treats of this troublesome little organ. 1. M. Lisfranc believes, with Richerand, that one function of the uvula is to forewarn the pharynx of the arrival of ingesta; by its touching this latter organ it may be supposed to create the impression on the glosso-pharyngeal nerve necessary for the production of deglutition by reflex action, as so well explained by Dr. Marshall Hall; and besides, the great number of glandular follicles which it contains may be supposed to supply mucus to favour the passage of the alimentary bolus. Thus the organ is connected with deglutition. But another duty is assigned to it by our author; viz. to prevent nasal mucosities from getting into the glottis, during emunctory efforts by strong inhalation o air through the nasal fossæ. And he states that when the uvula has bee either destroyed or disabled, mucus from the nose will be found enterin the air-passages, unless inspiration be effected slowly and with much caution. 2. The uvula is subject to many diseases; but the most prominent, by frequency and importance, is its elongation; when permanent, caused by paralysis of its muscular fibres. By many this affection is not treated with the respect it merits. According to the part on which the elongated uvula acts, it becomes the cause of various and important maladies; and we may not unfrequently find cases of cynanche, cough, hoarseness, nausea, and gastric irritations, which may have resisted every form of treatment, and yet, like most other affections, will at once yield to removal of their real cause. For example; an esteemed member of our profession had for several years laboured under a most distressing cough. "One day Mr. Liston happened to look down his throat, and found the uvula apparently interminable, its free end being lost in the deep fauces. A cough placed it suddenly on the dorsum of the tongue, where it uncoiled itself to the extent of fully three inches, with a fimbriated extremity, like a thin fallopian tube. By the stroke of a pair of scissors the organ was abbreviated, and the cough cut short at the same time." 3. The temporary elongations arising from excited vascular action of a chronic character, will yield readily to the nitrate of silver, or some stimulant powder, as pepper or ginger, applied directly to the par But much mischief is done by using such stimulants indiscriminately; i acute inflammations of these parts they must materially aggravate the disease; and they ought never to be employed except when the action has been chronic from the first, or when it has been in a great measure subdued either by antiphlogistics or by time. 4. Elongation by perma-

nent paralysis of the muscular fibres will not yield to such measures, and must be got rid of by cutting instruments. M. Lisfranc's mode of performing this little operation is nearly the same as is usually practised. He advises that it should be at once extirpated *in toto;* as, according to his experience, return of the elongation will otherwise ensue, bringing with it a necessity for repetition of the curtailment. On this point our experience differs. We have not met with such return of the disease after mere abbreviation; and even if we had, we should still encounter the risk of a second operation, rather than recklessly destroy an organ which has doubtless more than one good office to perform. 5. We cannot leave this subject without expressing our regret that the misdemeanours of the throat-cutting stammer-curers had not been made public before the penning of this memoir; for doubtless it would have been most gratifying to true surgery to have heard how soundly our author should have rated them.

The application of leeches. On this subject M. Lisfranc favours us with many canons: we must content ourselves with a few of them. 1. The cicatrices of leech-bites being often very apparent, we ought to refrain, if possible, from applying them to parts habitually exposed; if used there, the animals should be small. 2. In children and females of delicate skin the course of large veins should be avoided, especially in the neck. 3. Leeches on the eyelids produce unseemly ecchymosis, and often an œdematous erysipelas; they should be placed, instead, on the temple, along the roots of the hair, or behind the ears. This statement we think is too sweeping. To the general eyelid we grant that leeches are inapplicable, for the reasons stated; but we are in the habit of placing them on the inner angle, immediately beneath the tendon of the orbicularis—limiting them to that spot; and not only do we get much blood, but besides untoward results are a rarity. 4. Leeches to the inner surface of the eyelids are ineffectual as evacuants, and the bites prove injuriously irritant. Consequently, scarifications are here preferable. 5. In inflammation of the fauces, leeches should be placed over the mastoid processes or behind them; there the results are not seen, and moderate pressure readily commands bleeding. 6. In applying leeches to the epigastrium, let none fasten over the costal cartilages; otherwise, the movement of these is likely to entail a troublesome bleeding. M. Lisfranc has known it prove fatal. 7. In leeching a part where there is much subcutaneous fat but little blood will flow; in such circumstances therefore it will be prudent to increase the number of leeches or aid them by venesection. 8. Do not place leeches where there are many subcutaneous nerves; the pain will be great; erysipelas may result. For example, in leeching the forearm, prefer the dorsal to the palmar aspect. 9. Leeches should not be applied to the mucous membrane of the vulva, nor to the immediate neighbourhood of the rectum; the bites are apt to degenerate into troublesome ulcers; applied round the margin they are equally potent remedial agents. 10. The scrotum, prepuce, the skin of the penis, should not be directly leeched; the pain is excessive; inflammation and gangrene have resulted; when the leeches are placed behind the scrotum on the raphe, the result is in every way satisfactory. 13. By leeching the skin investing the mamma great pain is occasioned, and erysipelas not unfrequently results; the surrounding integument is the preferable site.

14. If possible, leeching of inflamed skin ought to be avoided. Here we most cordially agree with M. Lisfranc, and fear not to pronounce that leeching in erysipelas, for example, is far too frequent in practice; as in the eyelids, it fails as an evacuant, and proves a direct irritant; thereby aggravating the evil. When local abstraction of blood is demanded, in such circumstances, leeches are to be superseded by punctures and incisions, according as the nature of the individual case may require. 16. Leech-bites on a syphilitic bubo are, according to the experience of M. Lisfranc, liable to ulcerate and assume the venereal characters; the occurrence, however, he admits to be rare. 17. Do not leech a fractured limb at the site of the injury, otherwise degeneration of the bites may materially interfere with the efficiency of the retentive apparatus. 18. Leeching seems to be a favorite mode whereby M. Lisfranc combats strangulation of hernia; for obvious reasons, the leeches are not applied over the tumour, but in its neighbourhood. 19. When in doubt as to the nature of a tumour, leeches may sometimes seem a good tentative application. Do not apply them *to* the tumour, however, but *near* it; otherwise, should the swelling prove carcinomatous, these leech-bites may be the means of accelerating the open or advanced condition of that loathsome disease.

Touching this subject of leeching tumours, simple or not, with the view of obtaining absorption of the adventitious growth—we are sorry to find M. Lisfranc apparently so pleased with the proceeding. We hate it altogether, along with the whole catalogue of absorbents and discutients in such circumstances: having fully made up our minds not only as to their total inefficiency to do what is expected of them, but also as to the certainty of their doing evil; and we gladly avail ourselves of this opportunity to record our opinion upon the subject. It is only on the simplest form of tumour, the mere enlargement of texture, or simple sarcoma, that leeching and discutients can have any beneficial effect. This is the only adventitious growth that will or can yield to discutients. Some others may have their onward progress delayed by occasional leechings; but this is fruitless as to cure; and, as consuming valuable time which ought to have been employed in the only radical mode of treatment, the practice becomes absolutely hurtful. All other tumours are injured by counter-irritants, and other so-called discutients of the active class; for instead of stimulating the absorbents alone, they also, and more especially, excite the perverted *nutritive* action to further activity; the tumour grows apace; forms new attachments; stretches through its capsule; becomes incorporated with new parts; and, what is worse, is certain sooner or later to degenerate as to the character of perverted deposit —from sarcomatous to carcinomatous, encephaloid, melanoid or fungus hæmatodes, or these evil products in varied combination. By suppuration, a scrofulous tumour is broken down; and, after the integuments have given way, it may gradually disappear by disintegration. A fibrous or a fatty tumour may be isolated from its connexions by a suppurative inflammation, and come away in a sphacelus. But such fortunate accidents are rare even to such tumours; and none of these will discuss by absorption. How often do we see this broad and important principle for which we contend illustrated in the theatres of our hospitals? A patient is placed there for excision of a tumour, the integuments over which are

seamed and scarred by leech-bites, pustules of tartar emetic, issues, cauterizations, flayings by acid, &c.; whose history at its origin was one of an obviously simple and then innocuous formation. Then it could have been removed almost by simple incision; but now it will require a tedious, difficult, and dangerous dissection; and its section after all will probably display a structure of such an unpromising aspect as to give a very gloomy character to the prognosis. All this untoward chain of events is in many cases solely the result of discutient treatment, grossly misapplied; and the sooner such untoward attempts are extruded from the practice of surgery the better it will be for suffering humanity.*

On bloodletting in general. We extract the more prominent of M. Lisfranc's remarks. 1. General bleeding is especially adapted to inflammation of the parenchymatous organs, local bleeding to those of the membranous tissues; the one being mainly under the influence of capillary circulation, the other under that of the larger vessels. 2. A healthy system will not bear bleeding so well as the inflammatory; a tolerance of the remedy is induced by acute inflammation;—but only by the acute form, for after its declension bleeding is not well borne, and therefore ought not to be recklessly practised at that period. 5. Inflammation is sometimes so acute as seemingly to concentrate all vital power on the organ affected; thus the patient may be placed in a state of extreme feebleness and depression; and yet bleeding is rigorously demanded to save life and organic change. 6. Females repair loss of blood with extraordinary facility; they consequently require, comparatively, more frequent bleedings, and greater attention to subsequent regimen. 7. When acute inflammation has supervened on chronic, and when by bleeding the pulse and patient are already enfeebled,—as a general rule, bleed no more, although the disease be unconquered. 8. In old people, when organs previously sound are attacked by acute inflammation, and when the symptoms are of a sthenic character, cautious bleeding is advisable. But when the action has been preceded by one of a chronic nature, bleeding is dangerous. 9. In traumatic inflammations, bloodletting is especially useful; and by attending to this principle more than his followers, M. Lisfranc conceives that the result of injuries treated in La Pitié is remarkably successful. In penetrating wounds of the chest, particularly, severe bleeding is supremely advantageous, as proved by the experience of military surgeons. 13. In inflammation of the brain or its membranes, bleeding requires caution, otherwise "the equilibrium which ought to exist between the nervous and sanguineous systems may be destroyed, by excitement of the latter." 14. Abdominal inflammations soon induce prostration of the system inimical to full bleeding. This prostration is the result of a poisoning effect on the system, occasioned by absorption of gaseous and liquid putrescences generated in the deranged digestive organs. 17. Bleeding is not only useful as evacuant and antiphlogistic in large doses; but is also, in smaller, revulsive. Three ounces will not enfeeble the system, and yet do much good to a local disease, by a derivative effect. A few leeches placed at a distance from the affected part draw blood *from* it *to* the

* An apparent exception to this general rule is explained when treating of the extirpation of carcinoma.

site of the abstraction; and according to the place of application, *a few* of these animals may thus be made either stimulant or sedative to a local and depraved circulation. 18. On the foregoing principles, chronic affections of the uterus have been successfully treated by M. Lisfranc, by small revulsive bleedings from the arm; these are supposed to produce a congestion in the upper parts of the body, thereby relieving the lower; and accordingly while the uterus is relieved, headach, dyspnœa, and palpitation are found to occur, annoying the patient considerably for a short period. In some few cases, however, this mode of treatment failed to bring relief.

On the stethoscope as a means of diagnosis in fracture, &c. Shortly after the discovery of auscultation, M. Lisfranc employed the stethoscope in surgical diagnosis, and published a small work on the subject. He used this instrument to detect fractures on the dead subject; and being abundantly satisfied with the result, has now, after long experience, proclaimed it as perhaps the most useful means in the diagnosis of this class of injury. In fact he seems prepared to maintain that there need be henceforth no doubtful case, excepting certain breaks of the cranium, and mere fissures of bone. With the stethoscope, swelling is never so great as to conceal crepitation; while for its detection—and this is fully as important—very light movements suffice. The surgeon is saved from the horrors of error in diagnosis; the patient is spared much torture of manipulation, grievous in itself, and disastrous in its results. 1. Close to the fracture, it is immaterial whether the instrument be used entire or not; but when applied at a distance from the fractured point, it ought to be without its stopper. 2. The more superficial the bone, the more distinct the crepitation, and the slighter the movement required for its detection. 3. When there is *riding* of the fractured ends, crepitation is of course not so easily recognized: moderate extension and counter-extension will render it distinct. 4. Crepitation produced by the fragments of a compact bone are very sharp and strong, and sometimes positively painful to the ear. 5. Crepitation from fragments of a spongy bone is dull, and resembles the action of a file on a hard yet porous substance. 6. In oblique fractures, it is stronger than in transverse. 7. When fluids are effused around the fracture, besides crepitation we have a noise resembling that which the foot produces in a leaky shoe during wet weather. 8. When the fracture is complicated with splintering, crepitation is accompanied with a noise resembling that made by angular hard bodies rubbing against each other. 9. When fracture is combined with wound of the soft parts, besides crepitation we have sounds resembling those made during strong expiration and inspiration, the mouth remaining wide open. 10. Dislocation cannot be confounded with fracture; for the noise produced by displaced articulating surfaces is light, dull, limited, and plainly caused by polished and moist surfaces moving on each other. The working of tendons in their sheaths is still less likely to be mistaken. 11. As the eye with the microscope, so the ear with the stethoscope, is not to be trusted until after a preliminary *education.* 12. In regard to fractures of the radius, Lisfranc agrees with the majority of experimental surgeons in thinking that Dupuytren has been too decided in his declaration as to the impossibility of luxation of the wrist occurring without fracture of the distal extremity of the radius. That

such a combination usually exists, is beyond doubt; yet it seems equally well ascertained that such combination is not invariably essential to the existence of the dislocation. Cases in illustration are stated by M. Lisfranc, in support of this view; with most surgeons, as already stated, it scarcely needed this corroboration. 13. He also lends the weight of his authority to the occurrence of flexion of the long bones, especially of those in the forearm, in children, and even in adolescents as far advanced as fifteen or eighteen years, without fracture; and that such bending will prove permanent if not suitably treated, muscular action alone being totally inadequate to remove the deformity. Lately, traces of skirmishing on this subject were to be found scattered in the journals of this country. But we have not considered it necessary, now or then, to grapple seriously with the question; being convinced that every experienced and judicious surgeon has long since embraced the opinion as here stated. 14. Detection of stone in the bladder by sounding, even in these days, is not exempt from fallacy; and M. Lisfranc with his stethoscope is likely to aid in the investigation. Desault mistook a fungous tumour of the bladder for a stone; Dupuytren cut into the bladder, and found no foreign body there but the point of his own finger; and similar blank drawings of the covert have occurred in our own experience. According to our author, the stethoscope is to be placed on either the pubes or the sacrum; if the sound be then introduced and made to impinge on a stone, the *click* will be heard so very distinctly, under any circumstances, as to render the occurrence of mistake in diagnosis almost impossible. 15. Biliary calculi are *sometimes* to be detected by the stethoscope; our author in one case by its careful use was sensible of a noise, in the site of the gall-bladder, resembling that produced by small stones firmly impacted and rubbing on each other; and inspection after death showed three small gall-stones in the usual site. 16. M. Lisfranc would also extend the use of the stethoscope to sonorous foreign substances, lodged, or suspected to be lodged, in various parts of the body. For foreign matter in the air-passages, it has long been in efficient use.

Fractures and their treatment. 1. Some people, as we have already stated, took the liberty of asserting that bending without fracture did not or could not occur. We have already rebutted them; and now do the same good office to those who may venture to deny that a bone is sometimes partially fractured; that is, bent to a considerable extent, with more or less solution of its continuity in the osseous fibres at the convexity of the curve. Such accidents are not unfrequent; their peculiarity is that when by suitable manipulation the curve has been effaced or nearly so, crepitation then becomes distinct. Mr. Liston in his work on Surgery dwells particularly on this form of accident. M. Lisfranc entertains similar opinions on the subject. Sceptics have taken another step, as if desirous of securing unity in fracture, as in the arts and the drama, and have denied the existence of longitudinal fracture; apparently insisting that all bones shall break in one uniform manner, consistent with their own peculiar creed. With M. Lisfranc we agree in disagreeing with them, appealing to the evidences of museums, especially those enriched from military practice.

2. In regard to the use of retentive apparatus in treating fractures, there are two extremes of practice—delay, and severity of early applica-

tion. M. Lisfranc, we think, inclines too much to the former. We are ready to admit that nothing can be more injudicious than the tight rolling of a bandage on a much shattered limb soon after infliction of the injury, especially if the apparatus be placed with the intention that it shall remain for some considerable time unlooked to and undisturbed, and more especially if the degree of pressure be not uniform, as is too frequently the case; inflammation, abscess, gangrene, are most likely to be thus induced, of course with severe attendant injury to the system; and even failing these, experiment has shown that tight pressure over the site of fracture is likely to prevent the formation of callus. Nevertheless we are equally prepared to contend that there is no more effectual means of kindling and keeping up inordinate excited action in the part, than jactitation of the broken fragments by the voluntary and involuntary movements of the unrestrained limb, and the consequent increase of injury which such motion cannot fail to occasion; inflammation, abscess, gangrene are just as likely to result from this extreme as from the opposite; if abscess form, and suppuration continue, callus will not form, or will at least be deficient—for inflammation proper is as hostile to the formation of callus, as to the healing of a flesh wound either by adhesion or granulation; if the accident stop short of this, the callus will probably prove greatly excessive, and more or less deformity will inevitably result. And therefore we agree neither with M. Lisfranc nor with his antagonists on this point; but along with, as we believe, the majority of good surgeons, place the limb immediately in apposition, and retain it so, by suitable retentive apparatus—applied loosely, so as merely to restrain motion; and often looked to, and shifted if necessary, in order that no undue pressure may be exercised upon the limb, and especially on any isolated portion of it. This we maintain to be common sense, and we believe it to be common surgery now-a-days, at least upon this side of the channel. In brief, we are advocates for immediate application of retentive apparatus, light, suitable, and temporary; and of such a nature as to admit of modifications of tightness being readily accomplished, without painful and injurious jarring of the fracture. Of course, the more severe the injury, the more guarded and watchful the use of compression in any form, and with any view.

3. In prophylactic treatment addressed to the general system, M. Lisfranc is very energetic. He bleeds from the arm frequently, during the early period of treatment, and, he says, with the best effects, in those cases of severe injury where undue vascular excitement is probable. We agree with him thoroughly in principle, but may differ slightly as to the mode of obtaining the result; and are accordingly inclined to doubt that an imperative necessity exists for so copious an abstraction of the vital fluid as he would indicate. With one well-timed bleeding, followed by antimonials, sedatives, &c., and with bread and water as the staple commodity of diet, we believe that inflammation and its deleterious consequences may be warded off in a case of bad fracture just as effectually as by the more heroic treatment of M. Lisfranc, and with far less injury to the constitution. One principle in his bleedings is a good one; to choose the site of venesection as far as possible from that of the inflammation; that the remedy may not only be depletory but revulsive. And whether we agree with him or not as to the extent of such depletion, we

are free to admit that he has done good service to surgery by turning attention more closely to the subject; being fully convinced that by many otherwise excellent surgeons *such* treatment is too much neglected, to the detriment of both the patient's safety and their own credit; confining their attention almost solely to the local treatment, thereby doing but half their work; and seldom thinking of the general system at all, until it is too late to avert the mischief which uncared for disturbance there has induced. We often find that people far advanced in life make remarkable recoveries from fractures tolerably severe. It is by nature doing for them what the surgeon ought to do for adults; warding off inflammatory action—the great foe of union both in the soft and hard tissues.

4. If it be wished to abstract blood from the site of the injury, do not apply leeches directly to the part; otherwise the bites are sure to prove troublesome.

5. When fracture is complicated with wound, cease from bleeding and other evacuant treatment, when pus is about to form; otherwise, according to M. Lisfranc, you will induce reabsorption of the pus, and sad effects upon the system. However explained, this is a sound practical precept. More especially, let evacuant treatment cease when pus is profusely secreted; for now the patient is fast approaching, if he have not already attained, a time when all the resources of his system will be called upon to bring him through his trials and difficulties; and when further reduction of these resources by injudicious medico-chirurgery, would probably entail loss of the limb, of life, perchance of both.

6. As to position; a broken limb should generally be placed so as to relax the muscles implicated, for obvious reasons; hence the posture of semiflexion will be found the most generally applicable. Doubtless; but only when this is compatible with other indications of treatment, especially the maintaining of the due length of the limb. Accordingly, we disagree with M. Lisfranc in treating fractures high in the thigh with the limb semiflexed, being convinced by experience that it is only by permanent extension and counterextension as obtained by Desault's splint in the straight position, that shortening of the limb, and that to a considerable extent, can be prevented. At the same time we agree with him as to the imperious necessity there exists for great watchfulness in the use of this apparatus, in old and delicately framed patients; taking care that the pressure may be at no time excessive, otherwise sloughing and even fatal results may ensue. There is nothing good in this world whose use may not be recklessly and readily converted into abuse; so is it with many good rules of surgery; and this is one. Place then your limb in a relaxed and comfortable posture if possible; but rather let the attitude be a fatiguing one than encounter the risk of a permanent deformity with impairment of function.

7. Sometimes when, *à priori,* we imagine the half-bent position will prove eminently useful, we are deceived. Thus, while fracture in the upper third of the leg obviously demands the straight posture, fracture in the lower third is most especially suited to that of semiflexion; yet cases will now and then occur, in the latter situation, as we have ourselves experienced, when, on account of some peculiarity of the fracture, apposition cannot be satisfactorily obtained until the limb is placed

straight, and kept so. Failing the one posture then, we must try the other; and not walk blindly in the beaten track, either here or elsewhere.

8. It is the custom of some surgeons to moisten their retentive apparatus, and some use stimulating fluids for this purpose. Nothing can be worse; we do not wish to stimulate the part, but to soothe it. And therefore if the moistening system is to be pursued, let the fluids be as bland as possible.

9. During the first and second weeks, M. Lisfranc *looks at* his fractures every twenty-four hours, or at furthest once in the two days; and he is right. But we are afraid of a false reading here; substituting movement and rough manipulation, for mere watchfulness of the tenderest kind. All meddlesome surgery is bad; and in no class of cases is it more injurious than in fractures. A broken bone being given, and the means asked whereby union may be most effectually prevented, we answer—daily movement of the fractured portions. We knew a surgeon who treated a fracture with great attention, and at the expiry of the usual period of probation granted to such cases, he complained lustily that there was still no union even begun, although he had every day taken care to ascertain whether the process had commenced or not, by imparting free motion to the broken surfaces. To his great sorrow, and the patient's sad detriment, motion between these continued to be as easy and free as on the first day of the injury. It would indeed have been very surprising had the result proved in any way different. We cannot satisfy ourselves too often that all is advancing favorably at the site of fracture, but, at the same time, we cannot too seldom interfere with the position of the limb. There is a wide difference between examining a part, and subjecting it to rude, unnecessary, and injurious manipulation.

10. In compound fractures, a small quantity of pus, cooped up in the interior, may induce the gravest results. This is a most important and sound maxim, and ought to be much more generally acted on than it is. As soon as the matter has formed, the sooner its evacuation is accomplished the better. To talk of delay, and digestion, and maturation, in such circumstances, savours of puerility or senility, or both.

11. Splinters when small, and by adherence to soft parts likely to retain vitality, are to be replaced not removed. M. Lisfranc adduces a case in point; there are many such.

12. M. Lisfranc says that he has seen splinters absorbed. If they remained alive, he is partly right; they may have been vitally disintegrated. But if they died, and became sequestra, we hold him to be unwittingly guilty of an untruth; they could not then be got rid of but by extrusion. As well might he hope to obtain absorption of a bullet or stone. If a settlement of that question ever were required, it was obtained from the recorded experiments of Mr. Gulliver.

13. Remember that the callus often remains comparatively soft and pliable for a considerable period, and that therefore deformities by maladjustment are not to be despaired of, at the end of six weeks or more; suitable means to improve the apposition are instantly to be adopted, and patiently persevered in: not unfrequently, unlooked for success will reward the labour.

14. As diuretics are used for dropsies, so does M. Lisfranc employ them to remove obstinate accumulations of blood, resulting from fracture.

We are not quite sure about his reasoning on this point, but he says that in his practice, such treatment expedites absorption of the effusion. He exhibits purgatives with the same view. But, as a general rule, purgatives are inapplicable to fracture; they move not the bowels alone, but the limb also; while they may do good, they must do harm; and consequently are to be avoided.

15. For ununited fracture, the preferable treatment is continuance, doubly vigilant and accurate, of retentive measures; the only difference between the treatment of this and ordinary fracture being—that while the one requires but weeks, the other takes months for completion of the cure.

16. There are fashions in surgery as in other matters; and, at present, the starched bandage is all the rage in fractures. This caprice is rebuked by M. Lisfranc, and we think most justly. We do not object to this form of bandaging *per se*, but to its indiscriminate use. During the first period of severe fracture, it is inapplicable; for considerable swelling must occur, requiring proportionate slackening of retentive apparatus, which ought consequently to be light and easily changed. Further on in the case, when the swelling has reached its acme and begun to subside, the starched bandage is still inappropriate; if applied to-day, the limb will have shrunk so far by to-morrow that the apparatus has ceased to be retentive, and the ends of the bones may ride each other at pleasure. If employed at all then, it must be almost daily renewed, and that is foreign to the nature of the application. It is only during the later periods that its use becomes judicious. When the time for inflammatory swelling has gone by, and when further atrophy of the more or less swollen limb is improbable—then the permanent, fixed, and unyielding nature of the application ceases to be detrimental, and becomes most salutary. If it be used sooner, it ought not to be in mass, but after bisection, so that the apparatus then comes to resemble two neatly and closely fitting splints of the ordinary kind. But, on the whole, we believe that such splints will be found more advantageous in diseases of the joints—chronic and acute, curable and hopeless—by obtaining the all important *rest* of the diseased parts. It is by better attention to this point by such means, that superior success has been attained by modern surgery in this sphere of disease.

17. In fracture of the ribs, M. Lisfranc is not contented with mere bandaging of the chest, but by placing compresses over the sternum, takes care that the bandage shall exert its pressure chiefly in the antero-posterior axis, throwing out the arch of the ribs, and thus keeping the broken fragments not only more fixed, but in a more accurate position. He also most properly insists on the pad for fracture of the neck of the humerus being placed well within the axilla, otherwise it must do more harm than good.

18. Fractures of the forearm are to be treated not only by the usual long splints, but also by compresses placed in the interosseous space, to prevent undue approximation of the broken ends in that direction. The same principle extends to fractures of the metacarpal bones. In the latter case, the arched form of the part must also be kept in remembrance.

19. Dupuytren's splint for broken fibula is acknowledged faultless, even by Lisfranc.

Some cancers, apt to be thought deep, are superficial. Some textures seem to resist cancerous disease more stoutly than others; for example, fibrous membranes, not excluding the peritoneum and pleura. By observing this, our author was led to suspect certain affections, either malignant or seeming to be so, of being more limited than is generally imagined; by acting on this idea, he states that he has been able to save important textures which would otherwise have been sacrificed by the knife; and that, after such limited operations, the disease has not returned. 1. In apparent cancer of the penis, for example, by a preliminary incision having ascertained that the sheath of the organ is yet intact, he has simply dissected off, very carefully of course, the superficial disease, leaving the important parts beneath little if at all impaired in their functions. With this result he is the more especially delighted, having found in all his experience that amputation of the penis, whether in young or old, is invariably followed by dulness, inactivity, despondency; in fact, by complete emasculation in every sense of the term. Further, he says most truly, "the true end of surgery is to preserve, not to destroy." 2. In apparent cancer of the tongue he has been able, on the same principle, to save that truly important organ, (his first case is that of an advocate, a class to whom that dangerous little member is peculiarly valuable), by dissecting off disease not involving the muscular texture, instead of, after the usual fashion, removing the whole or a large portion of the tongue, by knife or ligature. 3. In the vagina and rectum, in the eyelids and eyebrows, over the loins, and in other situations, he has followed similar practice with success. 4. In many cases he states that cancerous affections of the nose do not extend so deeply as to involve the cartilages, and that consequently ablation of their coverings ought to supersede amputation of the whole organ. The cartilaginous texture, he says, granulates kindly enough.

Now, the foregoing is an excellent principle to have mooted, but the practice requires confirmation by others than M. Lisfranc. He is well known to be too easily persuaded of the cancerous nature of morbid products, and therefore we have guardedly termed the affections to which he here alludes, as "apparently cancerous." For the cases are far too loosely described, and the description which is given far too unsatisfactory, for almost any one but the author to admit that the disease is even malignant. If it were so, only in its incipient stage could such limited dissection prevail against it. And, most certainly, we are not to be induced, by any such reasoning, to forego the ancient and sound maxim of surgery, to cut as widely of actual carcinoma, and more especially of cancer, as circumstances will allow. When you cut (for cancer) do not cut close.

Some remarks on cancer. 1. M. Lisfranc is satisfied, by actual experiment, that the disease is not contagious. 2. But, scoffing too much at the idea of general infection of the system by the disease, he is led to espouse sanguinary practice in advanced cases of cancer, inconsistent with prudence and humanity. 3. Cancer apparently the result of external causes is less liable to return after operation than when the disease has seemed idiopathic. 4. Permanent pimples of the face are harmless in youth, and may remain so in old age, if not irritated; if injured, they are apt to degenerate, and to assume the characters of malignant action: as soon as such

degeneration threatens, they are to be extirpated. Would it not be wise to go a step further, and root them out at a still earlier period? It is always better to prevent than to cure. 5. The bistoury is preferable to caustic as the agent of removal. The latter is more painful and less certain; sometimes failing to destroy the whole morbid structure, sometimes killing parts which are sound. But if the patient be "horribly afraid" of sharp instruments, and more especially if the disease be superficial, caustic may be employed with advantage. In superficial suspicious ulcers of the prolabium, we have often used the potassa fusa with the best effects. 6. Nitrate of silver (used not as a caustic), leeching, and general treatment will often succeed in curing affections of the tongue, lip, and cheek, which simulate cancer, but which the experienced eye discovers not to be so. And we are inclined to suspect that M. Lisfranc might beneficially apply this principle to his own practice more extensively than he has hitherto been in the habit of doing. 7. "All is not gold that glitters." Desault and others who fancy they have cured cancers by compression are mistaken; they have, by the combined agency of ulceration and absorption, dissipated a morbid product, but it was not carcinomatous, though at first it may have seemed so. 8. Will cancer cure itself? No! A clap will run itself out; tic douloureux will disappear spontaneously, after the whole pharmacopœia has been swallowed fruitlessly; a scrofulous tumor will commit suicide; a stone in the bladder may find its own lithrotriteur; but no one, unaided, shall rid himself of cancer. 9. In obstinate ulcers simulating cancer, M. Lisfranc advises mercurial treatment, although no primary venereal affection have attacked the patient previously, having some vague notion about the hereditary qualities of this much-abused disease; as thus—the father probably had syphilis, he transmitted the virus to his fœtal son, the virus remained latent in the system of the latter for many years; at length it has ventured out; but, as if still ashamed to show itself, comes in disguise; natheless, the mask is to be plucked off, and the sly intruder is to be knocked on the head by means of its ancient and inveterate foe, mercury! This requires no comment. 10. Marked and extreme cachexy is not of itself sufficient to contra-indicate removal of cancer by operation. Granted; but when we are told that glandular enlargement in connexion with the original disease, combined with marked cachexy, is still no bar to operative interference, we protest most stoutly against such dangerous doctrine, and regret extremely that M. Lisfranc should be thus an encourager of rash and hopeless knife-work, while his brethren, at least on this side of the channel, seem to have come with one consent to a diametrically opposite practice, more consistent with humanity, good surgery, and common sense. By such a combination as we have just mentioned proof is plainly afforded that the disease has obtained a firm hold of not only the part and its neighbourhood, but of the whole system, that it is consequently impossible for any operation to cure the disease; and surely protraction of life and alleviation of suffering are more likely to be obtained when the malady is allowed its own way, than when its inroads on the frame are assisted by the shock of a severe operation, and by the ghastly draining wound which must inevitably result. Not only is the patient thus made doubly a sufferer, but by the failure which is inevitable, foul discredit is done to surgery. And this practice is advo-

cated by M. Lisfranc! *et tu Brute!* 12. Cancers of the skin, and those occurring in adventitious tissues are the least likely to return after operation. 13. Our author believes cancer to be in many cases hereditary, and yet, other circumstances being favorable, does not hold a well-grounded suspicion of such hereditary tendency to forbid operation. 14. A carcinoma stationary, and causing but little annoyance, in a patient of a far advanced age ought not to be interfered with. The disease has taken a nap, and it would be folly to disturb its slumber! 15. The superficial tubercles often found clustering around a carcinoma are not in the substance of the skin, as generally supposed, but subcutaneous, with the superimposed integument stretched and much attenuated. 16. When cancer returns is it possible to destroy the formation without either knife or caustic? M. Lisfranc's answer is affirmative; but the proof brought forward to support the bare assertion is not satisfactory. 17. The swelling of carcimona does not consist *entirely* of carcinomatous tissue; the circumference is often composed of simple effusion and deposit, as in swellings of inflammatory origin. On this general fact our author founds the following neat surgical maxim. When a carcinomatous tumour is of such a size as to render extirpation dangerous, either by the mere extent of wound, or by encroaching upon important vessels, viscera, or cavities, neither rashly operate, notwithstanding, nor yet abandon the patient to his fate, provided there be no other circumstances unfavorable. But adopt such means as are likely to dissipate ordinary hypertrophy of natural tissue; occasional and moderate depletion in the neighbourhood will remove the vascular excitement, and diminish the heat and pain; discutients, the best of which class here is gentle pressure, will gradually remove the deposit; but it is obvious that the utmost caution is necessary in carrying out the latter indication, otherwise the malignant action will be stimulated, and it is our object to have nothing to do with that at present. By due persistence in such preliminary treatment, the tumour will be ultimately reduced to what is merely carcinomatous—probably one half, or nearly so, of the whole former bulk. And now the operation becomes not only practicable, but comparatively simple and safe. 18. However, let the superficial observer beware of imagining in such cases that because one portion of the swelling has yielded to simple means, the rest will follow its example; acting on such a supposition, as we have already seen, he will at a certain point cease to do good, and then go on inflicting positive harm; the *common* product may be dissipated, the *perverted* cannot, and the latter will be but stimulated to further development by the unsuccessful attempt. 19. M. Lisfranc agrees with other good surgeons in cutting wide of the tumour, leaving about half an inch of sound tissues all round; and also in looking carefully in the wound for little tubercles which are apt to be overlooked, and which, if left, are certain to regerminate the disease. 20. A carcimona taken away, and cicatrization completed, the surgeon's duty is not ended. Relapse is probable: certain means tend to prevent it, and these are to be sedulously employed. Chirurgery consists not solely of "carpenters' work," but both in science and practice goes hand in hand with medicine. The patient is to be reassured, and kept from brooding in despondency on anticipated relapse; every one knowing that mind and body react closely on each other. If congestion threatens, bleeding is prudent, de-

pleting or revulsive, according to circumstances. Excretions and secretions are to be closely looked to, and put to rights if necessary. Our author has not forsworn the ancient allegiance to hemlock as supreme in this affection, and accordingly gives a grain of the powder in pill every morning; the dose is doubled at the end of the first week, tripled at the end of the second, quadrupled at the end of the third; and this last dose is continued. He believes this medicine to be "antinervous and discutient," and only in very few instances has found it irritate the digestive organs or excite the nervous system; in such cases of course its use is contraindicated. Against the glandular affections not avowedly carcinomatous, he directs the hydriodate of potash; this is doubtless good practice, but would become the opposite did malignancy declare itself in the tumours. Friction around the cicatrix, and compression of it are recommended, but we dread both; they are apt to overstimulate, and all stimulation here is dangerous. When he adds that "all causes capable of irritating the cicatrix are to be avoided," we think he contradicts himself, and therefore readily agree with him. Motion, especially, should for a long time be sedulously avoided; the difficulty of obtaining this indication complete, after removal of carcinoma or cancer of the lip, is the main cause of frequency of relapse after this operation, and a good illustration of the advantage of rest. 21. In hopeless cancers, starvation, it is well known, does harm. But after removal of cancer, and during hopes of permanency of cure, the diet cannot be too guarded, all exciting aliments being religiously avoided.

On the dressing of wounds. In this particular the French are behind us. In all wounds, it is the usual custom in Paris, to plaster on what they term their "first dressing," and leave it undisturbed until it is loosened by a soaking suppuration. M. Lisfranc naturally objects to this concealment of the wound during an important and often critical period, and wishes to see what it is doing, after the first twenty-four hours at latest. To pull off dry lint, however, at that time, would inflict great pain on the patient, and do a positive injury to the part. He accordingly makes *his* first dressing of easy removal, by larding well its surface, and taking care that it is sufficiently large to rest with its margins on sound skin, apertures being cut in its centre for the escape of fluids from the wound. This he considers a great improvement. We would be sorry to have it in exchange, however, for the simpler and more agreeable, as well as more efficient treatment, commonly known as the "water dressing," cold or hot, simple or medicated, now in use by the better-informed surgeons in this country, and for which they are indebted to Liston, Macartney, and others—zealous and successful improvers of this important department of practical surgery. We cannot help thinking that climate and season are not alone to blame in the marked failure of union by the first intention in the French capital; with their present system of dressing, it would probably be surprising did suppuration ever fail to be established. And as M. Roux occasionally favours us with a flying visit, we would suggest that he should draw an impartial "parallel" on this point; as we presume to think it might not be without advantage to Parisian surgery. Nay, we are free to admit, that even in this country the improved mode of dressing is not yet sufficiently attended to. And accordingly we have felt it to be our duty to

place before our readers the following condensed view of the practice, partly for the benefit of the future parallelist, partly for the sake of our junior professional brethren. (*a.*) In regard to adhesion of incised wounds, delay in approximation is advisable. Cold-water dressing is applied until bleeding has ceased. Then the wound may be closed; but it is better to wait a little longer until the reparative process has commenced. This preferable condition is indicated by a glazed appearance of the cut surface. (*b.*) Delay, and the cold applications are, besides, useful in preventing secondary hemorrhage. Should this occur, the open state of the wound is favorable to the adoption of means necessary for its arrestment. (*c.*) In effecting approximation, stitches may be employed when necessary; but they should be few, and in all cases their use is temporary. In a great number of instances they are entirely dispensed with. The principal and permanent retentive means are slips of non-irritating isinglass plaster.* And as soon as these have been applied, and become fixed in their hold, all sutures are removed. The harelip operation, and some other wounds, are exceptions. (*d.*) The isinglass plaster, being translucent, admits of a constant and complete surveillance of the uniting process in every part of the wound. It does not irritate the surface on which it is applied, is very adhesive, and seldom, if ever, requires renewal during the cure. (*e.*) No other dressing is applied. When coaptation has been effected and made permanent, all the manipulation necessary to adhesion is accomplished; dressings additional to the plasters, therefore, can do no good, may do harm, and are to be avoided. Cleanliness of the part, by gentle and occasional wiping, not of the wound but of its neighbourhood, is all that is further requisite. (*f.*) By this mode of dressing, the occurrence of adhesion is rendered much more probable; the patient is saved much pain and irritation, and the surgeon is freed from infinite trouble and annoyance. Should adhesion fail, the parts are in a much more favorable state for assuming the other process of union than they would have been, in similar circumstances, under the old system. (*g.*) In regard to union by the second intention, no stitches are employed. Approximation does not require to be complete, and the cure is effected by simple replacement and attention to position. (*h.*) If acute inflammatory action threaten to prove excessive, active and timely antiphlogistics must be resorted to, according to general principles. (*i.*) Usually, water-dressing is the only application, unless during the latter part of the cure; at first continuously cold, to retard and limit the inflammation; then hot, so as to moderate the excited action, at the same time favouring the escape of foreign bodies, if there be any; afterwards it is tepid and comfortable, as simply detergent. When gentle stimulation of the granulating surface is required, towards the end of the cure, the water is medicated by stimulants, as the sulphate of zinc, proportioned in strength to the exigencies of the case. (*j.*) Heavy, fetid, cumbrous poultices, and greasy, rancid, irritating ointments are superseded. (*k.*) In the last stage of union, both by the first and by the second intention, support, with mild and uniform pressure, is not unfrequently advisable. It is effected by plasters, by bandage, or by both. (*l.*) The process of union by the second intention may sometimes be dexterously supplanted by that of adhesion. The

* Liston's Plaster, vide our last Volume, p. 265.

period when this can be accomplished is, when the vascular action has subsided from the inflammatory and suppurative to what is simply essential to reproduction. When the young and active granulations are then brought into close contact, they quickly coalesce, a great part of the uniting process having been effected previous to apposition, and the secretion of pus having very much diminished. (*m.*) Two prominent exceptions to the preceding rules are,—1. When the cut surface is such that every point can be placed in close and accurate contact, without the risk of coagulum or any other obstacle to adhesion being interposed; then, twisted sutures constitute the whole dressing. 2. When the wound is so situated that neither plasters nor twisted sutures can be applied, then the common interrupted sutures must be employed, as few in number and of as short duration as possible.

On the treatment of Sprain. There is no difficulty in obtaining plenty of remedies in this case. The only nicety and skill is in knowing the proper time at which to use them. Cold, leeches, pressure are excellent means of treating sprain, but only when each is in its own place. The more important indications of treatment are simple; to prevent, diminish, or remove inflammatory action; and to favour reabsorption of the effused fluids. For the first indication leeches are useful, but they are totally inoperative when applied immediately after the injury, as is often the case, with the view of abstracting the actually effused blood; moreover, their bites may lead to unhealthy suppuration, and even to gangrene; for it is well known how imprudent it is to expose extravasated blood to atmospheric accident. M. Lisfranc supersedes leeches by venesection, this being less liable to injure locally, being equally effectual in combating inflammation, and being likely to contribute somewhat to the fulfilment of the second indication; because, according to the theory of M. Magendie, when we act directly upon the venous system, and suddenly diminish its amount of blood, the veins become "thirsty of liquid," especially if the patient himself drink sparingly; and then they are supposed not to be unwilling to slake their thirst even on the stale fluids of effusion; hence absorption is likely, under such circumstances, to be speedy. This, for removal or subjugation of excited action. To prevent it, cold is valuable; and just for this reason the proper time to employ this much-abused remedy is immediately after the accident, before inflammatory action has been lighted up; after this has commenced to form, persistence in the use of cold is downright folly; it must be superseded by poultice, fomentation, and other means suitable to combat a growing inflammation. After five or six days the swelling is diminished, and has become more œdematous than inflammatory, the effusion is partly reabsorbed, and the pain is much abated. In short the excited action has passed away, and now is the time to abandon depletion and fomentation, and have recourse to friction and bandaging; in order to expedite fulfilment of the second indication—restoration of the parts to their normal condition. But to use either friction or bandaging at an earlier period, as is very commonly done, is plainly to thwart nature and aggravate inflammation. Above all things let *rest* of the injured part remain complete and undisturbed throughout the whole period of excitement, otherwise that period will prove unnecessarily protracted; and this is a point, by the bye, on which our author is culpably silent. Neither let the pa-

tient have an imprudent haste to be well, but let an almost imperceptible gradation bring back the part to its former functions. By following such a mode of treatment in sprain, M. Lisfranc asserts, and we believe him, that he has never failed to effect a cure, within a comparatively short period, and to prevent weakness of the joint which might otherwise leave it prone to repetition of the injury. In fact he declares himself happily ignorant of this "pretended feebleness" which others bemoan so much. In some cases the sanguineous or sero-sanguineous effusions resist the ordinary routine of treatment. Against such obstinacy M. Lisfranc directs purgatives and diuretics, according to the theory we formerly noticed; to the latter we have not the same objection here as in the case of fracture.

Burns. 1. M. Lisfranc adopts nearly the same classification of this class of injuries as did Dupuytren: *a.* Mere erythema, resulting from slight injury of the mere surface. *b.* The rete vasculosum is fairly involved, and the inflammatory symptoms are more decided and serious. *c.* The chorion has been reached, and is more or less injured. *d.* The whole thickness of the skin is killed outright. *e.* The subjacent tissues are involved, and more or less charred. These five forms of the injury may be found all united in one instance, the fifth usually occupying a central position. 2. At four periods the patient's life is in danger; during the period of irritation or shock, the first effect of the injury; of inflammation and its direct accompaniments; of suppuration; and of prostration or collapse. 3. Our author justly reprobates the empiricism, too common in our profession, of imagining certain applications to be specific in all cases of injury by fire, and using them accordingly; and insists that the treatment, both local and general, shall be varied according to the nature of the injury, and the progress of the case. Cold, for example, can only be of use as a preventive of inflammation, or rather as a means of limiting its intensity; the time for its application is immediately after the accident, during the period of incubation; after a few hours have elapsed inflammation has begun and is advancing, and then its farther use not only ceases to be advantageous but becomes absolutely hurtful. It must give place to the suitable antiphlogistics. In irritable subjects too, especially if the internal organs seem prone to the assumption of morbid action, continued application of cold to an extensive burn, even of the first class, must often be omitted, or at all events managed with the utmost caution, otherwise the most serious results may ensue. Cotton and flour are valuable applications to burns of the first and second classes; but to them they should be limited. For if employed in those of a severe nature, they not only injuriously conceal parts where important actions are in progress, requiring supervision, promotion, or repression —but also, by retaining discharge, become filthy, disgusting, and perhaps the source of a fatal irritation to the very textures which they are vainly destined to protect. Bloodletting is often our most trusty means of meeting the brunt of a violent inflammatory attack, as is far from unfrequent in those of the third class; but after suppuration has been fairly established, a prudent man will draw no more blood, knowing that the period of its usefulness has gone by, and that now the system will have to muster all its resources, in order to bear up under an exhausting discharge. Stimulants and tonics are most useful to assist nature under

such circumstances, and the former are often most valuable in warding off an earlier danger from the first shock of the injury; but after reaction is over, and inflammation begun, what but harm can result from the use of either? Local stimuli are of the most signal service in obviating the sluggish tendencies of the sore that results from the separation of the charred sloughs; but how they should be otherwise than detrimental previously to this separation we cannot imagine. Sedatives and narcotics are of good service in the period of excitement; in that of collapse they are more likely to prove fatal. Such principles seem consistent at once with good surgery and with common sense, yet how often do we see them transgressed in the every-day practice of those who in other matters evince a far wiser experience. Laziness we fear is inherent to humanity; and we are all too apt to get into a routine system of treating diseases by their names, and not by the varying symptoms and indications as they appear; a fault not more common than absurd. 4. Meddlesome surgery is our abomination; and accordingly we are glad to find M. Lisfranc strenuously forbidding all attempts to pull away the yet-fixed eschars; let the detached portions be separated as soon as possible by the forceps and scissors; but leave the process of detachment to the more competent hands of nature. 5. Sometimes pus accumulates under the eschars, and according to the general rules of sound surgery is to be evacuated by suitable incision, at an early period; otherwise serious injury will be done to the parts beneath. But reckless scoring of the sloughs as practised by some, with the hope of giving greater efficacy to their stupid "antiseptics," is not only useless but dangerous; for important nerves, veins, arteries, may thereby be wounded, which might otherwise have escaped all but intact from the effects of the injury, of the inflammation, and of the subsequent sloughing. Nature has wisely endowed the arterial tissue especially with the remarkable power of resisting disease, but she is unable to contend with disease and with the relentless steel of a reckless knivesman. 6. The principal object of our author in this chapter seems to be to introduce to our notice *his* panacea for burns—the chloride of soda. Of the chloride of lime he has also a high opinion; but of the two, prefers the former. An abridgment of its character is as follows. It is astringent and sedative. It not only delays the approach of inflammation, but even dissipates it almost entirely. By mitigating the impression on the nerves, not only is pain assuaged but constitutional symptoms materially modified, if not wholly prevented. When there is no slough, breach of continuity caused by the burn is quickly covered by a plastic exudation, becoming rapidly organized, and resembling false membrane; and almost the whole credit of this production is attributable to the chloride. After sloughs have separated, and granulation is considerably advanced, the chloride is again able to finish the process with eclat by the effusion and organization of this membranous product; a process similar, apparently, to what in this country is termed the modelling. The cicatrix thus obtained is more solid than that resulting from the ordinary means; both in this new matter, and in the original tissues that surround it, there is much less tendency to contraction, otherwise so fertile in deformity; indeed so self-sufficient is the creation by the chloride, that the original tissues are scarcely asked to contribute anything by accommodation towards repair of the breach. It is at the same time admitted that

the chloride used during the supremacy of a powerful inflammation is decidedly injurious; that employed with the view of arresting gangrene so caused it is at least ineffective; and that it is most prominently useful in injuries of the first and second degrees. With such exceptions, however, it is *the* remedy in all burns; subduing all untoward symptoms, local and general; shortening the period of cure; expediting separation of the sloughs; producing more favorable cicatrices; and often preserving life, which would doubtless have been lost under the ordinary mode of treatment. This is no lukewarm praise: may experience confirm its truth! 7. The chloride is used in solution; the ordinary proportion is one part of the concentrated solution to thirteen of water; but this is to be varied according to the effects produced. The test of its efficacy is that its application produces an itching and slightly painful heat in the skin, of not more than a quarter of an hour's duration; more or less than this is either hurtful or inert. 8. So salutary are the effects of the chloride in the eyes of our author, that he enjoins even removal of cuticle in order that the application may prove more direct and consequently more efficacious. On behalf of the cuticle we protest against this principle, believing nature's to be at least as efficient a protection to the tender surface as any of artificial construction; and that therefore it ought not on light grounds to be superseded. 9. The mode of application is as follows: a compress covered with "cerat de Galien," and perforated at several points, is placed over the whole surface of the burn. Above it is laid a mass of charpie, at least two inches in thickness; this is saturated with the solution of the chloride and covered with dry compresses; the whole is fixed by a bandage. The apparatus is sprinkled with the solution every two or three hours; and every twenty-four hours the dressing is renewed. 10. After burns of the fourth and fifth degrees, the cicatrices are apt to prove vicious, with more or less deformity. In the treatment, the probability of this must always be kept in view, and endeavours made to produce a large cicatrix composed chiefly of newly created matter. Attention to position will sometimes suffice to fulfil this indication; in other cases, pressure, traction, and escharotics have to be called into use; and in not a few all efforts and skill will prove unavailing. One fact should never be forgotten; the tendency to contract does not cease with the completion of the cicatrix, but continues for many months afterwards; and unless the preventive treatment, by position and otherwise, be continued patiently until the contraction shall have entirely ceased, deformity is certain to occur. In burns of the face, contraction of the original integument is combated by centrifugal traction effected by strips of adhesive plaster, and sometimes with considerable success. M. Lisfranc proposes a modification of this means, likely to prove more effective. Around the breach hairlip pins are applied, their blunt heads projecting over the sore; a plaster, split to near its end, is hooked on each of these, laid down, and held in its place by a general retaining strip laid in an opposite direction. These needles and plasters, proportioned in number to the size of the sore, keep up a traction from the centre to the circumference, antagonist to the natural tendency of the ulcer. Such treatment is of course incompatible with rapid healing; but of the two evils we are undoubtedly to choose the least. Redness of the cicatrix is successfully combated by steady and uniform pressure on the part; and a similar means proves

very effectual in reducing irregularities of the cicatrized surface to one equal level, whether the result of burn or of smallpox: in the face a kind of mask is the most suitable agent of pressure; and it is the more likely to succeed the earlier the time of its use. 11. Position, traction, pressure, not unfrequently fail to prevent hard, prominent, contracting cicatrices. Excision is the remedy still within our reach. On this subject, however, nothing new is brought forward by our author; nor is his hope of success more buoyant than that of his less mercurial brethren; there is no reason therefore why we should enter farther on a subject so unpleasant and unsatisfactory. In the face he agrees that no such operation should ever be attempted.

On venesection at the bend of the arm. 1. This simple and sometimes fatally-facile operation, though conducted on much better principles than formerly, is yet not unfrequently followed by local accidents. 2. When detection of the veins is difficult, the ligature should be applied for half an hour or even an hour, the patient meanwhile frequently contracting the muscles of the forearm. 3. Often there exists an imperious necessity for general bleeding, while at the same time it is impossible to see or feel veins in the leg, foot, wrist, forearm, arm, or neck. Instead of being constrained to perform arteriotomy, which may seem an insufficient substitute for phlebotomy under the circumstances, it is better to cut down on the cephalic vein, as it passes between the deltoid and large pectoral muscles; the operation is attended with no difficulty or danger. 5. Meddlesome fingering of a wounded vein, with the view of compelling it to give forth blood, is forbidden both by reason and experience; phlebitis, always dangerous, is almost sure to result; it is consequently far better to make a fresh opening at another point.

On deligation of arteries. 1. In this chapter, M. Lisfranc commences badly. He has too many instruments. "Straight bistouries both sharp and blunt pointed, dissecting forceps, blunt-pointed scissors, straight and crooked, flexible "sonde cannelées," needles, and several ligatures of different thickness and breadth," do not constitute the most promising array of implements for attacking large arterial trunks, at least to the English eye. With a scalpel, forceps, one needle, and one ligature neither thick nor broad, all the requisite manipulations can be readily and safely performed. And we hold it to be one of the soundest maxims of surgery, that instruments, if efficient, cannot be too simple and few. 2. National prejudice leads M. Lisfranc to omit all mention of John Hunter in the improved operation for aneurism; he wishes to divide the honour of that discovery between Ambrose Paré and Anel, both excellent men in their way, but neither entitled to dispute Hunter's claim to the operation which in this country uniformly bears his name; unless indeed, according to the principle inculcated in a common domestic proverb, concerning the predilection entertained by certain aquatic fowls towards the assimilating organs of fishes; but which being somewhat coarse in the vernacular, need not be more plainly exhibited in these pages. 3. In operating with the view of placing a ligature on an arterial trunk, M. Lisfranc practises a preliminary movement, not thought necessary in this country, except in the case of puncture of the artery aforesaid, viz. pressure by an assistant at a distance from the scene of operation. He must be a very rash or unsteady operator who wounds, so as to cause hemor-

rhage, the artery which he exposes for the purpose of deligation; and moreover, by pressure, such as arrests the arterial flow, pulsation of the vessel is lost, which might otherwise have proved a very important guide in the earlier part of the incisions. 4. For compression, M. Lisfranc agrees with the majority of modern surgeons in preferring the fingers of a steady and good assistant; such pressure is more exact, more limited, less severe, and more easily shifted if necessary, than that of any tourniquet; inflicting less injury to the part compressed, and, in the case of large wounds, causing less injury to the system by sparing the loss of blood. In not a few situations, also, pressure can be thus applied, efficiently, when all tourniquets are plainly inapplicable. 5. Regarding the external incision, in the operation for aneurism, M. Lisfranc wishes to carry out the principle of reducing everything to an exact scale of inches and lines, a mechanical ingraftment upon operative surgery, of which he seems particularly fond; in fact, according to him, a side-pocket containing a foot rule of measure ought to be as indispensable to a surgeon as to a carpenter. Now, we are here opposed to this mincing exactitude, and are ready to assert that the superficial incision can scarcely be too free. We have repeatedly witnessed very palpable proofs of the inconvenience and actual danger of too-limited an incision; the operator is cramped for room, has a difficulty in reaching the vessel; having come to its whereabouts, he runs a risk of unnecessarily detaching it from its connexions in order to make certain of its identity, and accordingly performs the operation not only clumsily and tardily, but with a good prospect of secondary hemorrhage all in due time. To avoid such embarrassment, the superficial incision, extending through the skin and subjacent cellular tissue, ought not to be measured by lines or units of lines, but by the eye and experience of the operator; who, if he errs at all at this point, should lean towards the side of unnecessary amplitude. As he advances in his dissection, the limits of the wound should gradually contract; but as they do so, he will often have occasion to congratulate himself on the liberality of his first use of the knife. It makes little difference in the progress of the subsequent treatment; on the contrary, we are inclined to think a free incised wound, such as we recommend, more likely to unite by adhesion, than one which in its form resembles the "punctured," and which by dint of groping, pressure, and laceration of its tract, can also lay some reasonable claim to the designation of "contused and lacerated." 6. In speaking of venous hemorrhage, resulting from accidental wounds of the concomitant veins, during the dissection, M. Lisfranc recommends pressure as the hemostatic; and, failing that, then ligature of the vein at the bleeding point. Now, we can hardly conceive the possibility of an unavoidable necessity for deligation of a vein, and that is all; and we almost shiver when M. Lisfranc talks so coolly of the alternative. The least result from such a procedure is the occurrence of the mildest and most favorable form of inflammation in the venous coats so deligated, occasioning permanent obstruction of the canal. This taking place simultaneously with occlusion of the arterial trunk, so interferes with the circulating functions, as to render invincible gangrene of the whole limb fearfully probable. But a higher grade of phlebitis is more likely to be excited, inducing death, not of a part, but of the whole, and that with appalling rapidity: and yet a surgeon is found to contemplate

such contingencies with apparent unconcern. This is surely a dangerous example in clinical surgery; for, in our opinion, no more important precept can be impressed upon the pupil than high veneration for the venous texture, avoiding interference with it in all his operations as much as possible, and never dreaming of a ligature unless in the extremity where pressure fails, and life must be saved from hemorrhage at all risks, and that will be but seldom. 7. Should the artery prove yellow in colour, thereby intimating an unsound condition of its coats, we are recommended not to open the sheath, but to include this as well as the artery in the noose of the ligature. Against this precept also we protest, believing that be the arterial coats sound or unsound, the most likely mode of obtaining their salutary adhesion is to include them alone, taking especial care at the same time that the passage of the ligature for this purpose shall disturb the cellular connexions as little as possible. 8. In opening the arterial sheath, M. Lisfranc prefers his own finger nails to all other implements. Again we disagree with him; and prefer the scalpel with which we conduct the whole of the dissection, aided by the dissecting forceps. The scalpel cuts, the nails tear; an incised wound is capable of healing by adhesion, a lacerated one cannot, and must suppurate; adhesion is especially desirable in wounds implicating large arteries; suppuration, extensive or cooped up, is imminently dangerous; every means, therefore, should be adopted likely to conduce towards union by the first intention, and tearing is certainly not one of them. 9. The artery having been exposed, a "sonde cannelée" is passed beneath it, and along the groove of this an armed needle is subsequently introduced. This is not only an unnecessary complexity of instruments for the attainment of one simple end, but likewise occasions a detachment of the arterial coats from their cellular connexions to such an extent, as is most likely to prove fatal to adhesion and obliteration, and very favorable to ulceration and hemorrhage. A few cautious touches with the knife's point, on each side of the part destined for deligation, followed by the gentle insinuation of an ordinary aneurism needle, neither too sharp nor too blunt in the point, is surely both more simple and safe. 10. Previous to deligation, should "small nervous filaments or little veins" be found included in the noose, they are to be tied without remorse, according to M. Lisfranc, and no attempt made to disengage them; otherwise we might injure the cellular coat of the artery, thereby encountering the risk of "grave inconveniences." Once more we dissent. It is quite possible to disengage them; in fact to tie the artery and the artery alone, neatly and cleanly, without otherwise injuring any of the coats of that artery. There may be difficulty, when using one or more "sonde cannelées;" there is none, inseparable at all events, with the ordinary aneurism needle. In any case, the "grave inconveniences" of an attempt are not likely to equal those of the inclusion of nerves and veins. 11. Before completing the deligation, before tying the knot, M. Lisfranc toys with the vessel for some time, repeatedly elevating it by the ligature, yet loose, and applying pressure with the fingers, to ascertain the effects on the aneurismal tumours. Much, if not all, of this is unnecessary. An anatomist and surgeon scarcely requires this test to assure him he has obtained the proper vessel; or at all events, but little of it will suffice. So much as is required by M. Lisfranc seems likely to interfere injuriously

with the important arterial connexions, thereby favoring suppuration and ulceration, if not actual sloughing. And we may here mention that in each of the few cases of ligature of important arteries by M. Lisfranc, detailed in this chapter, secondary hemorrhage occurred. 12. M. Lisfranc uses ligatures which are at first flat, but become round when knotted; they always divide the internal and middle coats of the artery. The advantages derived from their being broad at first is not mentioned. He also seems to place some faith in the superiority of animal ligatures, believing in the possibility of their disappearing entirely by absorption after a long period. We are far from possessing so lively a faith on this point. We have seen the noose of a catgut ligature emerge from a wound in which it had been confined for months, exactly the same to all appearance as when it first entered. Because a wound heals, and ligatures are seen no more, that is no sign that they have been absorbed; they may either have escaped furtively and unnoticed, or they have come to an amicable arrangement with the neighbouring tissues, by them have been kindly provided with an investing cyst of their own, aud have comfortably established themselves accordingly in a permanent residence, as do bullets and other foreign bodies less adapted for such mutual accommodation. 13. But we cannot help suspecting M. Lisfranc, after all, of an involuntary and lurking doubt of the alleged good qualities of animal ligatures; for by and by, we find the following: "I think, along with many practitioners, that the ordinary ligature, one of whose ends is cut off close to the vessel, is preferable to all others." Doubtless. And glad we are to find him discountenancing the absurd practice of leaving a deserted noose, with both its ends cut away, thrown entirely upon its own resources for restoration to the external world, from which it is thus effectually excluded, at least for the time. The only chance of immunity from mischief in such circumstances, is encystment of the foreign body, as already mentioned; but in the great majority of cases, nature sooner or later ejects it by the only means in her power, suppuration; it is, in short, compelled to work its passage out. And often have we seen stumps reported cured, soon come back to treatment with deep-seated abscess, creating no little disturbance both general and local, and not to be quieted until after expulsion of the sole cause, the unabsorbed and undissolved noose. The leaving of one end pendent will not interfere seriously, if at all, with union of the wound; and when that is obtained, it may, under such circumstances, be happily regarded as permanent and secure; moreover, secondary hemorrhage need not be apprehended from deep-seated secondary abscess. 14. The application of two ligatures, with intermediate section of the vessel, as advised by Mr. Abernethy, is recommended by M. Lisfranc only in certain situations, as, for example, when the superficial femoral is deligated. All the advantage likely to result from such procedure appears to us to be obtained by suitable flexion of the limb; and hence we hold that an unnecessary extent of injury is inflicted on the vessel by such mode of operation. Under certain circumstances its use becomes inevitable; when from some cause or other the surgeon has detached the exposed vessel more extensively than is required for the mere passage of the aneurism needle, then by all means let him not neglect to apply a ligature at each extremity of the detached portion; and he may then practise intermediate section of the vessel or not, just as his

fancy may dictate. But in all other cases, the single ligature, well applied, is infinitely preferable. 15. M. Lisfranc fears that sometimes the ligature perforates the artery prematurely, before consolidation of the canal has been completed, consequently entailing hemorrhage; but it requires further proof to fasten the blame of such an accident solely on the ligature; the arterial coats may be at fault, and in the majority of instances of such hemorrhage we are inclined to believe they are so. Consequently we hold M. Lisfranc's censure of the ligature on this score to be not proven, and probably unjust. He, however, countenances none of the "serre nœuds," "ligatures d'attente," and other clumsy means intended to obviate such disaster, and which only realize the old proverb, in most other cases happily almost obsolete, that "the cure is worse than the disease." Temporary ligatures, too, he holds in no estimation; "sound surgery commands that we leave the ligature in its place, until its spontaneous separation." 16. When the arterial coats are obviously diseased, M. Lisfranc prefers compression to ligature, and talks vaguely of plugging the vessel, should hemorrhage threaten or occur. We, on the contrary, think such pressure and manipulation well calculated to ensure the very accident he wishes to avoid, and believe that a well-applied ligature affords the only chance of a successful result, even in such cases. 17. M. Lisfranc well insists on the propriety of avoiding all moral and physical causes likely to accelerate the circulation suddenly and much, at the time when the ligature is about to separate; otherwise the obliterating process may be broken up and hemorrhage ensue. 18. In 180 cases of the Hunterian operation for aneurism, collated by our author, hemorrhage took place in 32; that is, 1 to 6. In the majority, the bleeding occurred between the 16th and 24th days; the earliest hemorrhage appeared at the end of 24 hours, the latest at the end of the second month. The number of deaths was 43, or 1 to 4; other causes than hemorrhage having been at work towards the fatal issue. 19. A case of femoral aneurism is detailed, in which M. Lisfranc tied the external iliac. Two excellent precautions are given, observance of both of which is incumbent on every surgeon: (*a.*) Never to perform deligation of any large artery on account of aneurism, until the patient has been some days in hospital; in order to ascertain whether such residence agrees with him or not. And we would add, in order to afford an opportunity for preliminary treatment, tending to improve the general health, to tranquillize the circulation, and to avert the accidents incident to the operation. (*b.*) The second precaution we quote verbatim. "Notwithstanding my self-confidence, obtained by practice and experience, I have not thought myself warranted in performing so serious an operation without examining anew what I have so often seen and demonstrated. Thus, you will bring clearer and more accurate ideas into the practice of surgery, not imitating the crowd of imprudent operators who put their hand to the work without being sufficiently aware of what they ought to do. Never act thus when you have serious operations to perform, the malexecution of which may cost the lives of those who have confided in you." Such sentiments coming from a French surgeon are delightful, for they lead us to hope that the time has gone by when human life was by them held of but little account. 20. In exposing the external iliac, M. Lisfranc adopts a line of incision intermediate between those of Abernethy

and Cooper. The outer extremity is situated two lines above and an inch within the anterior superior spinous process of the ilium; the inner is placed an inch and a third without the spine of the pubes, and about an inch above its level. Thus he avoids the perpendicular section of the fibres of the abdominal muscles, which Abernethy's mode entails, and which by weakening the abdominal parieties disposes to hernia. His incision is much less extensive than that of Cooper, and consequently is not so likely to injure the spermatic cord, or the epigastric and anterior iliac arteries. The preferable point of deligation is about midway between the origins of the internal iliac and epigastric arteries, thus escaping the disadvantage of placing the ligature immediately beneath a collateral branch. 21. Of the mode in which he proceeds to dress the wound we cannot approve, for the reason formerly stated; first a compress well larded, then charpie, and lastly a poultice; all at once! and this for a wound in which it is of much importance to obtain union by the first intention. No wonder that in the subsequent history of the case we hear a copious suppuration spoken of as having threatened to "run away with" almost the whole of the external oblique muscle. 22. To another procedure we also demur. Immediately after the operation "the limb is enveloped in heated linen cloths, which are renewed from time to time." What more likely cause of gangrene is there, after ligature of the principal vessel, than stimulation of the limb by heat or friction? By the operation the vital powers of the whole parts beneath the point of deligation are materially diminished, and they are not recovered till after a considerable time; and yet here is a surgeon of much experience directing us to stimulate such parts, whereby we do our utmost to bring on an excited action which they have not the power to control, and before which they must consequently fall an almost unresisting prey! "Une potion antispasmodique, de l'eau de gomme, de la limonade edulcorée," will scarcely compensate for so flagrant a breach of sound surgery; the patient happily escaped, however, from the risk he was forced to encounter, gangrene did not take place. 23. On the 46th day after the operation symptoms occurred, alarming in appearance though not in reality; deception against which it is well to be on our guard. The patient, in a state of obvious anxiety, reported that for several hours he had felt strong pulsations in the wound. On exposing it, the abdominal parietes at that point were seen gently heaving; four or five undulations occurred in succession, ceased for an instant, and were then repeated. At first M. Lisfranc felt much alarmed; but the intermittence of these movements, and their want of synchronism with the heart's action, showed they were not arterial; they proved to be muscular, and soon disappeared. 24. M. Lisfranc justly exults in the happy combination which has taken place between medicine and surgery, and by which the latter is enabled to obtain a successful issue in cases far from promising, as was the one now in question; the patient "had pulmonary and cerebral congestions, hæmoptysis, pain, and tympanitis of the abdomen, and yet all these yielded to the medical treatment." But one of the auxiliaries to the art he does not mention, though not the least valuable in ligature of the large arteries; viz. the use of bleeding and other antiphlogistics, prophylactic and actual, both before and after the operation, more especially in the case of the common carotid. On this point the profession is much

indebted to his fellow-countryman, M. Robert; and the subject has been further illustrated by Mr. Miller in the January number of the London and Edinburgh Monthly Journal of Medical Science. 25. A case is related in which ligature of the common carotid was performed on account of a tumour in the side of the face, and which proved fatal by hemorrhage from the wound. With the designation of this tumour we are somewhat wroth. It is termed a fungus hæmatodes. Now, the usual acceptation of that term implies, at least with us, a fungus, a fungoid protrusion, of a bloody appearance, and prone to hemorrhage; further, this is usually found associated with malignant formation, sometimes carcinomatous, sometimes melanoid, but most frequently encephaloid. To speak of a fungus hæmotodes, then, necessarily conveys the impression that it is not only prominent, bloodlike, hemorrhagic, but also connected with malignant disease. How such a tumour is to be benefited by ligature of the common carotid is a problem of rather difficult solution; and hence, at the outset of the case, we are led to suspect M. Lisfranc of villanous surgery. But the sequel proves favorable to his surgical reputation, though far from laudatory of his proficiency in pathological nomenclature. Here is his own description of the tumour: "It is composed partly of aneurismal vessels, partly of erectile tissue; it has no distinct capsule, and is merely surrounded by the ordinary cellular tissue condensed, from which fibrous prolongations traverse the interior." So it turns out to be an aneurism by anastomosis; and he has been but imitating the more successful practice of Messrs. Travers, Dalrymple, Busk, &c. in similar cases, and has not been tying the carotid for a malignant growth, as he has led us to believe, and as he tells us M. Berard has cruelly asserted. But he has himself to blame for having induced us to suspect his judgment, however temporarily; and ought not to abuse M. Berard, or any one else, for an error of his own making. 26. M. Lisfranc considers that false aneurism is not always formed immediately after wound of the artery. The artery does not necessarily bleed immediately after the infliction of a puncture; for this, covered by a neighbouring muscle, or filled by a clot, may be obstructed; a shifting of the muscular fibre, or a displacement of the coagulum, however, will subsequently bring on the extravasation of blood. The coats of an artery may not have been cut, but severely bruised; they inflame and slough; and on the separation of the slough, blood may escape from the pervious vessel, so as to constitute a false aneurism. Hence it is wrong in surgical authorities to make the broad assertion, unqualified, that false aneurism is the immediate result of injury; and that when swelling takes place some time after its infliction, this is invariably attributable to inflammation. The great rapidity of its formation, along with the absence of the usual signs and characters of inflammatory effusion, declare it to be aneurismal at whatever period it may occur. 27. With our author's pathology of false aneurism we fully concur, but object to the treament of an imaginary case of gunshot injury which follows. A ball has made an ugly and extensive wound in the inside of the arm; the arteries below the wound are pulsating normally; hemorrhage takes place and the ordinary hemostatics fail to arrest it; are we to tie the brachial artery above the wound? M. Lisfranc says No; and prefers energetic stuffing of the wound. We would say Yes; and having tied the vessel as we best could, would apply a *moderate* de-

gree of pressure to the wound, placing unbounded confidence in a successful issue. Whereas, with the use of such severe pressure as our author recommends, we should be afraid of ulceration being induced in and around the injured vessel; consequently, on the loosening or ablation of the compress, we should be nervously apprehensive of hemorrhage; and it is most likely that our fears would be fully realized.

We have now reached the middle of the volume, and, with what we hope may be considered becoming discretion towards our readers, pause awhile from our labours. The celebrity of the author has perhaps led us into a more minute examination than is our wont. We have extracted not only what is original, but also what seemed likely to prove both interesting and useful to the majority of our surgical readers; and from that regard to the latter which our position naturally engenders, we have not permitted either our veneration for a justly great name, or the possibility of an imputation of presumption against ourselves, to deter us from freely animadverting on what we humbly conceive deserving of censure. In the same spirit we propose to continue our dissection of the remaining portion of the volume in our next Number.

Art. II.

A Treatise on Dislocations and Fractures of the Joints. By Sir Astley Cooper, Bart. F.R.S., Serjeant-surgeon to the King, &c. Edited by Bransby B. Cooper, F.R.S., Surgeon to Guy's Hospital. —*London*, 1842. 8vo, pp. 576.

The announcement of a new edition of one of Sir A. Cooper's greatest works, edited by his nephew Mr. B. Cooper, is calculated to excite high expectations on the part of the profession. The original work is unquestionably the most valuable contribution that has ever been made to the department of surgery of which it treats, and though not all perfect, (what work ever was, or will be so?) it must yet long remain the classical authority respecting these injuries. It would, however, be absurd to deny that many valuable additions have been made to this branch of knowledge since the first publication of this treatise; and indeed it would be a very questionable compliment to the illustrious author to maintain such a proposition. The happiest result of a real addition to the existing stock of knowledge on any subject is, that it almost certainly leads to further improvements in the same direction; an impulse is given to enquiry, and the newly-opened paths of investigation are explored, with the almost certain consequence of placing on record much valuable matter, which would previously have escaped observation, or been neglected as unimportant. We accordingly know that since the publication of Sir A. Cooper's work on Dislocations, surgeons throughout the world have constantly compared the results of their practice with the doctrines laid down in that incomparable book, and the result has been that additional information has been obtained respecting many points of considerable importance. A new edition of this great work was, we therefore think, called for. Such an edition as we would desire to see could not be expected from the hands of the author. He was indeed, *rude donatus*, and might well depute to another the task of pro-

ducing an annotated edition presenting the comparatively slender yet absolutely important corrections and additions rendered requisite by the multiplied experience of numerous observers.

If the present volume was a mere reprint we should not consider it necessary or right to notice it in this review, but we are told (Preface, pp. ix, x,) that much new matter, derived from Sir A. Cooper himself, selected from the editor's own practice, and contributed from various sources, has been added to this edition. But above all we find that this is an annotated edition, being accompanied with notes, some intended to illustrate, others to correct the doctrines contained in the text; and we consequently infer that the editor designs the present volume not as a mere exposition of Sir A. Cooper's experience and opinions, but professes also to exhibit the existing state of knowledge and opinions respecting the various subjects of which it treats. We therefore feel ourselves called on to examine how far the promises thus implied have been fulfilled, and shall at the same time lay before our readers the marrow of the more important new matter that is incorporated in the present edition. We must not be understood as undertaking to notice the entire work in detail; it will be only necessary to advert to a few of the more prominent subjects, from which as a specimen, an opinion may be formed of the manner in which the whole is executed.

We must first, however, briefly advert to one or two points, which we think require at least a passing remark. We are told (Preface, p. xi.) that Mr. B. Cooper, in consequence of having been much occupied in writing the life of Sir A. Cooper, and also in attending to his professional avocations, has deputed to Mr. Druitt the task of "arranging the new matter, and generally preparing this edition for the press." Had Mr. Druitt's co-operation been obtained merely with the view of rendering the editorial comments more copious and perfect, we should be far indeed from finding fault, as we are well aware how valuable would have been that gentleman's assistance in such a task; but we do find fault with the reason assigned by Mr. Cooper for his having thus sought assistance. Sir A. Cooper contrived, amidst professional occupations of perhaps unexampled extent, to produce the work which Mr. B. Cooper cannot find leisure to edit. We trust that in any similar task which he may undertake with respect to any other of the works of his illustrious uncle, he will not allow his time to be occupied, or his attention distracted by any concurrent literary undertaking. We say this the rather because the present volume betrays signs of carelessness, which, though not very important, nor really detracting from the value of the work, are yet blemishes which would have been better avoided, and which a very moderate amount of editorial attention would have sufficed to obviate.

We have also to regret that the editor has not presented us with a strictly accurate transcript of the original text. We are told (Preface, p. x.) that "it has been thought necessary, in some few instances, to condense the original," and this for the sake of admitting the new matter, without swelling the size of the volume beyond a certain limit. We are happy to be enabled to assure our readers that the "condensations" to which we object are on the whole few and unimportant, and can hardly be considered, as impairing the authority of the text. But we must add that they would have been much better altogether avoided. In

some instances passages eminently characteristic of Sir A. Cooper's style and manner have been omitted, as an example of which we need only cite the suppression of the most graphic "instance of mistake" related at p. 2 of the 4to edition. We shall probably, however, have occasion hereafter incidentally to advert to this subject, and need not dwell further on it at present.

Dislocations in general. Important as is the subject of this chapter, it does not on the present occasion call for any very extended remarks; the editorial notes are more numerous on this than on any other chapter in the work, and we think we may fitly refer to some of them as bearing out the observations we have above made, and showing that the occupation of the editor's time by other avocations has led to some inaccuracies and oversights, which would have been avoided had he devoted his undivided and serious attention to his undertaking.

The very first editorial remark in the present volume (note, p. 3) evinces, we think, the haste or carelessness of which we complain. Sir A. Cooper in the text makes the important practical remark, that, in the first moments after dislocation, considerable motion often remains, and the position of the limb is not so determinately fixed as it afterwards becomes: in evidence of this position a clinical fact is as usual immediately adduced, and the author says,

"I have seen a man brought into Guy's Hospital who, but a few minutes before, had the thigh bone dislocated into the foramen ovale, and I was surprised to find, in a case otherwise so well marked, that a great mobility of the bone still existed at the dislocated part; but in less than three hours it became firmly fixed in its new situation by a permanent, or, as it is called, *tonic* contraction of the muscles."

The following is the editor's note on this passage:

"This extent of mobility would not have been present in any other dislocation of the hip-joint, but into the foramen ovale; so that the comparative greater mobility in this luxation forms one of its diagnostic marks."

We must say, that this comment combines the twofold demerit of being calculated to weaken the force of a most important remark contained in the text, and of inculcating a false and most unpractical doctrine respecting dislocation of the femur into the foramen ovale. The truth is, and we are sure that no one is better aware of the fact than Mr. Bransby Cooper, that a very considerable extent of mobility may remain for some time after perhaps any dislocation of an orbicular joint. We have ourselves seen two cases of dislocation of the femur on the dorsum of the ilium in which this phenomenon was remarkably obvious, and in one of these cases the surgeon who first saw the patient entertained doubts as to the real nature of the accident, because of the great extent of motion which the limb admitted of, though the symptoms were otherwise most strikingly marked. But we need not insist further on this point: we should not indeed have adverted to it, were not the comment on the text calculated to weaken, indeed to do away with the force of a precept of great importance, as regards both the diagnosis and treatment of certain dislocations when seen shortly after the receipt of the accident. The second error contained in this annotation is perhaps of less importance, but yet requires correction. A "comparative greater mobility," to borrow the editor's phraseology, does *not* form one of the diagnostic

marks of dislocation into the foramen ovale; and had but a moderate degree of attention been paid to the text whereon the editor commented, this mistake, the result obviously of sheer haste, could not have been committed; for we are told that the limb, at first unusually moveable, "in less than three hours *became firmly fixed in its new situation*." No doubt, in dislocation into the foramen ovale, the limb admits of abduction and flexion to a considerable extent; but the editor cannot mean to imply that Sir A. Cooper was ignorant of this fact; the text of course conveys that the motions of extension and abduction, the motions *usually limited*, in this dislocation were extraordinarily free in the first instance.

At pp. 12-13 is recorded a case in which a dislocation of the femur into the ischiatic notch was reduced with unusual facility, an event at the moment attributed to the relaxation caused by the presence of nausea. The patient died, however, the following day, when the jejunum was found to be ruptured. To this case the editor appends the following note:

"This case exemplifies the necessity of carefully investigating every circumstance connected with every injury before pronouncing our prognosis or proceeding to treatment. It is evident that the facility with which this dislocation was reduced depended on the mortal injury of the intestines."

Surely the editor did not imagine that this note was his own. If we turn to p. 22 we shall find Sir A. Cooper himself saying,

"That the muscles are the chief opponents to reduction is strongly evinced by those cases in which the dislocation is accompanied by injury to any vital organ...... *Thus, in the case already mentioned, of the man who had an injury to his jejunum and a dislocation of his hip, the bone was reduced with little difficulty.*"

And, at p. 33, Sir A. Cooper impresses on us that it now and then happens that the surgeon's attention is so abstractedly directed to the obvious accident, that he overlooks some important concomitant injury. "I need not remark, then, on the necessity of such examinations being invariably made in every case of severe local injury."

In p. 13 we find a "condensation" which leads to a misrepresentation of Sir A. Cooper's opinion respecting the possibility of the occurrence of dislocation of the vertebræ without concomitant fracture. In the 4to edition, p. 14, it is stated that the accidents which have been called dislocations of the spine "are not true dislocations of the spine, *excepting those of the upper cervical vertebræ*." In the present edition the words printed in italics are omitted; why, we cannot pretend to say. It is remarkable, however, that, coupled with this omission, we have an editorial note, informing us that dislocation of the vertebræ may occur. The fact is, that Sir A. Cooper never denied the possibility of luxation of the cervical vertebræ, as may be further seen by referring to p. 500 of the 4to edition, or p. 526 of the present edition. He merely says that he has never witnessed such an accident. The note in question is, however, a very proper addition to the text; and we have only to remark, that it would have been still more so had it supplied the deficiency of observed cases in the original by a fuller reference to the experience of other surgeons. Thus, we are told that "Boyer has related cases in which dislocation of the atlas from the vertebra dentata has occurred, and death was the immediate result." No doubt he has; but the experience of J. L. Petit and other eminent surgeons who have observed

the same accident should have been alluded to, and the history of other luxations of the cervical vertebræ should, we submit, have been given in a more extended and satisfactory form. We were particularly surprised to find that no allusion is made in this note to the remarkable case of dislocation of the second cervical vertebra—the most remarkable case on record of this class of injuries, detailed in the Gaz. Méd. de Paris, 1840, No. xlii. p. 657, et seq., and which was reduced after the lapse of seven months by M. Jules Guerin. It is true that the nature of the accident may be questioned, as there was *no dissection;* but as MM. Marjolin, Bouvier, Sanson, and Guerin all agreed that the case was one of dislocation of the second cervical vertebra on the third, it was surely worthy of being mentioned in a note which professes to supply a lacuna in the MAGNUM OPUS on this class of injuries.

We have hitherto noticed editorial comments which we conceive to be uncalled for or incomplete; and we have now to advert to the absence of comment where we conceive it might have been useful. At p. 17 Sir A. Cooper says, "I have read of dislocations of the hip in children, but their history is that of diseases of the hip-joint, in which the dislocation has arisen from ulceration;" but yet at p. 37 is recorded the case of a child aged seven, admitted to Guy's Hospital labouring under traumatic dislocation of the femur on the dorsum of the ilium; and here, by the way, we must again complain of a suppression which injures the authority of the text; this case is we believe unique, at least we are not aware of any example being recorded of a similar accident having occurred in a child of such tender age; a reference to the present edition would lead to the supposition that this case had occurred in Sir A. Cooper's own practice, and was verified by his own personal observation; and yet we find, on reference to the 4to edition, p. 50, that Sir A. Cooper *did not see the case*, that it was communicated to him by "Mr. Daniel, one of Mr. Lucas's dressers," who "relieved the limb in the presence of many of the students." We do not for a moment mean to insinuate any suspicion as to the authenticity of the case, but we do maintain that this is not the way in which the materials for the history of surgery should be written. We have no note from the editor on this important matter. The few analogous cases of dislocation of the femur in early life should, we take it, have been appended in a note; at least the recent case recorded by Mr. Norris, of the Pennsylvania Hospital, of dislocation of the femur on the dorsum of the ilium, in a boy aged eleven, might, we presume, without any very extended research, have fallen under the notice of the editor.

Previously to the publication of Sir A. Cooper's Treatise on Dislocations, in the event of a bone being both dislocated and fractured, the rule of practice was, in accordance with the recommendation of J. L. Petit, to defer any attempt at reducing the dislocation till the fracture was consolidated; reduction consequently was very rarely accomplished in these cases. Sir A. Cooper, on the contrary, inculcated the propriety of endeavouring to reduce the dislocation without delay, provided the fracture occurred at a sufficient distance from the displaced extremity of the bone. This is perhaps the only practical rule in Sir A. Cooper's entire work which is not based on and illustrated by clinical observation; it is indeed not a little remarkable that not a single case is recorded by Sir A. Cooper of dislocation and fracture of the same bone in which the practice

he recommends was adopted; but two cases of fracture with dislocation of the femur are mentioned. In one (p. 49) the fracture was allowed to unite, and in five weeks Mr. Badley reduced the dislocation. In the second case (p. 50) the dislocation remained unreduced. But one case of fracture and dislocation of the humerus is mentioned (p. 382), the fracture having united, a fruitless attempt at reduction was made six weeks after the accident. The value of the rule of practice inculcated by Sir A. Cooper has been nevertheless amply established, and we think the editor should, when sending an annotated edition into the world, have cited some of the cases which exemplify the sagacity of the illustrious author in recommending a practice which, though he seems never to have had an opportunity of adopting it himself, has yet been attended with the happiest results in the hands of others who no doubt acted on Sir A. Cooper's authority. Thus Mr. Bloxam (Med. Gaz., 1833, vol. xii., p. 702,) records a case of dislocation on the pubes with fracture of the femur reduced on the eighth day. The most remarkable case of the kind with which we are acquainted occurred in the practice of M. Etene, (Gaz. Méd. de Paris, 1838, p. 751:) A man was struck down by the fall of a tree; the left femur was dislocated and fractured about its centre, there was a penetrating wound of the left knee-joint, and a compound fracture of the left fibula, with dislocation of the corresponding ankle; Sir A. Cooper's practice was adopted in this case, the dislocation was readily reduced, and the man ultimately did well.

Sir A. Cooper's opinion as to the period within which reduction of dislocations may be safely attempted is of course generally known: he considers "that three months for the shoulder, and eight weeks for the hip, may be fixed as the period at which it would be imprudent to make the attempt at reduction, except in persons of extremely relaxed fibre, or of advanced age." (p. 28.) On this passage, the editor has, in our opinion, a very judicious note. He asks whether there is not a better criterion to determine the point in question than mere length of time:

"Should not the principal consideration be the precise condition of the new joint, especially as to the degree of motion of which it is capable, for by this a fair judgment may be formed as to what extent nature has altered the surfaces of the bones in contact to fit them for the functions of a joint in their new situation. If any useful motion can be performed, then I believe it may be considered as ill judged to attempt to restore the dislocated bone to its former articulating cavity, for it seems invariably to happen that as a new joint becomes fitted for use, so the structures of the old one are rendered incompetent to restoration."

These changes, the editor conceives, depend less on time than on the condition of the limb as to rest or motion: if the limb has been kept motionless, the changes occur very slowly; if continued efforts have been made to use the limb, they are apt to occur more rapidly; and once any useful degree of motion has been acquired, the editor deprecates any attempt at reduction.

When we consider that some dislocations—say of the femur—have been reduced after four, six, eight or even eighteen months, while, on the other hand, in most cases reduction is impossible after the lapse of a few weeks, it is difficult to avoid the conclusion that some difference must exist in the pathological condition of the parts in two classes

of cases differing so widely in their results. We think it extremely probable that Mr. B. Cooper has the merit of suggesting at least one of these differences, and he has also probably indicated the signs which will aid our practice, by assisting us to diagnosticate the condition of the parts. Should experience confirm his opinion, he will have made a very valuable contribution towards the solution of a most important difficulty in one of the great questions of surgery. The opinion of M. Malgaigne on this point being, to a certain extent, borne out by pathological anatomy, is worthy of being placed in juxtaposition with Mr. B. Cooper's explanation of this important and difficult question. He considers that whenever the capsular ligament is torn to but a moderate extent it still continues to secrete synovia, which keeps open the passage of communication between the articular cavity and the head of the bone in its new situation, which will therefore readily regain its natural position by traversing the passage thus kept open by the synovia, if the adhesions it has formed be once broken up. When, however, the capsule of the joint is very extensively lacerated the articular cavity becomes filled up, and reduction is impossible. We have as yet, however, got but little precise and positive knowledge on this important subject.

The case in which Dieffenbach effected the reduction of a dislocation of the shoulder, of two years' standing, by "the subcutaneous section of everything that resisted him," is added to the text of the present edition, (p. 29) without any comment or opinion as to the value or propriety of the practice.

Dislocations of the hip-joint. The anatomical description of the hip-joint (p. 34 *et seq.*,) appears to have been revised by the editor, and yet it requires still further correction on one or two points which have a most important bearing on the history of dislocations of the femur. No description is given of the acetabulum in the dry state. We are told that in the recent subject it "is much deepened by a ridge of fibro-cartilage, which surrounds its brim, and which fills up the notch on the inner side. Now the acetabulum presents three distinct notches or depressions of great importance in a surgical point of view, as it is the existence of these notches that determines the direction of the displacement of the femur in all ordinary cases, and explains why it is that the head of the bone is always found (save in very rare and exceptional cases) in one of four (or regarding the more ordinary cases, only in one of three) determinate and constant situations. Sir A. Cooper (p. 100) considers this remarkable fact "as the natural result of the influence of the muscles which drew the bone into these positions." But an attentive examination must convince every one, we conceive, that the configuration of the brim of the acetabulum is at least the chief cause of the phenomenon in question. The brim of the acetabulum is in point of fact very prominent, superiorly and inferiorly, in which directions displacement of the head of the femur is consequently exceedingly difficult to be effected. At about the union of the upper and middle thirds of its *anterior* border (we use this as a more proper phrase than *inner* border, as the acetabulum looks chiefly outwards, and but very slightly downwards and forwards,) is another elevation corresponding to the ilio-pectineal prominence above, and especially below which the brim of the cavity is to a certain extent deficient, and thus we have two distinct *anterior notches*,

through either of which the head of the femur may escape. If it escapes through the upper of these depressions *dislocation on the pubis* is the result; if through the lower one, dislocation into the *thyroid foramen* ensues. The *posterior notch* is the largest; it occupies almost the entire length of the *posterior margin* of the acetabulum, and its depth is perhaps one third of the entire depth of this articular cavity. Hence *one reason* of the facility and frequency of the occurrence of dislocation backwards. No doubt in the recent state, the cotyloid ligament fills up these depressions, but it is scarcely necessary to say that it cannot resist displacement as effectually as a bony ridge would. Sir A. Cooper (p. 100) says,

"There seems something a little anomalous in the frequency of the dislocation upwards and backwards upon the dorsum of the ilium; for upon examination of the hip-joint it will be found that it is the direction of all others in which there seems to be the greatest protection against dislocation, the capsular ligament being there the strongest, the edge of the acetabulum the most elevated, and the ligamentum teres offering the greatest hinderance to displacement in that direction. There is comparatively a much greater apparent facility to the dislocation into the foramen ovale, for the under and inner part of the acetabulum is partly formed of fibro-cartilage; the capsular ligament is much thinner here than at the upper part; and the ligamentum teres does not offer the same resistance to the displacement in this direction that it does to that over the upper part of the cotyloid cavity."

It is to be observed, that Sir A. Cooper seems to have applied his mind but little to the *mechanism* of dislocations. In the 4to edition there is, we believe we may say, not a syllable on this subject; the chapter from which we have made our last extract not appearing in the earlier editions. The difficulty here started is easily explained, and all the common dislocations of the hip can be not only accounted for, but it can be shown that they are the *only* dislocations which *ought* (if we may so speak) to happen with facility. We cannot, of course, enter into this discussion at length; suffice it to say, that the explanation chiefly flows from the anatomical considerations already detailed, coupled with the fact, that two of the most extended motions of the thigh are precisely those which approximate the head of the femur to the depressions that exist in the brim of the acetabulum. If the most frequent displacement of the femur was *upwards* its occurrence would be indeed anomalous, but the direction of the dislocation is upwards *and backwards.* We leave it to our readers to judge, whether the discussion at which we have here glanced did not come fairly within the scope of the editor's duty.

Dislocation of the hip-joint on the dorsum ilii. In the original 4to edition we find the following passage:

"In the dislocation upwards, the pyriformis and the glutei muscles are all shortened, as are also the triceps and pectineus, the psoas magnus and iliacus internus, the rectus, the semitendinosus, and semimembranosus, and one head of the biceps. The obturator externus is shortened; but the obturator internus, gemini, and quadratus are put upon the stretch."

It is very remarkable that in all the subsequent editions this passage has been expunged, without the true pathological anatomy of the accident being substituted in its stead. This is a deficiency which we certainly think the editor should have supplied.

The following valuable case of fracture of the acetabulum is inserted in the present edition (pp. 57-8) amongst the cases of dislocation on the

dorsum of the ilium, and perhaps properly so, as the two accidents resemble each other very much.

A strong, young, muscular man, aged twenty-seven, was admitted into Guy's Hospital, September 2d, 1839, under Mr. Key. Shortly before admission he had been struck by a three ton weight on the back of the right leg and thigh in such a manner as to force the right knee in advance of and across the other leg.

"When the patient lay on his back the right leg appeared shortened two inches; the knee in advance of the other; the foot inverted, and lying over but not in contact with the dorsum of the left foot; the trochanter was unusually prominent, and nearer the anterior superior spinous process of the ilium than natural; and the anterior inferior spinous process could distinctly be felt. The patient had considerable motion of the hip, and could in some degree bend the knee, but could not evert it. After some exertion he could, when standing, bring the heel of the right foot within an inch of the ground, the toes remaining inverted. By employing gentle extension the limb could be elongated to its proper dimensions, and the prominence of the trochanter was considerably diminished, but returned as soon as the extension was discontinued. On rotating the femur with one hand applied to the knee, a very distinct crepitus could be felt at the hip. The crepitus could be felt only by the hand applied to the knee, not being discovered when the hand was placed on the hip. The trochanter on rotation moved with the femur, which could be bent and inverted, but could not be so much everted as to turn the toes out. The head of the femur could not be felt on the dorsum ilii."

Mr. Morgan attempted reduction with the heel against the perineum. On making very moderate extension the head of the bone returned into the acetabulum "the return was accompanied with a jerking, grating motion not with the ordinary snap," and as soon as extension was discontinued the head of the bone slipped from the acetabulum. He was placed with both legs extended and bandaged together, but the hip becoming painful, hot, and swollen, was leeched and the limb was placed on a double inclined plane. It is an extraordinary instance of carelessness or of oversight that the termination of this interesting case is not given. The last statement respecting it is that on the 25th September, 1839. "He still continues on the plane; there is slight shortening and inversion of the foot; he can evert the foot, but not the knee; there is no pain in the hip, and the joint has its natural shape." This case occurred in the hospital of which the editor is himself one of the surgeons. He might have learned, one would think, the result of the case for the trouble of asking, and yet he leaves us in ignorance as to its issue.

At pp. 51-2 the editor inserts two cases of dislocation on the dorsum ilii, communicated to him by Mr. Elliott, stating that "they show that an able surgeon may succeed, even with inferior means, if he has the right principles to guide him." The ability exemplified in these cases simply consisted in reducing the dislocations without pullies, and by the aid of a sufficient number of vigorous soldiers. Surely Mr. Cooper ought to know that hundreds of dislocated hips have been reduced by the hand; and that this has for many years been the mode adopted, by preference, on the continent.

Dislocation into the foramen ovale. With reference to the treatment of this accident, Sir A. Cooper, in the 4to edition, (pp. 37-8,) objects to the mode of reduction employed by Mr. Hey, but most candidly adds,

"I am not sure that, in all respects, I understand the description of the method which he adopted." The entire passage is omitted in the present edition; and we must say, that, in our opinion, the editors should have restored it to the text. The materials for the history of surgery should all be preserved in the most authentic form. The very passage, for example, just adverted to is of great importance when we come to *weigh authorities* respecting the most eligible mode of reducing dislocations of the femur—a point far indeed from being yet definitely settled.

Dislocation into the ischiatic notch. With respect to the diagnosis of dislocation into the ischiatic notch, the editor adds the following note:

"I have heard it a matter of discussion between equally competent surgeons respecting a case of dislocation, whether the head of the bone was placed upon the dorsum ilii or in the ischiatic notch; the dispute arising as to the extent of the shortening of the limb, which, in my opinion, does not form a very perfect diagnostic mark; for the limb becomes invariably shorter as the space of time increases since the accident has occurred, so that it may vary from half an inch at first to a subsequent shortening of three inches. The best sign of this accident, I believe, is the greater displacement of the head of the thigh-bone backwards, and the consequent obliquity of the shaft, so as to direct the knee across the middle of the opposite thigh, just above the patella." (p. 72.)

We must certainly object to the statement, that the head of the femur, if in the ischiatic notch, can be drawn upwards two inches and a half and still continue in that space. No doubt, the extent to which the limb may be elongated varies considerably, and we do not deny that the tonic contraction of the muscles exerts some little influence in this respect. There is, however, an anatomical circumstance which explains the variability of this symptom in different cases. The relative height, as Malgaigne observes, of the cotyloid cavity and of the ischiatic notch must evidently vary with the angle of inclination of the pelvis; but this angle does vary, both in the two sexes and in different individuals of the same sex. We have not space, nor is it indeed our business, to follow out these considerations, which evidently have a very important bearing on the symptomatology of this dislocation.

Dislocation on the pubes. Two of the cases recorded in this chapter are interesting because of the unusual situation of the head of the femur; which in one case, (pp. 89-90,) "was placed upon the femoral artery so as to stop the pulsation." While in the other case, "it was placed to the inner side of the femoral artery."

Anomalous dislocations of the hip-joint. In the earlier editions, Sir A. Cooper, when treating of *varieties of dislocations* of the hip-joint, admitted of but four dislocations of the femur; those in fact which we have just so briefly passed in review. The only other dislocation which he even mentions is that "downwards and backwards," respecting which he speaks somewhat vaguely, but on the whole seems to deny the possibility of its occurrence. In the quarto edition, (p. 33,) he does not absolutely negative its possibility, "yet," he says "I am disposed to believe that some mistake has arisen upon this subject." Again, (4to, p. 72,) he says "I cannot help thinking that some anatomical error must have given rise to this opinion, &c.;" and finally, (4to, p. 76,) we read, "it is to be remembered, that there is no such accident as a dislocation of the hip downwards and backwards." In the present edition the three passages just quoted are expunged, and the first of the foregoing citations is

replaced, (p. 36,) by the following sentence: "Besides these four, two other anomalous dislocations have been described, of which we shall speak in the proper place." Sir A. Cooper's opinions, therefore, on this subject underwent considerable change, a circumstance, however, which no reader could even suspect from the perusal of the present edition.

In the present chapter (on anomalous dislocations, p. 100 et seq.) Sir A. Cooper admits the possibility, and describes cases of dislocation of the femur upwards and forwards—upwards and downwards. He considers such accidents, however, as anomalous, and as only capable of occurring when the dislocation is complicated with fracture, or when collapse from any cause is present, when the sheer physical force which produces the accident may place the bone in some unusual position, which it maintains either from a portion of the broken bone resisting its being acted on by the muscles, or from the muscles themselves having lost all power. This opinion, we need scarcely remark, is very questionable, and is far from being generally assented to. We would have been glad had the editor discussed this interesting point, sufficient materials for the consideration of which would have been afforded by the cases detailed in the present volume, together with those which are recorded elsewhere. The editor should also, we think, have at least noticed the other observed varieties of these "anomalous" dislocations not mentioned in the text. We should suppose that the case of dislocation on the spine of the ischium observed by Earle, for example, (Lancet, vol. xi. p. 159, 1827,) must have been within his knowledge.

The dislocations recorded in this chapter being of extremely rare occurrence, we shall abstract two of the number.

Dislocation of the hip upwards. A man was admitted into Guy's hospital, having about an hour previously fallen backwards while helping to carry a heavy crate down stairs. He fell backwards, receiving the weight on the groin. When he lay extended on his back,

"The left leg was shortened at least two inches, and the foot excessively everted, so as almost to give the toes a direction backwards. The injured limb had a tendency to cross the sound one, so as to throw the heel of the former over the instep of the latter; nevertheless, when they were placed side by side they remained in that position. The leg was susceptible of all the natural motions to some extent, with the exception of rotation. The projection of the trochanter major was entirely lost, while the luxated head of the bone could be felt under Poupart's ligament, just below and to the inner side of the anterior inferior spinous process of the ilium, and the junction of that bone with the pubis. It thus rested upon the brim of the pelvis, and projected towards the abdomen. The femoral artery was not displaced, but could be traced on the inner side of the bone."

The mode of reduction adopted is worthy of notice. Extension was made downwards from the knee by means of a towel; while Mr. Morgan fixed the pelvis by sitting on the bed and placing his foot between the scrotum and the thigh. Three or four students made steady extension for about three minutes; the patient was then directed to raise his shoulders from the bed, and the thigh was forcibly rotated inwards, the power of a long lever being obtained by bending the knee at right angles to the thigh, and grasping the knee in one hand and the foot in the other. The head of the bone returned immediately with a snap into its socket. (pp. 103-5.)

Dislocation downwards. Mr. Keate was called to see a gentleman whose horse had fallen with him and upon him into a deep and narrow ditch. The limb was at least three or three and a half inches longer than the opposite one. The thigh was much flexed upon the pelvis, the leg as much bent upon the thigh. The whole limb was carried outwards or apart from the other to a greater extent than Mr. Keate had ever observed in a case of luxation. The head of the femur was inferior to the ischiatic notch, and was found lying close to, and on a level with, the tuberosity of the ischium. In the first attempt at reduction the head of the femur was thrown into the foramen ovale; a second extension enabled Mr. Keate to place it *nearly* in its proper position in the acetabulum, but it could not be perfectly replaced; and on gently moving the bone a distinct grating as if of ruptured cartilage was heard. By drawing the upper part of the femur outwards, and pressing the knee sharply inwards, the head of the bone was replaced with a snap in the acetabulum. But even then, Mr. Keate says, "I was enabled to elongate or pull down the limb, and it was evident to me that this was owing to a portion of the cartilaginous labrum having been broken off."

Mr. Cooper has not taken any notice of the great difference of opinion that at present exists respecting the general principles that should be adopted in the reduction of dislocations of the hip. Many practitioners are of opinion that in most if not in all displacements of the femur, the thigh should be flexed on the pelvis, and the leg flexed upon the thigh; and independently of the cases recorded by the older practitioners, as Pouteau, Pott, Hey, &c. numerous examples are being constantly published in which these accidents have been reduced with the greatest facility by adopting this plan. Several of the cases Mr. Cooper has edited illustrate the—at least occasional—success of this old and recently revived method, (e. g. case lxiii. p. 96-7;) and would have afforded opportunity for interesting and valuable comment.

Our observations on the remainder of the present volume must be rapid and general. The chapter "on Fractures of the Pelvis" is newly arranged, contains some new matter of interest, and its practical value is decidedly increased; it calls, however, for no particular remark on the present occasion. The chapter "on Fractures of the upper part of the Thigh-bone" has also gained much in clearness and precision from the rearrangement of the matter, and we are happy to be enabled to add from the editorial comments. Almost every point requiring illustration has been noticed by the editor; and, for the most part at least, in a satisfactory manner. At p. 131 the editor in a note alludes to that puzzling phenomenon the inversion of the limb that occasionally occurs in fracture of the neck of the thigh-bone completely internal to the capsular ligament. We could perhaps have wished his observations on the subject to have been somewhat fuller; but, after all, the note possibly is a fair summary of the state of our knowledge or rather ignorance on the matter. As to the symptoms of this accident the editor (p. 133,) properly adds to those detailed in the text the following note:

"Another strong diagnostic mark of this accident may be ascertained by desiring an assistant to make extension of both limbs and simultaneously rotate them, when the surgeon, by placing his hands upon the trochanters, will perceive that they move in the arcs of different circles, that on the injured rolling

on its own axis, while the healthy trochanter describes an arch of which the neck forms the radius." The editor adds, "and further it will be found that the patient cannot raise the whole limb from the bed, in consequence of the thigh-bone having lost its *point d'appui* in the acetabulum."

This latter observation is no doubt true in the immense majority of cases, but the editor should have warned his junior brethren that in some rare instances a patient labouring under this accident can not only raise the limb from the bed but even walk for some time after the receipt of the injury. As to the age at which fracture of the cervix femoris within the capsular ligament is liable to occur, Sir A. Cooper, as is well known, had in all his immense experience "*only known two cases occur under fifty years of age.*" The editor (p. 135,) adds to these "Mr. Stanley's case, in a boy aged eighteen, reported in the Med. Chir. Trans. vol. xviii."

We cannot be expected to enter into the questio vexata of bony union of fractures of the neck of the thigh-bone within the capsular ligament; we believe that Sir A. Cooper's views, long misunderstood or misrepresented, are now assented to by the great majority of surgeons. At pp. 144 and 155 we have two notes from the editor on the subject, the one being pretty nearly a repetition of the other. In the latter the editor says, "I am quite of Sir A. Cooper's opinion, that fractures of the neck of the thigh-bone within the capsular ligament do not, excepting under peculiar circumstances, unite by bone," &c.: and he further says, "I maintain that there are several circumstances tending to prevent the ossific union of a fracture of this part of the thigh-bone," &c. Of the several circumstances thus "maintained" by the editor the only one of any importance not directly mentioned by Sir A. Cooper, is the peculiar change sustained by the cervix femoris in advanced life; and which, as it renders it more fragile, may, Mr. Cooper argues, be fairly considered as rendering it, when broken, incompetent to set up restorative action. This reason for non-union has been long since assigned by Velpeau and others (atrophie senile—imbibition huileuse), to whom, however, the editor makes no reference.

We pass over the chapter "on Dislocations of the Knee." In the chapter "on Dislocations of the Head of the Fibula," the editor gives a case of this exceedingly rare accident. Sir A. Cooper had already recorded one case, but it was connected with compound fracture of the tibia. In Sanson's case it could be scarcely said that dislocation existed, the ligaments of the articulation were ruptured, and the head of the bone could be readily displaced by pressure, but when left to itself it resumed its natural position. Mr. Cooper's case is, however, an unexceptionable example of dislocation of the upper extremity of the fibula from violence. The accident occurred from a waggon passing over both limbs.

"The left fibula, when traced upwards, seemed to pass more backwards than natural. It was also more moveable than usual, and the upper part of the bone was felt in the popliteal space. There was little deformity visible, but the projection of the outer head of the soleus muscle was not quite so distinct as in the other leg..........There was scarcely any pain given by motion of the limb, and the man possessed the power of extending it perfectly and of flexing it to a considerable extent.... ....The dislocation was reduced by bending the leg to relax the biceps muscle, and then grasping the head of the fibula and pressing it into its natural situation."

Some difficulty was experienced in maintaining the bone in situ, to effect which object a tourniquet pad was applied over it for nearly two months, at the "end of which time the fibula remained rather behind its usual situation, but was perfectly fixed."

In the chapter "on fractures of the patella," we find two new cases (one from the editor's practice) of compound fracture of the patella terminating favorably. And the editor has also inserted from the fifth vol. of Guy's Hospital Reports, Mr. Ward's most remarkable case of recovery from a gun-shot which carried away nearly the entire of the patella.

The chapter "on dislocations and fractures of the ankle-joint," admirable as it is, need not detain us on the present occasion. It is true that it is improved in form and general arrangement, and is enriched with some new and valuable cases, but still it would not be easy to specify the improvements within any moderate space, nor would it perhaps be very useful to do so; as it does not in point of fact *essentially* differ from the corresponding chapter in former editions. This chapter has called forth scarcely a remark from the editor. We shall only observe that the author, in simple dislocation of the tibia inwards (p. 253,) and also in fracture of the fibula near the ankle-joint (p. 317,) recommends almost exclusively that the leg should be placed on the outer side in a semi-flexed position. Dupuytren's mode of treating these cases is so very obscurely and imperfectly described, that we suspect few if any practitioners would be able thence to apply his method, even with the aid of the diagram, (p. 256.) The editor might, we conceive, have advantageously appended a note on this subject, and at the same time have given an account of the mechanism of these injuries as described by Dupuytren.

We pass over "dislocations of the foot," and "dislocations of the jaw." As regards "dislocations of the clavicle," we have only to remark that the editor might have noticed M. Pellieux's case of dislocation of the sternal extremity of the clavicle backwards from violence, the accident having arisen from the patient falling under his horse. (Revue Médicale, 1834, p. 161.) The case of this affection recorded by Sir A. Cooper, long the only one known, was after all scarcely an example of dislocation in the common sense of the phrase, as it was the result of disease.

The chapter on "dislocations of the shoulder" shall not detain us long, both because of want of space and because the editor's annotations on it are very few in number and very unimportant. We may observe that, at p. 371, a case is given in which Dr. Gibson, of Pennsylvania, in endeavouring to reduce a dislocation of the shoulder, of two months' standing, effected indeed the reduction, but at the expense of tearing across the axillary artery, which caused the death of the patient. A reference to Dr. Gibson's work of surgery would have shown the editor that this lamentable accident occurred *three* times in the practice of that gentleman; who has, with the most praiseworthy candour, published the cases in detail as a lesson and warning to his professional brethren. At p. 379, the editor describes, and illustrates by an admirable engraving, Mr. White's method of reducing dislocation of the humerus, a method recently revived by M. Malgaigne, and in some instances with remarkable success. In the section "partial dislocations of the shoulder-joint," the editor has incorporated Mr. Soden's observations on dislocation of the biceps tendon from its groove.

We pass on to "Dislocations of the Radius," and cannot but regard it as singular that the editor makes no observation on the remarkable fact that Sir A. Cooper's experience respecting these dislocations differed so widely from that of every other surgeon who has written on the subject. Sir A. Cooper met with seven cases of dislocation of the head of the radius forwards; while he never saw dislocation of the radius backwards in the living subject. Setting Sir A. Cooper's experience aside, the number of recorded cases of dislocation of the radius forwards are extremely few, while we have at hand upwards of twenty references to dislocation of the radius forwards. Two of these observed by Professor Langenbeck are incorporated with the text. (p. 460.)

We have already almost exceeded our limits, but we have fully accomplished the object we had in view, which was not to give anything like a detailed analysis of the present volume, but to enable our readers to judge of its value as compared with the former editions of the same work. In so doing it will be perceived that we have not scrupled to express our opinions freely and to find fault where we considered there was cause for so doing. It is to be observed, however, that any strictures we have made apply rather to faults of omission than of commission. Had the editor confined himself to merely producing the text of his great original, our duty would have been to deal out unmixed praise and thank him for an edition decidedly superior to any of its predecessors; but he made his option to publish an annotated edition, and in so doing we think he judged most wisely. The distinguishing merit of the original work is its eminently practical character; and one of its most distinctive peculiarities is the almost complete absence of reference to books or to the experience of others, always excepting such cases as were originally and directly communicated to the author himself. This great work, however immense its value, was still but the record of the experience of one man and of a few individuals of *his school;* and it is therefore obvious that its merit was susceptible of being augmented in no trifling degree by judicious comments and additions from the experience of others, who had been set in the track of profitable observation by the impulse communicated by its admirable contents. Mr. Cooper, then, as we think, most judiciously chose the part of publishing an edition with notes, and consequently professes to criticise and comment on the text—to correct erroneous doctrines—to supply defects—to make the work, in a word, represent the existing state of knowledge respecting the subjects of which it treats. Such being the case, we thought it right to examine the performance not simply with reference to what the editor has done, but also with a view to what he might and, as we think, should have done.

If, however, we look at the present edition merely as it is—if we take it as it is given to us, we feel bound to say, that it is a most valuable and important addition to surgical literature. In it we find the last, the most matured views of its venerable author, who, with unexampled zeal, continued to almost the last moment of his life to accumulate materials for perfecting his works. Every practical surgeon must add the present volume to his library. Its commodious and portable form—no mean consideration—the graphic, the almost *speaking* force of the unequalled illustrations, the copious addition of valuable and instructive cases, and the great improvement in clearness and precision which has been gained

by the judicious arrangement of the materials, all combine to render the present edition indispensable. The woodcuts profusely scattered through almost every page are beyond praise: nothing can exceed their faithful accuracy and clearness. Truly is it remarked by the editor that "the advantages of such engravings being placed in immediate connexion with the portion of the text which they are intended to elucidate, will not pass unnoticed by those who have felt the inconvenience of having to search at the end of the volume to which reference occurs in the text;" and the benefit of such an arrangement is vastly enhanced when the illustrations are of such exquisite beauty as those of Mr. Bagg.

Art. III.

Traité des Fièvres Intermittents, Remittents, et Continués, des Pays Chauds, et des Contrées Marécageuses, suivi de Recherches sur l'Emploi Thérapeutique des Préparations Arsénicales. Par J. C. M. Boudin. —*Paris*, 1842. 8vo, pp. 336.

Treatise on Intermittent, Remittent, and Continued Fevers of Hot Climates and Marshy Countries, followed by Researches on the Therapeutic Employment of Arsenical Preparations. By J. C. M. Boudin. —*Paris*, 1842.

M. Boudin, in his capacity of a physician of rank in the French army, has, as he informs us, enjoyed many opportunities of observing the effects of marsh miasmata in several parts of France, Germany, Spain, Greece, and the Algerine districts of Africa. A summary statement of the fruits of his study in those countries is all that his volume professes to give, for M. Boudin condemns the custom into which his contemporaries have fallen of augmenting the size of their publications by loading them with numerous details. He makes a better apology for the omission, which will be deemed serious by many, when he hints at the long and painful peregrinations during which his notes were made, than when he professes to consider the narration of cases, and the record of particulars, unnecessary and inconvenient, because they afford no better security against dishonest sophistication, while they consume the time and patience of the reader. As this opinion is not confined to M. Boudin, we may remark, in passing, that detailed reports and observations are to be regarded less as tests of an author's probity, than of his means and methods of investigation, and of his capacity to observe and reason correctly, for it is to be hoped that intentional misrepresentation does not enter, in any considerable degree, into the sources of error in medicine.

M. Boudin's first chapter contains his definition of marsh poisoning (l'intoxication des marais), or limnhemic affections, (affections limnhémiques, from λιμνη, marsh, and αιμα, blood;) under which term he comprehends all maladies endemic in marshy countries, and described by authors under the names of intermittent, remittent, pseudo-continued, masked, and pernicious fevers, &c., a system of nomenclature which he conceives to have thrown a very great obstacle in the way of a correct understanding of the nature of the diseases of which he treats. Allowing the old designations to which he objects to be imperfect, we cannot see

in them the amount of evil which he deplores, since the various disorders which they signify have been amply acknowledged as traceable to a common cause. Nor is it an easy matter to perceive how the general denomination which he proposes to substitute will "reduce the types and forms to their true value, destroy the factitious barrier erected by error between intermittent and continued fevers (of marshes), and conduct, by establishing a consanguinity between the different forms of the same disease, to a more rational treatment," (p. 31;) seeing that the true value of types and forms may be understood without a unity of name, and that consanguinity may be fully established and admitted, though the patronymics may be various.

His second, third, fourth, and fifth chapters contain, in the space of thirty-two pages, his views of the etiology of marsh diseases, the action of heat and of cold in producing them, the nature of miasm, and the modes of its introduction into the system. They contain no novelties of any value.

The sixth chapter treats of the antagonism of the palludal poison to certain pathological conditions, and more especially to the tubercular diathesis.

"The rarity and even complete absence of pulmonary consumption in certain localities is an incontestible fact, of which I shall not undertake the demonstration; but, if all physicians agree in recognizing it, it is still necessary that this remarkable phenomenon should be ascribed to its proper cause." (p. 72.)

With this sentence the author commences his demonstration of this proper cause residing in the marsh poison, and seems to speak of the opinion as hitherto unknown to the world, a conviction in which he is doubtless confirmed by the testimony of the "Société Royale de Médecine de Marseille." That learned body, in reference to this discovery, declares that "The observation which follows is one of great importance, and our colleague has all the merit of it; for no one that we know of has made it before him." (p. 330.) Perhaps some excuse for the ignorance of both parties on this point might be found in the circumstance that the essay of Dr. Wells, in which the doctrine was originally broached above thirty years ago, was published in a periodical work which did not continue long in existence;* were it not that Dr. Wells's doctrine has been noticed by many systematic writers on consumption. It is, nevertheless, true that the observations of that ingenious and able physician, though founded mainly on the statements of incompetent witnesses, on floating rumours, and general impressions, considering the importance of the subject, and the curious coincidence if not the respectability of the testimony which was adduced, merited more consideration and enquiry than have been bestowed upon them. The additional statements which are adduced by M. Boudin in support of the doctrine, though destitute of that precision and detail which alone can serve as the solid foundation of a general conclusion, are not altogether without interest.

"The action, sometimes prophylactic, sometimes palliative, of certain countries on the tubercular diathesis, has almost always been attributed to the influence of *méridionalité* of geographical latitude, or of temperature, if the

* Transactions of a Society for Improving Medical and Surgical Knowledge, vol. iii. 1812.

term be preferred. But it is requisite that observation should justify such an opinion, and to be convinced of this, it is sufficient to reflect that, if the febriferous Algeria excludes pulmonary consumption, the delta of the Rhine in Holland does so equally, whilst this malady, almost unknown in the marshy part of the Romagna, from the mouth of the Arno to Terracina, rages with the most cruel intensity at Naples, Malta, Gibraltar, and Corfu...........

"If tubercular consumption is rare in the marshy part of the north of Italy, to compensate for this, it exercises great ravages in certain southern countries of that peninsula. According to a statistical work, presented by M. Journé to the Royal Academy of Medicine of Paris, which he had framed at Naples from 1833 to 1837, 695 deaths occurred from pulmonary consumption, that is, 1 in 2·34 of the whole mortality. I have established many times the same contrast in the north of Africa. The rarity of diseases of the chest at Algiers is such, that it has happened to me to visit many hundreds of fever patients, without having had occasion to practise in a single instance auscultation or percussion of the respiratory organs. Among a total of 12,853 patients whom I have treated either in the army of Africa or at the Lazaretto of Marseilles (after their return from Africa), I have met with only 31 cases of consumption, of whom 25 had incontestably been affected with tubercle before having embarked for the Morea or Algeria.... ....

"We read in a letter addressed to the Academy of Medicine of Paris, 23d October, 1833, by M. Moreau, then physician to the army of Africa, the following passage, which constitutes a new proof in support of my opinion: 'I join to my letter a numerical table of the affections which were presented in the service with which I was charged at the military hospital of Bona; it comprehends a period of two years and a half. It thence appears that among 6,245 patients, 12 only figured as attacked with consumption, and that among 250 deaths, there were but 6 from that disease. .... The affections of the thoracic organs are presented so rarely to my observation in Africa, that their study is become very necessary to me, and I am come to Paris in order to follow the instructions and the practice of those great masters who treat those diseases daily. .... In short, my experience and my practice in Africa authorize me to conclude: 1st, that consumption is extremely rare among the inhabitants of that country; 2d, that Europeans are rarely affected by it; 3d, that the progress of the disease is retarded (enrayée) in consumptive Europeans transported to Africa.'" (pp. 72-5.)

M. Boudin adds, that while consumption is thus proved to be extremely rare in the essentially febriferous part of Algeria, the immunity from the tubercular diathesis appears to decrease in proportion as the country becomes more healthful in respect to the marsh diseases; so that a district often farther south, but less marshy than the coast, predisposes so much the more to consumption the less it is subject to fevers. "Thus, at Constantine," says M. Bonnafont (Géographie Médicale de l'Algérie), "where the fevers are both less serious and less frequent than at Algiers, a greater number of individuals affected with diseases of the chest, with scrofula, rachitis, &c., diseases almost unknown at Algiers, are remarked." (p. 77.)

Such is nearly the whole amount of M. Boudin's facts in support of his opinion of the beneficial agency of marsh exhalations in consumption. They are obviously extremely inconclusive and defective from the want of accurate statistical data and various details necessary to enable us to judge both of the value of the facts and of the other differences which may have existed between the localities which he contrasts, besides the existence and absence of marshes.

Before quitting this subject we shall adduce a few facts of an opposite

kind to those on which the above opinion rests. On turning to the statistical reports of sickness, &c. in the British army, we find that, in our West India possessions, of the more serious kind of diseases which affect the European troops, the most prevalent are those which M. Boudin has placed in the relation of antagonism, the so-called marsh fevers, and diseases of the lungs, of the latter of which consumption forms nearly one half. Among the black troops, the supposed protective power of miasmata against this class of diseases is still less observable, more dying annually from thoracic affections, among which consumption forms a very large proportion, than among the same number of troops in the United Kingdom by all diseases together. Yet the black troops are more constantly exposed to miasmata than the Europeans are. In Canada, the proportion of marsh fevers in the Upper compared with the Lower province has been found as 178 to 26, or indeed much larger, since the greater number of cases reported as occurring in Lower Canada had originated in the Upper province; yet the returns, so far as they go, render it probable that "the influence of consumption would be equally manifested in both." It still remains therefore to be discovered on what the great rarity of thoracic diseases among the troops in Algiers depends.

M. Boudin traces cholera, plague, and yellow fever to marsh poison, notwithstanding the ample testimony which exists of the frequent occurrence of those maladies in circumstances which exclude the possibility of the operation of that agent. "These three forms of disease," he says, with singular ignorance, "show themselves constantly preceded, accompanied, and followed by intermittent fevers." (p. 161.) There is a kind of cholera, doubtless, very common in the marshy districts of all hot countries; and yellow fever often affects localities of the same kind, as does also plague; but their frequent occurrence in situations far removed from everything like marsh is so well known, that M. Boudin's assertion serves but to illustrate the power of preconception on men of moderate understanding. In respect to plague, it has been asserted by Van Swieten, that intermittents are so opposite to it in their nature, that while other diseases showed a tendency to pass into that distemper when it was prevalent, tertian never degenerated into it. The plague of London in 1665 occurred at a time, as attested by Sydenham, when the city was unusually free from agues. In Egypt, too, it is well known that plague habitually ceases as an epidemic about Midsummer, the very time at which intermittents begin to prevail.

The nature and seat of marsh maladies are discussed in the tenth chapter. The author chooses to select the blood as essentially the seat of those disorders. In that fluid the miasm may lurk for a long period, and even without ultimately affording any evidence of its presence; and it is to this preoccupation of the blood that he ascribes the supposed protection against the tubercular *virus*. A curious instance, if there be no mistake, of the contamination of the blood by marsh miasm is given at p. 193:

"A female attached to the army, newly arrived from Africa, and enjoying perfect health, undertook to nurse an infant residing at Toulon; on the third day of the nursing, a marsh fever attacked the infant, and did not yield but to the employment of quinine."

Treatment. The therapeutic portion of the work is devoted chiefly to the virtues of arsenic. As a remedial agent, this substance has been employed from remote antiquity, according to Homberg, even by the Chinese and Hindoos. It has been long employed in modern times by all the nations of Europe. Our author, after some comments on the *doses* in which this remedy has been administered by different authors, viz. $\frac{1}{8}$ or $\frac{1}{2}$ grain, remarks as follows:

"It is because I am less convinced than these authors of the entire innocency of arsenical preparations administered in such doses, that I have applied myself to discover if it be not possible to preserve their efficacy by a smaller dose, while at the same time depriving them of their poisonous inconveniences: and I believe I have succeeded in resolving this problem in the most satisfactory manner. After having commenced my experiments with the twenty-fourth part of a grain, I am assured by successive trials, which have been already repeated with similar results by a good number of physicians at Marseilles, that arsenious acid properly prepared preserves, in the somewhat microscopic dose of the hundredth of a grain (un demi-milligramme), all its medicinal energy, not only in the treatment of marsh fevers, but also in a multitude of other diseases. Further; I have often obtained by a single dose of the hundredth of a grain of this medicine, the entire removal of fevers contracted in Algeria and Senegal, and which had previously resisted means of various kinds, including the sulphate of quinine and change of climate. .... A very remarkable circumstance is that the degree of efficacy of arsenical preparations, in the treatment of intermittent fevers, is subordinate manifestly to the reigning medical constitution, so that they are seen sometimes to lose in a great degree their febrifuge virtue, although some days previously, no intermittent fever resisted their heroic action." (pp. 276-8.)

This circumstance he ascertained not to depend on errors in preparing the material. The sulphate of quinine sometimes produced the best effects when the condition specified above occurred; but in the great majority of cases, the quinine also failed when the arsenious acid did not succeed. He has ascertained by numerous experiments that the action of quinine is equally influenced by the epidemic constitution. On the whole, in respect to the comparative febrifuge virtues of quinine and arsenic, he has found that the success of the former when the latter had failed formed the exception to the general rule, while nothing was more common than to find the arsenic successful in cases which resisted the quinine.

"I have been able, in a great number of cases, and by very small doses of arsenious acid, to put an end in a short time to quotidian, tertian, and quartan fevers, contracted in latitudes the most various, often complicated with chronic engorgement of the abdominal viscera, and for a long time rebellious to the sulphate of quinine." (p. 280.)

A review of the treatment of 266 cases, only half of those in which he had administered arsenical preparations, of which he has preserved notes, gives the following results: Of fevers, quotidian, tertian, quartan, quintan, irregular, and masked, 188 which had not undergone previous treatment, were cured by arsenic; 57 which resisted quinine were cured by arsenic; 13 which resisted arsenic were cured by quinine; and 8 resisted both remedies. He cannot specify the circumstances which would *à priori* indicate the preference of arsenic to quinine; but he has always begun his treatment with the former, and has had recourse to the latter

when the two or three first doses of the arsenic had not produced the desired result. He has used arsenic with success on many occasions in continued forms of disease when these were capable of being traced to marsh poisoning. Its beneficial action is not confined, however, to diseases which originated from this cause, for he has found it to have the power, in the dose of one hundredth of a grain of arsenious acid, of removing the paroxysmal complication which is so often noticed in the typhoid fever; in which disease it also appears to exert a contra-stimulant effect.

In considering the mode in which arsenic acts as a medicinal agent, he justly distinguishes its irritant effects on the gastro-intestinal mucous membrane from its remedial properties. The latter he ascribes to its absorption into the blood, an event which is favoured by the absence of its irritant effects, which latter interfere with the due performance of absorption,—whence his preference of minute doses.

M. Boudin is evidently puzzled between his doctrines of antagonism and a certain leaning to homœopathy. He mentions at p. 264, that on one occasion the arsenic appeared to produce, in a patient to whom he administered it for the cure of ichthyosis, a quotidian intermittent fever, which he was obliged to subdue with quinine; and at p. 296 he says: "On meditating on the phenomena observed in a great number of individuals poisoned by arsenic, and of which authors have transmitted to us an account, it is difficult not to perceive a certain analogy between these phenomena and those which the medicine is often required to combat in the sick."

In treating intermittent fevers with arsenic, he thinks it of great consequence that the doses should be administered always five or six hours before an expected paroxysm. He abstains from giving the medicine on the days of apyrexia, as useless; and if after two or three successive administrations no effect is produced, he resorts to quinine. In the continued form of marsh fevers he does not specify any particular time for the doses, nor does he mention the frequency with which they should be repeated. In cases complicated with bilious or inflammatory disorders, he advises the discontinuance of the remedy, or its being assisted by other means. In old and obstinate intermittents it may be continued for a longer time than is already specified; but no precise directions are given on this head.

The preparation which he prefers is the arsenious acid, from the greater simplicity of its composition than that of the other compounds of the metal. This or any other preparation may be given in the form of powder, with the sugar of milk; in pills; lavements; cigars (*cigarettes*), so as to be absorbed by the bronchial mucous membrane; or ointments. The cigarettes are much of the same kind as those employed by Trousseau in pulmonary disease. Boudin recommends an hundredth part of a grain of arsenious acid to be deposited on a small piece of paper, and then moistened with a few drops of water, so that the paper may absorb the solution. The paper being dried, rolled up, and lighted, the inhalation is performed. He does not say whether he has found this plan succeed in marsh fevers, the note to his formula containing directions merely for cases of asthma. If the arsenic be required for infants, he says, it may be given to the nurse or to a she-goat, or she-ass, whereby the milk becomes furnished with

the properties of the medicine. It would be difficult to regulate the quantity in this way, we suspect.

Of auxiliary remedies M. Boudin says very little. Arsenic needs them much less frequently than quinine does. He refers to the occasional necessity of emetics in bilious complications, and of bloodletting in the inflammatory.

The high price and frequent adulteration of quinine render such a remedy as arsenic worthy of much more attention than has been hitherto bestowed on it. In military practice more especially we are surprised that it has not come into more general use, considering that its influence in marsh diseases has been so long established. One great objection has, doubtless, been the occasional violence of what Fowler calls its "operative effects," when administered in the doses which have been commonly prescribed; and therefore, presuming that M. Boudin's account of the advantage to be derived from minute doses is correct, we consider his suggestions on this point as extremely important, and receive them thankfully, as some atonement for the poverty of his book in other respects.

Art. IV.

Medico-Chirurgical Transactions; published by the Royal Medical and Chirurgical Society of London. Second Series. Vol. VI.—*London*, 1841. 8vo, pp. 258.

This upon the whole is an exceedingly creditable volume, containing some valuable information. We shall, as usual, give a pretty full abstract of its contents.

I. The first paper is a short but very interesting communication from Mr. G. Gulliver, "on some points connected with the anatomy of the entozoa belonging to the *genus cysticercus.*" The principal object of the communication is to show the situation and structure of certain oval corpuscules, of which the author had previously given an account in a paper read before the Zoological Society. If the white part near the head of the entozoon be gently pressed a little viscid fluid will escape, containing a great number of these bodies. Their figure is that of a short and generally somewhat flattened ellipse, the length being to the breadth in the proportion of rather less than $1\frac{1}{2}$ to 1. Sometimes they are nearly circular. Their average long diameter is $\frac{1}{1332}$ English inch, their average short diameter $\frac{1}{2030}$ inch. They are of a whitish colour, smooth on the surface, and with a glistening appearance when seen by reflected light; viewed by transmitted light, they present a dark tinge towards the circumference, often with much brilliancy of surface, and a very clear and regular outline, but occasionally they have a dull white tint, and uneven margin. The majority are opaque, but in some a nucleus is visible; this is generally oval, but sometimes circular; it is most commonly situated in the centre of the corpuscule, but occasionally occupies a position near the edge. The texture of the nucleus is granular, as may be seen by compression.

The colour and opacity of the head and neck of the worm appear to

be dependent upon the presence of these corpuscules, for the intervening tissue is nearly transparent. They are most numerous about the middle of the neck; their number diminishes towards the head, and they are but thinly scattered in the neighbourhood of the suctorious [?] orifices. They terminate suddenly where the neck expands into the bladder-like body of the worm. They are arranged in two layers, a superficial and a deep; the first appear ready to be cast off, for many of them will fall into a drop of water dabbed with [?] the part, and numbers can be removed by gently scraping the surface of the corium. They are soluble in muriatic and acetic acids, with evolution of gas. The solution gives a white precipitate with oxalic or sulphuric acid.

The corpuscules are not contained in any visible sac or cyst, but are situated in the parenchyma. Mr. Gulliver believes them to be the *ova* of the cysticercus, founding his opinion upon their heterogeneous structure, their regularity in size and shape, the aggregation in the true body of the worm, and the abundance of carbonate of lime contained in their shells. "I am not aware," he says, "that any gemmæ or sporules have yet been found to possess these characters; if, therefore, the oval corpuscules should be regarded as sporules, they must be sporules of a peculiar kind." We think these observations of Mr. Gulliver are well worth the attention of naturalists. It is the most commonly received opinion that the mode of generation of the cysticerci is gemmiparous, but in truth, the whole subject is as yet involved in no little obscurity, and much more extended observation will be necessary before definite conclusions can be arrived at. The ordinarily-admitted distinctions between ova and sporules are somewhat of an arbitrary character.

The caudal vesicle contains a number of oil-like spherules, which Mr. Gulliver thinks are probably nuclei that advance to the neck of the worm, and there become invested with cells, and form complete oval corpuscules.

This paper also contains a good description of the hooks or spines which surround the proboscis of the worm.

Good drawings are given of all the parts described.

II. This is a very meager account of what is certainly (if there be no fallacy) a most remarkable "case of osseous union of a fracture of the neck of the femur," observed by Mr. Walter Jones, of Worcester. The subject was more than eighty years of age when the accident happened. After recovery the limb was shortened about one inch and a half, and considerably everted. The patient died two years afterwards. The neck of the bone was found much shortened and entirely osseous. We confess we should have liked greater details in the description. The nature of the case was probably satisfactory to those who had an opportunity of examining the specimens, but the readers of this communication will, we apprehend, find it anything but conclusive.

III. "Some observations on vaccination and small-pox," by Dr. G. Gregory. This is an interesting paper, and entitled to the fullest and most careful consideration.

The chief points adverted to are, the relation which subsists between the cicatrix and the character of the consecutive variola, and the theory of vaccine influence. We shall shortly notice both of these.

1. Three hundred and twenty-seven patients were admitted into the

Small-pox Hospital during the year 1840. Considerably more than half of these presented themselves in the course of the last three months. Eleven had complaints which proved not to be of a variolous nature. Of the remaining 316, 194 were wholly unprotected, and of these 87 died, or 45 per cent.; 120 had been previously vaccinated, and of these 8 died, being in the ratio of 7 per cent. only. Two were supposed to have previously undergone smallpox.

Dr. Gregory states that his attention had long been directed to ascertain whether any, and what relation subsisted between the number and character of the vaccine cicatrices, and the intensity of the consecutive variola; and he accordingly availed himself of the opportunity afforded by this epidemic for investigating the subject. The result was as follows: "In the majority of cases wherein the cicatrices are *numerous*, *normal*, and *well defined*, the consecutive variola is mild and varicelloid; again, where the consecutive disease proves severe, there the cicatrices will be imperfectly seen, or altogether wanting. But instances of the converse of these propositions are so numerous as scarcely to be called exceptions to a rule." In illustration of this remark, two series of cases are given, in the first of which smallpox was *severe* when the cicatrices were normal; and in the second, the lightest and most truly varicelloid eruptions coexisted with *small* and *very imperfect* cicatrices. We recommend the perusal of these cases to our readers, as being most instructive, and we think confirmatory of the opinion now expressed by Dr. Gregory, and which we have always entertained, that "the character of the cicatrix depends more on the *accidental* or *secondary*, than it does on the *primary* or *specific* inflammation," and that consequently little reliance can be placed on it as a measure of vaccine protection. Attentive observation of the disparity in the topical effects of vaccination in different individuals, the result of various but obvious contingencies, had long ago brought us to this conclusion, which the effects of our own revaccinations, and especially those on the continent, only tended to confirm. For these reasons we have been prepared to expect that Dr. Gregory's acuteness and opportunities of observation would eventually lead him to alter his former estimate of the three kinds of cicatrices figured and described by him some years ago, and adopt that which we have just quoted.

2. On the theory of vaccine influence. Dr. Gregory is not a believer in the identity of the variolous and vaccine poisons. He enumerates five distinct methods by which vaccinia may be produced: 1, By *spontaneous* generation in the cow; 2, by *contagion*; 3, by inoculation with the matter of *grease*; 4, by what is called *retro-vaccination*; 5, by inoculation with the matter of *human smallpox*, as clearly shown by the valuable experiments of Mr. Ceely, of Aylesbury, already fully noticed in this journal. (Brit. and For. Med. Rev., X., p. 464.) Upon these he remarks as follows:

"When we consider that five modes of producing this morbid secretion in the cow are now known to exist, it is not unreasonable to suppose that others may hereafter be discovered. In this state of our knowledge, then, surely we cannot be justified in assuming the fifth, and the last discovered of the whole, as the most important, and as affording the true clue to the mystery of vaccine protection. We should reflect that Mr. Ceely's experiments have entirely set aside Dr. Jenner's notion that vaccinia was the original or primitive poison, which

time and fortuitous circumstances had aggravated into the malignant or secondary form, which we call smallpox. They have proved (if indeed they have *any* bearing on the intimate nature of these poisons) that smallpox is the *primary*, and cowpox the secondary form. But when we further reflect on the absence of a contagious principle in vaccinia, and the remarkable fact that febrile disturbance is not essential to its perfect development, we shall probably be nearer the truth in saying that the vaccine is a poison *sui generis*; that its relation to variola is still hypothetical; that the real and intimate nature of the protection which it affords is still unknown to us; and that a thorough acquaintance with its anti-variolous powers must be derived, not from analogy, but from an extended and careful observation of facts, through a long series of years." (pp. 27-8)

We confess these arguments do not carry great weight with us. The origin of vaccinia from inoculation with the matter of *grease* is more than doubtful; and if it be the case, which is most probable, that the horse is liable to a varioloid affection resembling that of the cow (vide Brit. and For. Rev., IX. p. 78), and capable of producing the true vaccine vesicle in that animal, *as well as in man*, the question is evidently much simplified, for the poison may certainly, without any undue stretch of hypothesis, be regarded as identical in its nature in all these instances. Again, it should be remembered that the subject of the spontaneous development of the disease in the cow has scarcely yet been examined with sufficient care, we mean in its connexion with the existence or non-existence of smallpox in the same locality. In the present state of our knowledge, we cannot help thinking that Mr. Ceely's experiments go *very far* towards proving the identity of the two poisons; it seems much more probable that the inoculation of smallpox matter into the cow should produce a fluid of the same, though a modified, nature, than that it should originate a poison totally *sui generis*. At the same time we quite agree with Dr. Gregory that years of extended and careful observation may be required before we shall be enabled to form any definite and satisfactory conclusions.

IV. "On Gouty Concretions." By Dr. Alex. Ure. The object of this paper is to introduce to the notice of the profession the *benzoic acid*, as a remedy calculated to prevent or remove these morbid depositions. The following is the rationale of the proposed method of treatment.

The tophaceous concretions, or chalk-stones as they are called, consist of urate of soda, with occasionally a small proportion of urate of lime. One part of urate of soda requires about 4000 parts of water to dissolve it, and this is probably the cause of the refractory nature of the above deposits But the *hippurate of soda*, which exists in the urine of graminivorous animals, as the horse and cow, is an exceedingly soluble salt, requiring only two parts of water at 60° Fah. to dissolve one. It therefore occurred to Dr. Ure, that if by any means the hippuric could be substituted for the uric acid in the secretion of the human kidney, much good might result. Experiment showed that this could be readily effected.

"If, an hour after a meal, a scruple of benzoic acid be taken into the stomach, in the course of a couple of hours subsequently the urine voided, amounting to five or six ounces, will be found on adding a small quantity of muriatic acid, to yield a copious precipitate of beautiful rose-pink acicular crystals, which weigh, after being allowed to settle for a day, about fifteen grains. This quantity is by atomic computation equivalent to little more than half the benzoic acid employed."

Nearly the same results are obtained by employing the benzoate of ammonia or potass. No traces of uric acid or its salts, nor of benzoic acid can be detected in the urine after this treatment. No affection of the general health, nor irritation of the urinary organs was observed during the course of these experiments, which Dr. Ure first tried upon himself, afterwards upon individuals labouring under gout. Dr. Ure deserves great credit for being the first to suggest this philosophical plan of treatment, which we recommend to the earnest attention of our readers.

V. A "case of Phlebitis," by T. H. Silvester, M.D. The most remarkable features in this case were, 1, the situation of the disease, in the veins of the upper lip, side of the nose, and scalp; 2, its insidious progress, commencing from the irritation of a pimple, with the mild symptoms of a common cold, without violent shivering or delirium, the pulse and countenance alone betraying the severe nature of the affection, and these even regained their ordinary tranquillity in the course of the affection, while the patient's appetite never failed; 3, the appearance of a peculiar exudation along the course of the veins, which formed scabs, resembling those of *rupia;* when these were artificially removed, purulent matter escaped, and an ulcerated opening of the vein was found; but if they were allowed to fall off spontaneously, the vessel underneath was seen to be filled up with lymph. The patient was emaciated to the highest degree at the time of his death.

Why does Dr. Silvester use the French word "foyer," for a collection of purulent matter? Surely his own language could have furnished him with a term, at least as expressive and correct.

VI. Mr. Cæsar Hawkins has related "four cases of cancerous or malignant disease of the spinal column." We pass over the first, in which the patient died from mechanical obstruction of the bowels, by the pressure of two ovarian tumours upon the rectum, while the local disease was yet in an early stage; and proceed to give some account of the second, which presents also several interesting physiological features.

Jane Hall, æt. 55, was admitted for paraplegia. Her right breast had been removed six years previously for cancer, and she had remained well until May, 1839, when some cancerous tubercles formed in and around the cicatrix. Two months before the appearance of these tubercles, she began to complain of pain in the dorsal region of the back, and this was gradually followed by the ordinary symptoms of paralysis. The only appearance deserving notice in the back, was the projection of the spinous process of the sixth dorsal vertebra, which formed an acute angle with the rest of the column. Pressure upon this point, or anywhere below it, and for a little way above, gave rise to much increase of the pain.

Below the affected part of the spine the functions of the various organs were materially impaired, the patient, when attempting to sit up, feeling "as if she was about to drop into two parts." 1. All *voluntary muscular power* was totally lost, except *occasionally* over the expulsor fibres of the rectum. Many of the paralytic muscles were in an almost constant state of tonic spasm, and there were, at times, convulsive or clonic, and *painful* spasms in the same, or other muscles of the limbs and abdomen. The feet and legs did not much participate in this tonic spasm, nor in the occasional convulsions, and their muscles did not act nearly so much

from external impressions, such as pressure, or pinching any part of the limbs, or tickling the soles of the feet, which readily excited the action of the muscles of the hip and knees. The sphincters of the anus and bladder were permanently paralysed. The bladder still retained its expulsive power, so that there was never any partial retention, but, unlike the rectum, it was entirely removed from the dominion of the will. 2. The *sensibility to external impressions* was entirely lost in every part below the seat of the disease, so that she could not feel smart pinching or pricking, or heat or cold in any part. The outlets of the bladder and rectum were also utterly insensible. Yet the spasms, whether spontaneos or excited, caused violent pain; and there was occasionally severe pain in the limbs even in the absence of all spasm. She had also much suffering in the abdomen, but this was probably dependent upon the cancerous disease which was detected after death in the peritoneum. 3. The *temperature* of every part below the disease was permanently exalted, being about 4° higher than that of the upper half of the body, allowing for difference in clothing, &c. 4. The secretions of the kidneys and intestines were, as usual unhealthy. The urine was alkaline, but contained no mucus. 5. The circulation and nutrition of the paralytic parts were impaired in the common way, sloughs and bullæ forming. The mortification on the hips and nates was the immediate cause of death.

On examination, all the vertebræ were found unusually soft and vascular, and in several, spots or tubercles of a yellowish white substance were found in the lateral plates. The medulla presented no appearance of inflammation. Opposite the sixth dorsal vertebra, it had been pressed upon by a tumour projecting from the body of the bone, and a deep sulcus of the entire circle formed, to the extent of fully one inch and a half; so little medullary matter remained in this portion, that the centre was almost transparent. The morbid structure which formed the tumour occupied three of the vertebræ, the sixth or central one being the most altered in shape. Four oval prominences projected into the canal from the body of this bone, over one of which an oval opening of the dura mater with smooth edges had been formed, apparently by absorption rather than ulceration. The new growth was of firm consistence, composed of fibrous structure of a white appearance in bands, with some yellow softer matter in the interstices. The other morbid changes discovered need not detain us.

This is certainly a very interesting case; not only as presenting an example of a rare form of disease, scirrhus of bones being much less common than the fungous and medullary varieties, but from the physiological symptoms to which the pressure upon the cord gave rise. It is evident from the facts stated above, that the muscular irritability of the paralytic limbs was *augmented* rather than impaired. The excito-motory phenomena were also well marked, and yet there was total loss of sensation: this is another addition to the now constantly accumulating proofs of the correctness of Dr. Marshall Hall's views on this point.

But the narrative of this disease brings before our notice some other features, of which it is extremely difficult to find a satisfactory explanation. How for instance, shall we account for the occasional voluntary power over the detrusor muscles of the rectum? and for the entire absence of any such control over the bladder, although its coats were

equally free from palsy? What reason can be stated for the permanently increased temperature of the affected parts? Dr. Abercrombie explains phenomena of this nature by their having lost in some degree the power of preserving a medium temperature, and being consequently more affected by external circumstances; but Mr. Hawkins informs us, that, even when allowance was made for such influences, there was still a difference of 4° between their temperature, and that of the healthy parts.

We must advert to one other symptom, namely, the *spasms*. These have been commonly regarded as pathognomonic of disease of the membranes; but in the case before us, no such cause existed, for the opening through the dura mater was, according to Mr. Hawkins's account, the effect of absorption alone. We have ourselves met with at least two cases in which precisely the converse of this obtained, i. e. extensive disease of the spinal membranes, and no spasms; and we are, therefore, greatly inclined to think that the ordinary opinion is not so well founded as has been imagined.

In the cases above noticed, the disease of the spine was secondary to cancer of the breast; but the paper before us contains the record of two others, in which it was the primary affection. We have not space for them here, and must content ourselves with the simple statement, that one of them is regarded by Mr. Hawkins as an example of scirrhus, (though the description of the morbid structure does not appear to us very satisfactory,) while the other is an instance of fungous disease in a child, and affords another lamentable example of the little success which too often attends operations for the removal of these growths from the bones of the face. The whole paper is well deserving of an attentive perusal.

VII. Mr. Halberton has related a "case of slow pulse with fainting fits," which is chiefly remarkable for the very gradual development of the symptoms, and their apparent connexion with an injury of the neck received two years previously. We say *apparent* connexion, because the history of the case is somewhat defective, and there were also changes in the structure of the heart which throw some doubt upon the alleged origin of the affection.

VIII. The next article is a fourth memoir by Dr. Marshall Hall, "on some of the principles of pathology of the nervous system," and is equally interesting and important with its predecessors. It therefore claims from us a fuller notice than most of the papers with which it is associated. In this memoir, Dr. Hall directs attention to a very important topic, without an acquaintance with which, no scientific principles, however elevated their generality, however wide their scope, can be of any avail: this topic is, "How to observe." He had, on former occasions, remarked with justice, that almost all the diseases of the nervous system, or the groups of morbid phenomena which it exhibits, now require to be observed and recorded again, with express reference to his recent discoveries; and he now lays down the plan of observation which he considers most likely to produce valuable results. Of this plan and some of its applications we shall introduce a brief sketch, in the hope that the author's laudable design may be thereby in some degree furthered.

In considering the influence of any disease upon the nervous system, we must consider that system under three distinct divisions—the cere-

bral, the true spinal, and the ganglionic—and we must trace its influence upon each of these. This leads us to the following enquiries:—1. What *are* the distinct diseases of the cerebral, of the true spinal, and of the ganglionic subdivisions of the nervous system? 2. What is the influence of disease of one of these systems on the other two, respectively? 3. In what order is that influence manifested?

A disease which affects one portion of the nervous system exclusively, will present symptoms exclusively referrible to it. Thus, tetanus, in its simplest form, is exclusively spinal; whilst apoplexy or hydrocephalus is at first exclusively cerebral; and hemiplegia may be either cerebral, or cerebral and spinal. But it very commonly happens that diseases originally affecting only one portion of the nervous centres, extend their influence to others: this is the case, for example, with the severer forms of apoplexy and hydrocephalus which at first manifest themselves only in mental inactivity, more or less complete, but which subsequently affect the medulla oblongata, causing difficulty of respiration and deglutition, and may extend their depressing action to the whole spinal and ganglionic systems. "In cases of hemiplegia, the *danger* is precisely in proportion as spinal symptoms are superadded to those of the cerebral system. If the respiration be stertorous—if the deglutition be difficult—if the functions of the bladder, rectum, and sphincters be impaired, there is great danger; if these events *continue* for a considerable time, or if they *supervene*, the event is always fatal. . . . . The spinal symptoms which exist at first and gradually yield, probably depend upon *counter-pressure* from congestion; this counter-pressure is relieved by blood-letting, &c., and its effects cease. When, on the contrary, the spinal symptoms continue, in spite of the remedies, they probably depend on the extent of the effusion; and this cannot be remedied." In the simple congestive apoplexy, also, the spinal system may become affected by the counter-pressure; and this is indicated by stertorous respiration, immobility of the eyelids, difficult deglutition. From this condition the patient may partially recover by means of free depletion; the breathing becoming more natural, a deep inspiration being excited by dashing cold water on the face, and the eyelids closing when the eyelashes are touched, without any decided advance towards consciousness—showing that the spinal symptoms are only consecutive upon the cerebral. If they *continue*, however, the issue must be fatal; for the patient will die of asphyxia. There is one remark of Dr. Hall's on this point with which we cannot accord. He says, "If the stupor and stertor continue, the next series of phenomena are those observed to result from defect of the functions of the ganglionic system. The bronchi become clogged with mucus, and the intestines distended or tympanitic from flatus." Now, Dr. J. Reid's experiments have clearly proved that the source of the effusion in the lungs noticed in cases in which the pneumogastric nerves have been divided, is to be found, not in any direct power of the nervous system over the secreting processes, but in the stagnation of the blood in the pulmonary vessels, which results from deficient respiratory action. We should not have noticed Dr. Hall's error (for such we believe it to be), were we not jealous of anything that can tend to perpetuate a doctrine which we believe to have had a most injurious influence on the progress of sound physiology—that of the imme-

diate dependence of the functions of nutrition, secretion, &c., upon the ganglionic system of nerves. Tetanus is one of the most characteristic examples of a true spinal disease; the cerebral system being long spared, in fact, until a late period of the disease, when delirium or stupor manifests itself. In regard to the primary and secondary affections of the ganglionic system, our information is less precise; for the very obvious reason, that our knowledge of its normal functions is very far from being definite. "It is doubtful," Dr. Hall thinks, "whether any set of *diseases* originates in the ganglionic system; but there is a series of accidents which have their chief seat in this system. It is those which are caused by *shock*." That the nervous system has an influence, of which the nature is yet unknown, both upon the contractions of the heart and arteries, and upon the forces which move the blood in the capillaries, there can be, we think, but little doubt; and there can be as little, that this influence is conveyed by the ganglionic system. It may originate in any part of the nervous system; for concussion of the brain, sudden breaking down of the spinal cord, blows upon the epigastrium, or crushing a limb may produce the same effect. In the third of these cases, the impression is manifestly on the visceral nerves, which are derived from the sympathetic system; and this is probably true of the last also, since Dr. Marshall Hall has shown that the suspension of the circulation may be equally induced in a frog, by crushing its leg, after the spinal cord has been divided, as if it were uninjured. Dr. Hall thinks that, in exhaustion from loss of blood, we have a very good example of the successive affection of the cerebral, spinal, and ganglionic systems from the same cause. "The *cerebral* system is first affected; and then the previous symptoms of reaction give way to impaired vision and hearing, dozing and slight coma, and slight delirium when roused; then the *true spinal* system suffers, and the respiration loses its regular, even, and rhythmic character, and becomes slightly audible or stertorous, and each inspiration becomes accompanied with a sudden descent of the larynx—a symptom from which I have never known a patient recover; deglutition is slightly impaired, and the larynx is irritated to choking and violent coughing by the admission of fluids, whilst the sphincters of the rectum and bladder fail; lastly, the power of the ganglionic system fails too, and the respiration becomes marked by a slightly crepitous rattle, like the catching of the larynx—a fatal symptom—and the intestines become tympanitic. I have seen precisely the same order of symptoms, the same order of affections—first, of the cerebral, then of the true spinal, and lastly, of the ganglionic functions—from shock to the nervous system" by mental emotion.

Dr. Hall then directs attention to the principles of *irritation* and *pressure*, as causes of particular symptoms of nervous disorders. He remarks that, since it is a well-established physiological fact, that irritation of the cerebral substance itself cannot produce muscular movement, whenever spasmodic affections present themselves in nervous diseases, we must conclude that the spinal system is involved, either primarily or secondarily in the malady. Now, there are various modes in which cerebral disease may produce spasmodic actions through the spinal cord; as, for example, by counter-irritation or counter-pressure on the cord itself, or by involving its excitor nerves. Of the latter and least obvious

mode of action a curious and important illustration is given in a postscript, in which Dr. Hall thus calls attention to the influence of irritation of the *membranes* of the brain in inducing *spasmodic* affections. "In an important experiment which I propose to lay before the Society in the next session, I found that, although every kind of irritation, puncture, laceration, &c., of the cerebrum and cerebellum was entirely inoperative, yet that laceration or pinching of the dura mater immediately induced peculiar spasmodic movements of the eyeball, the eyelids, the head, &c. These effects are probably induced through the branches of the trifacial nerve, which, as in the recurrent of Arnold, is well known to impart branches to the dura mater, and which may do so to other membranes within the cranium." It cannot be regarded as improbable that the surfaces of other membranes should be similarly furnished with excitor nerves, so that spasmodic diseases may result from states of irritation in them. This is a most fertile subject for experimental enquiry.

As, in all the secondary affections that have been alluded to, time is a most important element, we can no longer be surprised "that the same lesions, as found *post mortem*, have been attended by totally different series of symptoms during life, any more than that in the different periods of the same lesion, the symptoms have been different. . . . . It is not the *disease*, but its *effects* upon the brain and spinal cord, which are the source of the symptoms. If ramolliss ment, effusion, a tumour, &c., produce similar effects on these textures, the same affection of the functions, the same symptoms will be observed. . . . . If the source of the symptoms be not the mere lesion of a function, induced by the lesion of a special part or organ of the encephalon, but the effect of irritation and counter-irritation, of pressure and counter-pressure, it is obvious that these primary effects, and their effects in their turn, may result from *any* disease, if the *times* be similar, whatever that may be. It is accordingly to the *history* that we chiefly have recourse for the diagnosis of cerebral diseases, and especially to those of the seizure and first stage: at their close, almost all diseases of the encephalon are alike; almost all terminate by coma, paralysis, convulsions, stertor, and impaired actions of ingestion and egestion of the orifices and sphincters, from compression of the cerebrum and medulla oblongata." Morbid changes take place towards the close of many diseases which do not properly or at all constitute the disease; on the other hand, the physical condition of some morbid states, such as that resulting from *shock*, *exhaustion*, &c., are of a kind which seem totally inscrutable, at least to our present means of research. The effects of cold in occasioning at first paralysis, and subsequently in many instances spasmodic tic, are also noticed by Dr. Hall as interesting subjects for enquiry. It is to the indication of such subjects, and of the mode of prosecuting the investigation, that the present memoir is devoted. We could wish that it had been more systematic in its form, especially in regard to its special object—"What to observe;" since we fear that those who have not made the subject one of especial study, will find it difficult to apply Dr. Hall's principles to the cases that may fall under their notice; and that a large amount of valuable data may thus be lost. The following table is given by Dr. Hall as a classified view of the points of enquiry to which he considers that our attention should be specially directed in cases of nervous disease.

i. The cerebral symptoms.
    1. Excess, or defect in the senses—pain.
    2. Delirium—coma.
    3. Paralysis.
ii. The true spinal symptoms.
    1. Spasm, clonic or tonic.
    2. Paralysis, in regard to
        a. The functions of ingestion.
        b. The functions of excretion.
        c. The muscular system generally.
iii. The ganglionic, in regard to
    1. Nutrition.
    2. Temperature.
    3. The secretions; especially those of
        a. The bronchi.
        b. The stomach and intestines.
        c. The kidneys and bladder.
iv. The effects of emotion.
v. The effects of shock.
vi. The effects of counter-pressure, &c.

We think it necessary to add, that, in recording the symptoms of a case of nervous disease under these heads, their *sequence* should be most carefully noted; the inferences from this being, as Dr. Hall has shown, frequently more important than those derived from the simple occurrence of the symptoms. We trust that his perseverance in following up the important enquiries to which he has devoted himself may be speedily rewarded by a splendid collection of such cases, "taken with the care and accuracy of M. Louis, and then as carefully analysed and compared."

IX. Mr. Stanley has given an account of "seven cases of spontaneous dislocation of the hip-joint." They cannot be satisfactorily abridged, and we have not room for their introduction in full; we must therefore content ourselves with simply directing the attention of our readers to them, with this passing remark, that Mr. Stanley appears to us to have passed over somewhat too slightly the local effects which may be rationally supposed to have resulted from the injuries mentioned in two, and the rheumatic affections which existed in three of the number. We certainly think it most probable that in most, if not all of these there must have been some low degree of inflammation, ending, perhaps, in effusion, and thus productive of elongation of the ligaments. It will be observed that in one case only (the second in which the dislocation occurred during paralysis from disease of the spinal cord) was there any dissection, and therefore the actual state of parts could only be supposed. The subject is one of considerable interest. We have no doubt that many surgeons have incurred most unmerited opprobium for the supposed fault of having overlooked dislocations, when in reality such were not in existence at the time of their examinations; having occurred subsequently, in a similar manner to those which Mr. Stanley has related, during the progress of convalescence or disease. The fact of such things being far from uncommon should be extensively known.

X. This is a short but interesting communication from Dr. Addison, "on some points in the anatomy of the lungs." The following is the result

of his examination into the distribution and course of the *pulmonary veins.*

"The pulmonary artery was injected with size, coloured red, whilst the vein was injected with the same material, coloured yellow: the lung was then laid aside, and kept moistened in a cool place for several days, with a view of softening, by approaching decomposition, the connecting cellular membrane distributed throughout the lungs. In this way the common cellular membrane beneath the pleura became so lacerable that the pleura itself was stripped off without much difficulty, and without inflicting any breach whatever in the aeriel cellular structure of the lung which it had covered. The lung, thus divested of its pleura, presents to the eye, more or less distinctly, lines on its surface, which indicate the situation of what may be called the *pulmonary fissures* —a term more correctly applicable than that of interlobular, inasmuch as by the term interlobular is usually understood a something situated between either the longer lobes or smaller lobules; whereas, by the term *pulmonary fissures* is meant certain spaces occupied by common cellular membrane, and which descend from the surface towards the interior, but without penetrating the aeriel cellular tissue of the lungs; thereby dividing, more or less deeply, the surface of the organ into a number of insular portions, some of which may comprise a great number of lobules. Guided by the linear indications on the surface of the now naked lung, we can in general, with the aid of a pair of points, let into handles, or a pair of fine scissors, and without much difficulty, succeed in laying open and exposing the pulmonary fissures, at the bottom of which, merely surrounded by a loose cellular membrane, and resting on the unbroken aeriel pulmonary tissue, we discover a vessel; that vessel is the pulmonary vein—alone, and unaccompanied by any artery whatever. This vessel may be distinctly traced from larger to smaller trunks towards its source, until we reach the common cellular membrane between the ultimate lobules, from the exterior of which the vein appears to originate; whilst, on the other hand, by continuing the mechanical operation towards the root of the lungs, we with almost equal facility trace the vessel, still lying at the bottom of the pulmonary fissures,and becoming gradually larger and larger by the addition of branches, which proceed into the pulmonary fissure, and are derived either from the neighbouring smaller pulmonary fissures, or from the uniting cellular membrane between the ultimate lobules themselves, until at length it joins the large trunks at the root of the lungs to form the great pulmonary veins. A small artery is not unfrequently observed running across the pulmonary fissures, from a portion of lung on one side to a portion of lung on the other; and in one instance I have found an exceedingly narrow strip of healthy lung passing like a bridge across the fissure, on the very surface of the lung.

"Thus, then, the human lung may be said to be made up essentially of a vast expanse of membrane, the interior of which, during the whole of extra-uterine life, is unceasingly exposed to the influence of atmospheric air, and upon the surface or in the substance of which are spread out the capillary ramifications of the pulmonary artery; these arterial capillaries passing from thence to the exterior of the membrane to form the pulmonary vein, which throughout its whole course is found to be situated on the exterior of the aeriel cellular structure of the organs." (pp. 151-2.)

The application of these views to the purposes of pathological anatomy is sufficiently obvious. Dr. Addison thinks they will be the means of throwing additional light on the origin and progress of phthisis, and will enable us to set at rest the long-agitated questions respecting the origin and seat of pulmonary apoplexy, and more especially of what has been called *œdema pulmonium*.

XI. "A statistical account of the results of amputations at University College Hospital," by Mr. John Phillips Potter, late house-surgeon.

This is one of a class of papers which we hope to see continually mul-

tiplied. It is a lamentable fact, that little has been hitherto done in this country for the advancement of our science in the only path that can conduct to accurate and important generalizations; and we therefore hail, with the greatest satisfaction, every attempt, however humble, to fill up an hiatus that does so small credit to our philosophy.

The account before us extends over a period of six years and half, and comprises the results of 66 amputations on the shoulder, arm, forearm, wrist, thigh, and leg. The number is too limited to warrant the formation of any definite conclusions; but the report is valuable as far as it goes, and more especially because the cases were all treated on the same principles, and placed as nearly as possible under similar circumstances.

Of these 66 cases, 10 were primary amputations, (in the text the number is stated to have been 11, but the tables and calculations show that this must be an error of types,) and three of these proved fatal. Of the remaining 56, all of which were secondary operations, only 7 were lost. There were 22 cases of amputation of the thigh, with 4 deaths; 25 of the leg, with 3 deaths. Nearly all of the latter were performed close to the tuberosity of the tibia, leaving only a sufficient length of stump to fit firmly on the cushion of the wooden leg. The success attendant upon them is very different from that which has been procured in other hospitals. In the Glasgow Infirmary, for example, we find by the statements of Dr. Lawrie that fully one half proved fatal. (Lond. Med. Gaz. vol. i. p. 397.) How this discrepancy is to be explained, we cannot tell. The flap operation was adopted in all. With *very few exceptions* we believe this mode is decidedly preferable to the old circular method. The paper before us contains nothing new upon this point, beyond the advantage of forming the flaps of skin alone, when the leg is removed, immediately below the knee, and the patient is remarkably muscular. In only one case was the tourniquet applied, compression by the fingers being trusted to in all the others. There are many advantages in this plan, but the want of efficient assistance must often render it inapplicable in private practice. The paper closes by an account given by Mr. Liston of the mode of dressing which he employs. This eminent surgeon's views are now so generally known to the profession that we shall not occupy any more space with them here, than simply to bear our testimony to the striking success which we have often witnessed from their adoption. We should state also, that out of the whole number of cases there were only two instances of secondary hemorrhage, which is so much dreaded by the opponents of the flap method.

XII. "On the treatment of colica pictonum by warm water," by Dr. John Wilson. The mode advocated in this paper is the use of the warm bath, and the injection at the same time of some of the water into the intestinal canal. Copious evacuations have been procured in this way, with speedy relief of the pain, when all other remedies had failed. Cases illustrative of its success are narrated. The plan is applicable to other diseases in which obstinate constipation is a prominent symptom. It is very simple, and deserves trial.

XII. Dr. R. Boyd has related a case of "malposition of the kidneys, absence of the vagina, uterus, and fallopian tubes, and disease of the left ovary." The right kidney was found in the right iliac fossa; the left in

the pelvis, below the psoas. The renal capsules were in their normal position.

XIV. "Pathological and surgical observations on diseases of the ear," by Joseph Toynbee, Esq., F.R.S. This is a very valuable paper, containing the first fruits of an enquiry conceived in a just spirit, and pursued with philosophical accuracy. It is perhaps less valuable, however, for what it does than for what it promises, and to which it will probably lead the way. It must be acknowledged that the science of surgery has hitherto thrown but a very faint light upon the nature of the diseases of the ear; indeed, this branch of surgery is so far in the rear of all others, that, in the *vast majority* of cases of deafness which fall under the observation of the surgeon, he is quite incapable of offering any diagnosis or of affording any relief. There is no truth-speaking member of the medical profession who has not made such a confession, and many who have tried the modes of relief which have been lately suggested, and found them inefficient, are inclined to believe that, as a general rule, deafness is an incurable disease. Under these circumstances, it becomes indeed most desirable that some light be thrown upon the true nature and the seat of this class of diseases; and, as is stated by the author of this paper, this end can be accomplished only by the aid of pathology.

Mr. Toynbee here points out the pathological changes which the structures composing the ear undergo, by giving the details of 41 dissections which he has conducted: 39 of the specimens were taken from patients who died of various diseases and at different ages, who during life were not known to be deaf. In pursuing this plan, with the expectation of discovering disease in its incipient state, he follows the example of Sir B. Brodie in his researches upon the Joints; and, like him, Mr. Toynbee finds disease in organs which were supposed to be healthy; and he is thus enabled to point out the seat and nature of the morbid changes at their origin. It appears that, of the 39 specimens examined, 29 presented well-marked pathological conditions in different stages. These may be divided into three classes: the *first* consisting of those in which the mucous membrane of the cavity of the tympanum is in a thickened state; the *second*, in which membranous bands connect various parts of the cavity of the tympanum; the *third*, in which the thickened state of the membrane is combined with the presence of the bands of adhesion. The appearances found in the healthy specimens are first described; from which it appears that the mucous membrane of the tympanic cavity presents the following characters when in a natural condition:

"The cavity of the tympanum contains but a very small quantity of mucus, which is spread over the surface of the investing membrane. The latter is so extremely thin and transparent, that its presence upon the surface of the osseous walls of the tympanum cannot be detected without the use of a magnifying glass, and by the aid of the touch. The nervous filaments upon the surface of the promontory are most distinctly seen; the margin of the fenestra rotunda is distinct and defined, and the membrane which closes it is thin and transparent. The ossicula at first sight do not appear to be covered by any membrane. The crura of the stapes and their point of attachment to its base are seen distinctly, and between the inferior surface of these crura and the promontory is seen a well-marked fissure." (p. 194.)

The author then proceeds to show that, in specimens wherein this membrane is thickened, it becomes opaque and white; the tympanic plexus of nerves is not discernible through it; the ossicles receive a thick investment from it; the stapes is completely concealed; the fenestra rotunda, instead of having the appearance of a defined foramen, presents a simple depression in the pulpy membrane: bands of adhesion most frequently coexist with this "soft and velvety" condition of the membrane, and they have been observed to connect the crura of the stapes to the membrana tympani and surrounding parts, and the malleus and incus to the surface of the promontory and to the membrana tympani; accompanying this diseased state of the membrane is an increased secretion of mucus.

The following is a concise view of the state of the cavity of the tympanum in the 41 dissections detailed in this paper:

"1	In a healthy state	10
2	With simple thickening of the investing membrane	6
3.	With bands of adhesion passing from various parts of the cavity of the tympanum, most frequently connecting the stapes to the circumference of the tympanic cavity	4
4.	With slight thickening of the investing membrane, accompanied by the existence of bands of adhesion	13
5.	With considerable thickening of the investing membrane and the presence of bands of adhesion	5
6.	Suppuration in the cavity of the tympanum	1
7.	Anchylosis of the base of the stapes to the circumference of the fenestra ovalis	2
	(p. 208.)	41"

The author concludes his paper with observations to the following effect. He says, it must appear remarkable, that, out of 39 ears taken promiscuously, 29 should have presented appearances indicative of disease; he supposes, however, that in several specimens the deviation from the healthy state was so slight, that it may be presumed no derangement of the functions of the organ attended them. Nevertheless, he continues, the specimens in which the diseased conditions were considerable are so numerous as to cause surprise, which, however, will be diminished when it is considered that many persons who consider that they *hear perfectly well* cannot distinguish the ticking of a watch at a distance of two feet and a half, or even of four or five inches, which can be heard by the healthy ear seven or eight feet from the head. Dr. Wollaston wrote a paper in the Philosophical Transactions, showing that there was a very striking difference between the powers of hearing of different individuals; and as the defects to which Dr. Wollaston refers always occurred in persons more than twenty years of age, Mr. Toynbee is inclined to believe that they may be ascribed to the pathological conditions which he has pointed out.

XV. Mr. Roden has recorded "two cases of displacement of the long tendon of the biceps humeri." In the first it was unaccompanied with any other injury, and the tendon was found lying on the lesser tubercle of the humerus. In the second the bone had been dislocated forwards

and upwards, and the tendon was found lying at the inner and posterior aspect of the joint, having completely slipped over the head of the humerus.

XVI. Dr. J. A. Wilson has given an account of "two cases of aneurism of the superior mesenteric artery," a rare form of disease. The first was particularly remarkable. The patient, a female, æt. twenty-four, suffered under jaundice of a very severe character, which proved fatal seven weeks after her admission. External examination of the epigastric region (where there was occasional pain) gave no indication of the existence of any tumour, and the true nature of the case was, of course, not suspected until after death—an excellent commentary upon the necessity of using the *ear* as well as the hand in all doubtful affections of the abdominal cavity. Upon dissection, it was found that the sac had compressed the ductus communis choledochus throughout its whole extent. In the second case the tumour was felt, and its structure recognized.

XVII. The last paper is one by Mr. Stanley, "on congenital tumours of the pelvis." The author considers that they may be arranged in four classes. 1. Those in which the tumours consist wholly of morbid tissues, generally a combination of the fibrous and cystic varieties. 2. Those in which they are composed of morbid structures, in combination with isolated portions of perfectly-formed animal organs, having no other relation to the living being with which they are connected, than as they are dependent upon it for the means of nutrition and growth. These belong to the class of parasitic monsters. 3. Spina bifida. 4. Those in which the tumour is composed—either wholly or in part—of membranous cysts, communicating with the spinal canal, but *exteriorly to the theca.*

Mr. Stanley mentions one fact which has considerable interest in a physiological point of view. In two of the cases examined an *isolated* portion of intestine was found in the parasitic monster, containing a fluid, "which in colour and other obvious characters closely resembled meconium, although there existed no liver or other distinct hepatic apparatus which could have furnished the colouring matter of the fluid, and there was certainly no communication between this portion of intestine and the intestinal canal of the child to which the parasitic monster was attached." We cannot help regretting that recourse was not had to chemical analysis; it would have been extremely important to have ascertained the presence or absence of other essential constituents of the bile besides the colouring matter.

A question naturally arises in reference to these tumours—should they be ever made the subjects of surgical operation? The paper before us shows that in some instances they might be removed with success; indeed Mr. Blizard has done so with the best results. But when we consider the almost absolute impossibility of determining the true nature of the case by external examinations, and the certainty of death if there were any communication with the theca of the cord, we feel greatly inclined to the opinion that it would be the part of a wise surgeon to decline all interference.

Art. V.

Grundzüge einer neuen und wissenschaftlich begründeten Cranioscopie. Von Carl Gustav Carus.—*Stuttgart*, 1841. 8vo, pp. 87.

Principles of a New and Scientifically-based Cranioscopy. By C. G. Carus.—*Stuttgart*, 1841.

A period of little more than two years has elapsed since we felt ourselves called upon to enter at some length into an examination of the merits of phrenology, and of its claims to the respectful and attentive consideration of the members of the medical profession. In this examination we first took a cursory view of the actual estimate in which the subject seemed to be held throughout a considerable portion of the civilized world; we next discussed the character of its leading principles and some of the more interesting details, always attempting to keep in view the true value of the evidence upon which these rested; and in conclusion we expressed our conviction that, as a matter susceptible of practical application, it would be found, so far as proved to be true, of considerable importance and utility, not only in medicine, but also in many other things bearing upon the welfare of humanity. In this our advocacy, so to speak, of the real deserts of phrenology we would have it distinctly understood that our assent and our convictions extended only to that aggregate of natural facts which, in our belief, had been ascertained from correct and unbiassed observation, and only to those principles which constituted the expression of the general truths elicited by those facts. There are few sciences which have not suffered disfigurement and whose progress has not been seriously retarded by inaccurate observation and hasty generalization; and it was *à priori* to have been expected that phrenology, however true in its foundation, should, in the erection of the superstructure, be subjected to the same hinderances and causes of misapprehension, to the same admixture of inaccuracy and imperfection in the detail, and to the same confusion of mere hypothesis with true logical deduction, as more or less happens to almost every science, especially when in its infancy, and when struggling for general recognition. We conceive, indeed, that this has been the case with phrenology to a more than ordinary extent; and to this cause we mainly attribute the great backwardness shown in so many instances by scientific men to a fair and candid examination of its true merits. We propose in the present article, before referring to the work with whose title it is headed, to offer a few remarks upon the present state of phrenology *as a science*—as an accumulation of facts developing principles—upon some of the causes which, in our opinion, have retarded its progress *as a branch of physiology*—and upon the necessity of its being prosecuted more in the spirit of a true inductive philosophy than has hitherto been exhibited by many of its more enthusiastic and popular expounders, if it is to emerge from its present infantile condition, and to obtain the bold and defined outline of a well-matured science, commanding, not soliciting, the attention of those to whom its truths are of practical importance.

Notwithstanding the antiquity of the notion that the brain was, in some way or other, the constant associate of the intelligence; notwithstanding this notion received confirmation from the disquisitions of almost every philosopher of the ancient world, and illustration from the labours

of a more practical and experimental age; we hesitate not to affirm that, as a proposition resting upon widely-extended observation, as a principle deduced from the universality of the fact, it is entirely within the domain of phrenology. Anterior to the teaching of Gall, it was most customary to regard the brain merely as the source and centre of nervous influence, distributing the same, through the medium of the nerves, to the rest of the system. We here allude, not to the speculations of metaphysicians, but to the doctrine taught in the schools of anatomy and physiology. Witness the nature of the objections proposed by individuals of no mean repute to the discovery of Gall at its first promulgation. Did not these, in some cases, professedly rest upon certain assumed facts tending to show that the phenomena of mind could be displayed, in all their integrity, in the event of partial or even complete disorganization of the cerebral structure? Is not such a line of argument, moreover, adopted by many at the present day? The truth is, neither metaphysician nor physiologist attempted, in anticipation of Gall, even by hypothesis, to establish a precise and definitive relation between the mind and the brain, after the manner of the leading proposition in phrenology. Mind, in the language of phrenologists, includes within the designation not only the intellectual faculties, but all the affections and sentiments proper to the principle of consciousness—in other words, the conscious principle itself, with all its attributes; whereas, in the hypothesis that had been hazarded relative to the association of mind and brain, the former term was almost universally restricted to the intellectual faculties: but even had this been otherwise, the establishment of any truth by processes agreeable to a rigid philosophy of induction alone constitutes the merit of discovery. Distinction between the functions of sensation and voluntary motion had long suggested the idea of separateness in the subservient nerves, and pathological facts had strengthened the idea; but confirmation by demonstration, and the establishment of the great principle which it tends to develope, belong most assuredly to modern physiology. In a similar view of the case, we must reiterate our decided conviction that the proposition, that the brain is the organ of the mind, forms peculiarly a principle of phrenological science, and that, however general the assent yielded to the same in the present day, even by parties who would disdain to be considered phrenologists, it is not the less a principle strictly phrenological. Gall was the first who interrogated nature, in all her departments, to ascertain the fallacy or soundness of the principle in question; and he was the first successfully to investigate certain facts that had seemed to militate against the proposition, and to show their entire accordance with the general rule. Hence we conceive that whoever admits the function of the brain to be to develope the attributes of the conscious principle is, *pro tanto*, a phrenologist, and a disciple of Gall. In fine, phrenology, as a science, has, in our estimation, established, by the method of induction, the soundness of its first principle, that *the brain is the organ of the mind.*

The next leading proposition in phrenology, which sets forth that separate parts of the brain are instrumental in manifesting the individual faculties of the mind, we hold also to be quite as well supported by the evidence of facts as the great majority of recognized truths in nature. We believe that the extensive observations made by Gall and his disci-

ples, not only upon man, but upon the animal kingdom at large, have clearly made out that, in proportion to the complex and multiform character of the cerebral hemispheres, the species or the individual ranks high in respect of psychical endowment; that in those species where the presence of any structure really analogous to the cerebral hemispheres is wanting, there is no clear or certain manifestation of the principle of consciousness; that in fishes, where evidence of this to a slight extent is noticed, an extremely simple development of brain is discovered; that in birds, where an obvious addition of mental power takes place, a correspondingly more complex character of the cerebral hemispheres is coincident; and, lastly, that in pursuing the anatomy of the brain through the whole class of mammalia up to man himself, a continuous illustration of the same fact is to be found, suggesting the inference, in our estimation most philosophically, that multiform character and complexity of these masses are related, in the way of cause and effect, to variety and extent in the faculties of the mind; and, consequently, the second principle in phrenology becomes fixed, that *the brain forms a congeries of organs, the function of each being to manifest some faculty of the mind.*

The third principle insisted upon by the phrenologists, that which embodies the whole soul of the new doctrine, and that without the satisfactory establishment of which the whole system would lapse into chaos, like an arch on the withdrawal of its key-stone, flows from the universality of the fact, that *size of cerebral apparatus*, cæteris paribus, *constitutes a measure of functional power.* It was the observation of facts illustrating this principle that laid the foundation of phrenological science, and we believe that in the progress of such observation many general truths have been ascertained. We are convinced that any one who will take the trouble to look at what occurs in nature will be satisfied that, under certain conditions detailed at some length in our former article, an ample, well-developed forehead, indicating great size of the anterior convolutions of brain, is the constant associate of high intellectual power; that a broad and elevated sincipital region ever characterizes the heads of individuals eminent for the excellence of their natural dispositions; and that a large mass of brain immediately above and behind the ears shows the possession in a great degree of the qualities communicating to the individual animal energy. Notwithstanding the difficulty of obtaining clear evidence or even illustration of these truths from every case, we are quite certain that if extreme instances be taken—and this is always necessary when the mind has to be convinced of something new—we are quite certain, we say, that the facts in question will be observed to be universally true, and, for this reason, calculated to settle, upon a sure basis, the third principle of phrenology. The same thing will ever obtain, both in comparison of individuals and species, provided certain qualifying circumstances are kept in view, which our present limits will not allow us to discuss, but which are qualifications that enter into the estimate of physiological facts in general, and therefore not rightly to be charged upon phrenology as so many loopholes of escape—a proceeding sometimes indulged in by parties directing only a superficial attention to the subject.

In forming an estimate of the present condition of phrenological sci-

ence, as a system expressive of certain general truths deduced from the consideration of natural phenomena, it might not be out of place here to enter into an examination of the more minute details; to determine, so far as practicable, the particular cerebral organs (to use the ordinary phraseology) which, from the extent and character of the observations, may be regarded as *established;* to specify those the proof of which is yet defective, and which consequently are but to be deemed *probable;* and, lastly, to discuss those which are merely *conjectural.* This, however, would carry us much beyond our allotted space; and we must content ourselves therefore with stating in a few words that, after a fair amount of attention bestowed upon the subject, we are satisfied that, in the case of many of the cerebral organs, it will be found that a conspicuous development of the same will *always*, under given circumstances, be found associated with great vigour in the corresponding faculty or disposition; that in the instance of some others ordinarily assumed in popular works upon phrenology, we believe them to rest upon observations which are either too imperfect in their character or too limited in their amount, yet to rank amongst the truths of science; and lastly, that a great deal which is readily and eagerly embraced by the more enthusiastic and the less thinking of professed phrenologists, deserves, in our opinion, little more consideration than that which is due to so much pure speculation.

Over-zealous and injudicious friends are ever more effective obstacles to the advancement of any new science than its professed opponents. We believe that in nine cases out of ten where truth is rejected, it occurs from misapprehension of the real character of that truth; and when ignorant zealots, in any cause which they afflict with their advocacy, misstate facts and overstrain inferences, they furnish enemies with most powerful weapons of attack and at the same time disgust the impartial and the sober-minded, deterring them from that investigation to which a simple exposition of the real merits of the case might have led them. It has happened so with phrenology. A great proportion of its popular expounders, in lectures and in publications, have presented it to the public as a science that had unravelled every mystery of man's moral nature, and as something that had simplified to the utmost possible extent the nature of the mind's dependence upon organization—as a key to unlock the secret recesses of every heart—and as a system which, whilst it might enable the bad to render it subservient to selfish and evil purposes, would yet furnish the philanthropist with means for the regeneration of society far transcending any that had heretofore been at command. We do not think that we exaggerate in this account of the ridiculous pretensions of many of the *soi-disant* phrenologists. We do not of course refer to the works of the more celebrated and philosophical writers on phrenology, though we have sometimes wished that the labours of the leading cultivators of this science had rested more exclusively in the field of physiology and natural history, thereby following in the track so successfully beaten by Gall, whose exertions we very much think with Dr. Elliotson, brought the new physiology of the brain nearly to its present mark, *as a science of facts.* We should have been glad to have seen it a little more matured and extended in many of its details ere so many of its truly meritorious advocates had drawn so largely upon

it for practical application. To illustrate this, however, is not exactly our present business. It is notorious that many writers and lecturers upon phrenology have taken up the subject, not from any patient and detailed examination of its evidences, but from their fancying it reconcilable with, and confirmatory of, anterior prepossessions, or from the facility it has seemed to afford of exploring hitherto inaccessible regions. This would be a matter concerning themselves only, if it did not happen that the progress of science becomes thereby arrested; adversaries, from ignorance or less creditable reasons, directing their attacks to the subject as propounded by such persons. We have heard and have read much in opposition to phrenology, and yet we can affirm that the phrenology opposed was scarcely ever that of Gall, but usually its miserable caricature exhibited by half-informed enthusiasts.

Popular phrenology has indeed exceeded all reasonable limits of a sound philosophy. When we remember that, in its legitimate pretensions to rank among the sciences, it is essentially a department of physiology—that which explains the functions of the brain—we must greatly regret that its exposition and pursuit have fallen so much into the hands of persons whose accessory knowledge has but little qualified them for the prosecution either of science in general, or physiology in particular. In this state of things, which for the sake of truth cannot be too much lamented, one can hardly feel surprised that this branch of science has been cultivated by many much more in the spirit of speculative advocacy of a favorite set of opinions, than in that of an honest desire to obtain a correct acquaintance with the economy of nature. What do we find upon opening three fourths of the treatises extant upon phrenology? Assuredly, to any one whose mind was new to the question, it would seem, from their perusal, as though phrenology were the most perfect of the sciences, rather than as one professedly in its infancy. We shall see, probably, in such treatises, the brain systematically divided into its thirty-six organs, the functions of which shall be stated with as much confidence, and with the same terseness and abruptness of language, as if there were question of the five external senses; scarcely any distinction being drawn between the established, the probable, and the conjectural; the primitive destination or final cause assigned to each, not in the way of conclusion arrived at from a consideration of all the allied circumstances, but as part of the general demonstration; then we have the doctrine of the temperaments paraded, as if *that* were peculiarly a branch of phrenology, and as if there were some positive knowledge upon the subject—which there is not; and altogether there is in the affair of fact so much error and over-statement, in the detail such confusion of demonstration and hypothesis, that that which should constitute the highest branch of physiology becomes really so degraded as to seem to merit exclusion entirely from the domain of science.

If in the representation of the actual facts of phrenology there is so much to reprehend, assuredly the matter stands worse still in the attempt at obtaining conclusions. Any suggestion or conjectural statement at any time hazarded by some leading phrenologist is, to serve the immediate purpose, referred to and employed as so much positive demonstration. Whatever be the favorite bias of thought of the advocate is sure to receive its strongest proof from the science at present under discussion.

Materialism and spiritualism, fatalism and moral liberty, revealed religion and pure theism are all in turns established incontrovertibly by popular phrenology. Thus, the philosophical phrenologist, in discussing the nature of man, aided by the new physiology of the brain, shall occasionally give emphasis to his words by employing the phraseology of the sacred volume, when immediately will arise some of your petty phrenologists, and contend that religion and its ministers may henceforth be dispensed with, because phrenology teaches the same things as the really excellent parts of scripture, and much more clearly; as if the reasonings of phrenologists, aided by the help of revelation, were something inherent in phrenology, and something deduced solely therefrom. The same process obtains with respect to medicine, politics, legislation, and education. Because useful hints and, in some instances, even new information may be gathered from the subject bearing upon some of these matters, therefore all that is advanced or enforced by the collateral aid of phrenology is phrenology itself! Any one who has paid much attention to the sayings and the doings of the vulgar herd of those whom Gall would have disdained to own as his disciples, will recognize a faithful representation in what we have here advanced. As expressed in our former article upon this subject, we are confident that, in the prevention, discrimination, and treatment of insanity and nervous diseases, in education and also in legislation, in the prevention of crime and the treatment of criminals, phrenology will, when freed from many popular errors, furnish very useful aids and suggestions; but to expect that a perfect revolution in dealing with all these things is, from such source, to be at once accomplished, is verily but the day-dream of the enthusiast, or the shallow pretence of the charlatan. We, for our own part, coincide very much with the sentiments perspicuously expressed by Dr. Cowan, of Reading, in a paper published in a recent volume of the Transactions of the Provincial Medical and Surgical Association, viz. that phrenology, as such, "does not involve that addition to our *real* knowledge of the nature and laws of mental and moral phenomena which some able writers seem to imply, since it must be admitted that much general knowledge had been previously acquired," and which general knowledge phrenology does not by any means supersede.

In reviewing the circumstances which have tended to lower phrenology in the estimation of scientific men, and consequently to retard both its progress as a science, and the general recognition of its leading truths, we should but very imperfectly perform our task if we did not refer, in the strongest possible terms of reproof and condemnation to the too-prevalent proceeding of examining living heads in minute detail and indiscriminately, and supplying the owners with an account of the "development," often on the receipt of a fee, varying in amount as there is furnished or omitted a general deduction as to the character and probable conduct of the individual, with or without the "philosophy," according to the phraseology of practitioners of this art. The attempted justification of this course rests upon the plea, that evidence of the truth of phrenology becomes thereby afforded, and that the most useful of knowledge—self-knowledge—is, at the same time, gained to the individual. What, however, is the real state of the case? Why, we unhesitatingly maintain, that the science is not sufficiently advanced to supply evidence of its

truth from every head, or from any one head; and consequently that such practice, as a general one, is so much pure charlatanism. Where any strongly marked peculiarity of individual character exists, its outward sign, in appropriate subjects, will certainly be detected; but, from the very nature of the thing, these cases must constitute, not the rule, but the exception. The practice we condemn, however, makes no distinction of instances. Injudicious zeal—the common ally of ignorance, a wish for effect, not unfrequently more sordid motives, stimulate the self-styled phrenologist in this empirical career; and, as a matter of course, the errors and mistakes perpetually made are constantly appealed to as indicative of the sandy foundations of the entire phrenological edifice. We write advisedly in this our unqualified reprobation of the popular custom of "taking developments." We believe it to be an extension of the practical application of phrenology much beyond its legitimate bounds; and we appeal to any one having acquaintance with its results, whether anything like uniformity—the true test of accuracy—is obtained in the majority of cases, even when the most experienced and dexterous pronounce their judgment, if their explorations be conducted separately. We ourselves have even witnessed the greatest possible discrepancies. Nay, we have seen the *same* phrenologist furnish one character from the head and a totally different one from the cast, whilst in ignorance of the original of this latter. This we have known to happen, not merely in the practice of one of your shilling-a-head itinerants, but in that of one not unknown to fame in the annals of the science. It may be contended, that mistakes on the part of the manipulator no more indicate the inaptitude of phrenology for the objects with which "taking developments" is practised, than do those of the chemist or the astronomer show the unfitness of their respective sciences for exhibiting certain results. This objection at first sight seems plausible, but a closer examination will show its futility. It must be borne in mind that here is question, not of erroneous or premature inferences to which an experimental research into natural phenomena may sometimes conduct even the most philosophical enquirer, but of the legitimate and authorized application of a science as it stands; and in this view of the matter we may assert, without any fear of contradiction, that mistakes occur, as the exceptions, in the progress of *illustrative* experiment in such sciences as chemistry and astronomy, at least in the hands of persons eminent in their respective departments, and are, moreover, easily rectified so as to prevent their recurrence; but in the case of *indiscriminate manipulation of the living head for the purpose of exhibiting evidence of the truth of phrenology*, blunders, and gross ones too, are perpetually arising, and that in the practice of every one, no matter how experienced. We do not speak rashly in our present assertion. We have seen many certificates of character from phrenological artists, with which the parties for whom they were drawn out have been quite delighted, and which have constituted the proximate cause of their conversion to the true phrenological faith, but which have ever seemed to us merely as a sagacious dwelling upon two or three actually possessed and really indicated peculiarities, mixed with a vast amount of generality that would apply to almost any case, and the whole dished up with a seasonable amount of flattery; which latter ingredient has commonly furnished the relish for the whole affair, ultra phrenology

into the bargain. It is degrading to an important—we believe the most important department—of physiology to solicit or obtain *such* converts by *such* means. Non tali auxilio, &c.

We regard, then, the unscientific character of a large proportion of those who have devoted themselves to the study and the exposition of phrenology, the over-statements and the mis-statements as to its ascertained facts, the extravagant inferences therefrom, and the confounding of these latter with the positive demonstrations of the science, and, above all, the empirical procedure in the matter of taking developments, as constituting the principal causes that have interfered with its cultivation as a branch of physiology, and retarded its progress to recognition on the part of those whose suffrages it is always desirable to obtain in that stage of a new science wherein its conclusions seem most to oppose themselves to anterior knowledge or prepossessions. To a certain extent, we believe that all this was inevitable from the very nature of the subject. Phrenology, at first view, addresses itself too much to that spirit of wondrous inquisitiveness prevalent in little minds not to have attracted the attention, and excited the interest, of the sciolist and the vain-glorious; and, as a natural consequence, we have had the misappreciation and the misapplication of the matter, such as we have just detailed. Let, however, the really profound, the *true* phrenologists, shake off the shallow and contemptible allies with whom they have been too long encumbered, and too much identified; let them disclaim their superficial unscientific modes of investigation; and let them pursue their own continued researches as much as possible in the spirit that actuated Gall.

Before offering any suggestions as to the way in which we believe it to be desirable to prosecute future investigations into the physiology of the brain, we would just point out in what way it has happened, in our opinion, that, except in diffusion and illustration, so little has been done, since the period at which the great discoverer closed his active and successful career. We attribute this circumstance in some respects to the premature introduction of speculative reasoning, by the earlier disciples of Gall, into the exposition of that which had been really ascertained. *Deduction* from principles already obtained, prematurely interfered with the *induction* of new truths; the former process being, at all times, the more captivating to great numbers, from its comparative facility. Thus, notwithstanding the credit that really attaches to the late Dr. Spurzheim for his share in the prosecution and exposition of the new physiology of the brain, it must be granted that his labours, after separating from Gall, rested but little in the field of active observation, except for the purposes of confirmation and illustration. We are aware that several important additions to the positive facts of phrenology are usually considered to have been made by Spurzheim. These, as a whole, we have long thought have too readily been admitted. We cannot go into detail, but, with the exception of one or two instances, we, for our part, regard these "additions" only as falling within the limits of the probable. Our present object, however, is to refer to the *method* pursued by Spurzheim, which we consider to have operated more or less disadvantageously upon the labours of his successors. From all that we have been able to gather from the published accounts of the assumed discoveries, made by the distinguished colleague of Gall, it has always appeared to us that nature

was appealed to, or interrogated, not so much with the design of educing a general truth from the aggregate of the facts observed, as for the purpose of gaining support for a preconceived hypothesis. If we take the organ of "conscientiousness," a sentiment which Gall identified with that of "benevolence," *reasoning* led Spurzheim to dissociate the former from the latter feeling, and to infer the existence of a separate division of the brain as its material instrument, and an appeal to a very few *recorded* facts was made to obtain a realization of the idea. Our own conviction is that, in this case, the *anticipation* of nature, to use the phraseology of Bacon, suggested the right *interpretation;* and we quarrel not with the result, nor yet with the method, in the individual instance, but we refer to it as having operated prejudicially in the way of example; for, of late years, whenever there has been an assumption of discovery in phrenology we have generally noticed that the first suggestion had originated much more in speculative reasoning than in observation, and that cases in corroboration were most commonly few in number, and *selected;* whereas, in the proceeding of Gall, whom, in the language of the late Mr. Chenevix, Bacon might have rocked in the cradle, facts in large amount, *indiscriminately obtained*, were constantly amassed ere he would entertain a suggestion; and only after observations had accumulated extensively, would he venture to pronounce that any proposition was made out. He himself informs us that, on some occasions, after his collected facts had for years pointed to a particular conclusion, the occurrence of one certain unequivocal exception ever induced him to start anew. It is in this spirit, in that of a true inductive philosophy, that we would see future researches into the functions of the brain pursued. We think that physiologists and natural historians would do well to leave for a while the examination of the fundamental powers and special destination of the particular faculties, and somewhat more actively and generally resume the comparison of the *actions* of man and animals with their respective organizations. After all, it is only the results obtained in this way that can ever command general assent: the expression of the universal fact can alone form an axiom not to be shaken. Whatever is done in the way of defining and explaining the primitive intention, or design, of the mental powers and affections, can impress the mind only, at best, as a highly probable conclusion, never as an absolute demonstration. These constitute very appropriate themes for the mental or moral philosopher who, in the prosecution of his studies, thinks it well to draw upon phrenology; our business at present with this latter is, however, chiefly as *a branch of physiology;* and it is to the subject regarded in this point of view that our present remarks are intended to apply.

We have often thought that that which has sometimes been attributed to Gall as his greatest mistake was, in point of fact, in the infancy of his investigations, a leading merit. We allude to his designation of many of the cerebral organs, according to actions that had been *motived* (if we may borrow from our Gallic neighbours) by irregular impulses, rather than according to the legitimate direction of the faculties. To have done this latter, in the early stage of his labours, would necessarily have partaken more or less of the spirit of the system, a plan irreconcilably at variance with the successful search after a new truth. Gall's course was to notice great developments of parts of the brain in combination with

energy of function, as manifested by actions; and, constituted as human nature is, this was of course pursued best, in many cases, in the instances of great abuse; and, by adopting a nomenclature that simply expressed the general fact observed, he avoided that very common and almost inevitable source of error, premature explanation. The proceeding was perfectly philosophical, and had the advantage not only of keeping his disciples within the limits of demonstration, but of pointing out, at the same time, the only method by which the subject could be successfully followed out by those who should come after him.

The foregoing is the only method, we firmly believe, whereby phrenology, as the physiology of the brain, can be advanced or perfected. Extensive observations yet require to be made, in order to establish the precise set of actions, or mental manifestations, constantly associated with great development of several of the organs, usually represented upon the marked bust as though they were quite ascertained. Respecting these, we have as yet much disagreement amongst professed phrenologists, as to the nature of the primitive function they are intended to perform. Would it not be well if it were determined, first, what are the outward efforts, or displays, *invariably* associated with the given development? As an example, let us adduce the case of that which has been most commonly designated the organ of wit: suppose that some phrenologist should tax himself with the discovery of its exact function, we conceive that it would be well, instead of indulging in metaphysical refinement by the aid of a few *selected* facts, to collect records of the mental peculiarity of *every* individual encountered who was characterized by the development in question; and, when the accumulation had become considerable, to examine the records in order to see what was common to every fact in the series, and the result might go far to suggest the special manifestation of the organ in question. If this proceeding were followed up by obtaining *indiscriminately* negative instances in large number, and if it were found that in *all* cases of deficiency in organic size an absence was detected of that which had distinguished the positive instances, the proof we imagine would then be tolerably complete.

We are of opinion that, in phrenology, observations lose much of their value, as suggesting anything new, when they have been made by different individuals, one having taken the idea from another, and a few only having been contributed by each; because there are circumstances, in such observation, which render it very difficult to examine their value as *comparable* facts: whereas the case is different with an extensive amount collected by one *good* observer, as here the instances all possess the recommendation of having been estimated by the same mind, of having been measured, so to speak, with the same instruments; and, in this case, they afford a reasonable presumption of constituting properly comparable examples.

The proceeding which we have endeavoured to point out, and to illustrate, would simply form the counterpart of the labours of that one observer whose results were so signally successful; it would merely be an application of the method of induction. Whatever, as fact, is imperfectly proved in the present state of phrenology should be submitted first to the test of this method; and, on the attainment of any inference, facts in natural history, pathology, and practical medicine, might all be appealed

to for confirmation; but, for the actual discovery, whatever should form the occasion of the first *suggestion*, assuredly the *proof* must ever be established in this way.

In cursorily reviewing the existing state of phrenological science, the causes of its too common neglect amongst scientific men, and the mode in which we believe its study should be hereafter prosecuted in order that it should be advanced and perfected as a branch of physiology, there are many circumstances and details into which our present limits will not allow us to enter; otherwise, there are some matters relating to all these points upon which we might have wished to dilate. We might have extended our remarks, somewhat, in referring more particularly to those parts of the brain whose function we considered to be pretty well made out; we might have *named* the organs, so called, in our belief established; those but probable and why; and those which we regarded as purely conjectural and the suggestions of a speculative spirit. We should have wished to notice more at length the custom of presenting a theory of the temperaments, as furnishing signs for the appreciation of the *quality* of cerebral structure, almost as accurately as prominence in the various regions of the head gives the clue to the detection of *size* of particular parts—a course pursued in the most confident and dogmatical manner by the vulgar herd of phrenologists. We should like to have spoken also upon the proposition, which some think they have demonstrated by experiment, that an alteration in the form of the head, caused by increased development of the particular organs, can be induced, even in adult age, by a *voluntary* action of the individual upon his own mental operations. These topics, however, we must omit, simply expressing our wish that, for the sake of what is really true in phrenology, enthusiasts and zealots would not press prematurely into the service what only rests upon data confessedly imperfect and, we believe, in great part erroneous.

Having long felt dissatisfied with much that we conceived to be unscientifically based in *popular* phrenology, we confess that it was with some feelings of interest and curiosity that we took up a work professing to teach the "principles of a *new* and scientifically-based cranioscopy"—the pretension with which the author, M. Carus, in his title-page, offers his system to the world. Before giving our readers any account of the contents of this work, we cannot refrain from expressing our surprise and regret that a man possessing a good reputation as a human and comparative anatomist, like M. Carus, should have identified his name with a series of propositions so thoroughly unscientifically based as those contained in the present production. We have here an additional fact, showing how wretchedly an individual may *reason*, who shall yet *know* a great many things; how an extensive acquaintance with details does not necessarily lead to right dealing with principles. M. Carus is, we believe, considered by some and, we suspect, especially by himself, as possessing a genius that is universal; and in his case we have an illustration of how incompatible, except in some rare instances, such an order of mind is with accuracy and precision of thought.

The new cranioscopy, inculcated in the little volume before us, rests essentially upon a threefold division of the contents of the cranial cavity—into the cerebral hemispheres, the corpora quadrigemina, and the cerebellum. These he speaks of as the anterior, the middle, and the posterior

cerebral mass; to these three divisions he regards what he calls the three cranial vertebræ—the frontal, parietal, and occipital bones—as corresponding. Certain analogies in the lower tribes of animals have guided him in the formation of this view of the matter. Pursuing the subject, he places the intelligence in the cerebral hemispheres, the sensibility (Gemeingefühl) in the corpora quadrigemina, and the will, desire, and instinct of generation, in the cerebellum; and he tells us that herein "lies the key to all cranioscopy that is true and supported by physiological principles." His ideas upon the cranioscopy or phrenology which recognizes Gall as its discoverer appear to have been taken from some of the humblest and least informed of the modern phrenological school, or, what is worse, from the representations of some scoffing opponent, as he goes on to observe that "most other relations specified by Gall and his successors, and especially the presumed relation of individual moral qualities to certain bony prominences are thoroughly illogical, unphysiological, and untenable. It is just to these hypotheses, however, that the multitude have the most resolutely clung. It was hoped that, in such representations, a means was to be found for ascertaining whether any particular individual were good, benevolent, religious, or imaginative, whether he were quarrelsome, cruel or thievishly disposed, and so on; in the cases of children, that one might be able so to grope out their special talents and inward vocation as to regulate their education accordingly. All that sort of thing has its proper place among dreams and imaginative conceits." This extract from the early pages of the work under review will form a fair specimen of the style and spirit of the whole.

We shall not engage the time and the attention of our readers with anything like a detailed analysis of M. Carus's scientific cranioscopy. We shall content ourselves with pointing out some few of its gross inconsistencies, and its utter variance with all sound physiological demonstration or reasoning. We presume that when our author speaks of that which he calls "scientific" he does not propose to institute a new system of philosophy as well as of cranioscopy, but that he recognizes the validity of the Baconian system, as applicable to the investigation of the economy of nature. As then, in the progress of his work, he distinctly acknowledges the influence of organic size in the communication of functional energy, we should like to ask him whether it be a constant and universal fact that, according to the development of the cerebral hemispheres, the "intelligence" of the individual is exalted; that, to take his own nomenclature in specifying the faculties appertaining to the intelligence, Conception (Vorstellen), Perception (Erkennen), and Imagination (Einbildung), are in contemporaneous strength and activity, as the brain proper, being healthy, is large or small? A very few instances, taken indiscriminately, would disprove such a position at once. If we proceed to the next conclusion deduced by M. Carus, that feeling is associated with the corpora quadrigemina, where is the proof that energy or intensity of this characteristic bears any such relation? A few vague analogies, admitting of various interpretations, collected from the animal kingdom, assuredly constitute no "scientific" basis for any physiological proposition. Again, in regarding the cerebellum as the organ of the Will and Desire, (Wollen, Begehren,) as well as of the sexual instinct, where is the shadow of proof physical or metaphysical? Have men strong determination and reso-

luteness of purpose, in the general sense, *always* in relation to the development of the cerebellum? Where is the evidence that desire, the *felt requirement* of any individual, apart from what relates to sex, is associated specially with this structure, or coincident in any way with the strength of the will? We shall see, by and by, the proofs which our transcendental philosopher adduces. Meanwhile, let us ask, could a more unsatisfactory series of propositions have been brought forward? Could a more meager, inconclusive analysis of the psychological principle have been offered?

We proceed now to state the mode in which these notions are supported. We are told then that a large forehead, for the proper estimate of which minute directions are given, indicates high intelligence, and an appeal is made to examples for corroboration of the assertion. Here we are very much agreed with M. Carus; but we utterly deny that size of the frontal bone furnishes any measure of the magnitude of the cerebral hemispheres in the aggregate, which, he however maintains, constitute the organ purely of the intelligence. We believe that we should be wantonly consuming the time and the patience of our readers were we to commence a formal confutation of so absurd a conception. Next, after having been given to understand that Feeling, in what precise sense our author very imperfectly defines, resides in the corpora quadrigemina, and that these are developed according to the "dominance of the vegetative life, and of the individual feelings, without enlightenment by knowledge and without force of will;" we are directed to ascertain the magnitude of the region inclosed by the parietal bones in order to ascertain the preponderance of the qualities allocated in the "middle cerebral mass," as he calls the corpora quadrigemina. The middle region of the brain is certainly in excess over the anterior and posterior regions, when the fulness and superficial extent of the bones by which it is covered are proportionately great, but it is the middle lobes of the hemispheres, and not the twin bodies, that fashion this portion of the outer head. It would be pure supererogation to go into any defence of our assertion.—Power of will and strength of desire are to be inferred when a large occipital region is discovered, according to M. Carus, as if the magnitude of the bone inclosing this portion of the brain, furnished a rule for estimating the size of the cerebellum, where, according to our author, these qualities have their habitation.

The above paragraph furnishes the groundwork and outline of the new theory proposed by M. Carus; and, in the little volume before us, it is wrought out by a number of illustrations and extensions of its application, to go into which would be thoroughly useless. We have given the present slight sketch, neither from believing it capable of yielding any useful information nor from a desire to amuse the reader, but from the honest wish to warn the *orthodox* school of phrenologists, so to speak, from attempting premature conclusions and from building upon vague analogies; since it is apparent that such a method, once in vogue, may lead to the formation of another and a new phrenology, which, however, in the present instance, we regard as too transparently at variance with the plainest facts to have any chance of creating a school of *new* phrenologists. Still a more plausible scheme, one a little more consistent, might share a luckier fate; and would it not become an exhibition truly de-

plorable to behold rival systems vieing with each other in contention for disciples? And, indeed, if orthodox phrenology is to be disguised and disfigured by half-proved statements, empty conjecture, and foolish reasoning, we see not why a new phrenology should not constitute a formidable competitor. It would be, to a great extent, hypothesis and speculation in opposition to hypothesis and speculation, and fancied resemblances would determine the decision.

A disposition to argue from analogy, which too often leads to a neglect of the steady inductive mode of reasoning, we believe to be at the foundation of most of the errors that arise in science. Too great an inclination to deduce an inference from facts not standing in direct relation to each other can alone explain how it comes to pass that a man of M. Carus's extensive acquirements and experience, can have imagined for a moment that his own crude and most inconsistent ideas were either to supersede the philosophical results of Gall's labours, or, in opposition to them, to be regarded as a system "scientifically based." Further, we are sure, from the whole style of the work, that the author has no correct acquaintance with phrenology, but that his notions respecting the same are obtained, either from the representations of some inimical scoffer or from the readily-perused account rendered of the same by some half-informed and shallow zealot. In either case it is most unworthy of any man of science to attack the discoveries and to deride the philosophy of others, himself being in ignorance of what these discoveries truly consist, and of what this philosophy is; and more particularly when this is done in a work heralded by a high-sounding and loftily-pretending title, and which work shall not contain a single original observation, and the philosophy of which shall be made up entirely of speculation.

We must, however, take our leave of M. Carus; and, in doing so, we must express our conviction that the department of comparative anatomy is but little calculated to afford the means of *discovery* in phrenology. In the case of all physiological discoveries, function has not been revealed by dissection of parts, nor by careful study of their forms, dimensions, or their analogies. This has always been accomplished by noting the phenomena of life and the conditions under which they have been displayed; and then anatomical facts have been investigated in order to illustrate still further and render more clear the information obtained by the observation of the living action of organs. When anatomy is appealed to for the *proof* of any assumed novelty in physiology, it will ever fail, though it may occasionally supply the first hint. If this be true of anatomy and physiology in general, assuredly the matter is not altered when there is question of cerebral physiology and comparative anatomy. If notions are to be taken up resting only on certain real or supposed analogies existing amongst various tribes of the animal kingdom, and to be regarded as "scientifically based," we do not see why there may not be as many systems of physiology as there are different students, for the perception of and inference from analogy will ever vary amongst individuals according to their anterior knowledge and natural cast of thought. A series of facts will seem related to one set of ideas in the mind of one individual, and will present a comparative aspect totally different to another whose stock of information shall be differently arranged. It is direct relation alone, not that of resemblance among facts, that can establish

any proposition bearing upon the economy of nature. Facts gathered from various sources, having certain general resemblances, do admirably well for the purpose of strengthening and elucidating any proposition, but when they are intended to serve for proof they should all present the same relation to the expected conclusion. So with phrenology. When the intention is to determine "scientifically" the function of any portion of the human cerebral mass, human heads should alone, in the first instance, supply the material for observation, and that which is noted invariably, under a given set of circumstances, may safely be treated as discovery. Then, if no error have taken place in conducting the observations, it will be certain to receive strength, and support, and elucidation from everything that natural history or comparative anatomy can afford. If it do not, the appeal to these sources of information will disprove the accuracy of the foregoing observations, and will lead to their rectification.

It is surprising to what erroneous conclusions a fault in method will lead even the most talented and the best informed. The mind under such circumstances, is with difficulty turned aside from the anticipated result, even on the presentation of the most glaring evidence. Who has forgotten the striking illustration afforded of this remark by Tiedemann who, some three or four years ago, took upon himself to prove that the negro race was not behind the European nations in general capacity or educability? From reflecting upon some vague analogies he had adopted the opinion that human perfectibility was in relation to the capacity of the cranial cavity. In order to determine accurately to what this capacity might amount he took a certain number of crania belonging to the diff rent races and filled them with millet seed, and this being weighed furnished of course a true index of the relative size of each head. The entire assumption was wrong; it had been made without an adequate notation of relation between facts that were identical; and, having started badly, error accumulated itself upon error, and Tiedemann himself actually published a result that was at variance with his own figures. This was, at the time, well and clearly exhibited in the Phrenological Journal, by Dr. A. Combe, the substance of whose little memoir upon the subject was afterwards transferred to the pages of our own journal.

For hints respecting the various modes in which phrenology may be practically applied, other departments of human enquiry may very advantageously be consulted. The illustration of many of its facts and principles will be greatly aided by an appeal to other sources of information, and more particularly to general physiology and other allied branches of science. What has to be guarded against is the liability to mistake an elucidation of that which is already proved, for the just process of establishing some new conception; a proceeding frequently adopted, especially amongst merely literary phrenologists.

Highly as we estimate the discovery of Gall, immense as we regard the advantages which may be ultimately derived from phrenology, we confess that we wish to see it *less* regarded, studied, and pursued as a separate science, and *more* as a branch of general physiology. We are aware that the physiology of the brain is so inseparably identified with metaphysics as necessarily to constitute, under all circumstances, a special department; still, however, a certain distinctness might be maintained between

the more strictly physiological and the metaphysical considerations of phrenology, which could but operate beneficially upon its future cultivation. A close attention to habits and pursuits, in association with specialities in the organization of brain, might go on, and the attempt to pursue these to their remote consequences be left to other hands. This latter is the business of the mental and moral philosopher. Gall might form the model of the former proceeding, whose method of dealing with the subject admits of positive verification or rectification by any one possessing the required accessory knowledge and appropriate opportunities; and a work like Mr. Combe's "Constitution of Man," might equally serve as an example for those who wished to illustrate, confirm or establish their particular views of human nature, by notions obtained from reflecting upon the physiology of the brain. Concerning the actual facts of phrenology, and its general axioms raised by the method of *induction*, there may ultimately, to a great extent, be concord and unanimity amongst its votaries. The *deductions* from these, on the part of individual phrenologists, will ever vary, according to their general views upon subjects in the discussion of which they apply its aids.

Art. VI.

Versuch einer Geschichte der Geburtshülfe. Von Dr. E. C. J. Von Siebold, &c. Erster Band.—*Berlin*, 1839. 8vo, pp. 384.

An Essay towards a History of Midwifery. By Dr. E. C. J. Von Siebold, &c. Vol. I.—*Berlin*, 1839.

The brief and imperfect sketches of the history of the obstetric art, furnished by Smellie, Leake, and Denman, continue to be the only records in our language of the progress of this branch of medicine. The subject has attracted much more attention in France, to which country we are indebted for several historical sketches of considerable value, though they are all inferior to a work which was published at Göttingen, at the close of last century, by the famous Osiander, who then occupied the midwifery chair in that university. This "History of Midwifery," which appeared in the year 1799, contains a store of materials, which has been much resorted to down to the present time. Its merits are very great, but from them there is one very serious drawback, namely, the asperity and unfairness with which the author criticises the labours of all whose opinions are at variance with his own. M. von Siebold, with more than equal learning and research, has brought to the task a much sounder and more impartial judgment and better taste, while he is altogether free from that spirit of anecdote-mongering in which his garrulous predecessor so largely indulged.

The history of the subject is divided, by Von Siebold, into three periods: the ancient, the middle, and the modern. The first extends from the earliest times to the cultivation of medicine by the Arabs, about the end of the seventh century; the second reaches to the end of the seventeenth century, an epoch marked by the invention of the forceps, and the publication of the Novum Lumen of Deventer; and the third brings us down to the present day. Each of these periods is distinguished by a peculiar character. In ancient times, the practice of midwifery was exclusively in

the hands of females, the physician being called in only in consultation, and chiefly in cases where benefit was anticipated from pharmaceutical measures. Operative midwifery was then characterized by disregard of the life of the child; and perforation and embryotomy were almost the only operations to which recourse was had. In the middle period, midwifery was regarded merely as a branch of surgery; hence its practice was purely mechanical. The last period is distinguished by the employment of the forceps, by great improvements in the whole of operative midwifery, by a juster appreciation of the powers of nature, and of the circumstances calling for the interference of art. M. von Siebold further subdivides each of these periods by a judicious selection of epochs, the first into three, the second into four, the last into two eras. We will hastily review the characteristic features of the first five of these periods, which are treated of in the present volume; the remaining four, which embrace the history of midwifery since the year 1532, will form the subject of the second volume.

First period. From the earliest times to Hippocrates, or to the end of the fifth century before the christian era.

After a few preliminary remarks on the difficulties of investigation, into the origin of any of the arts or sciences, M. von Siebold observes that midwifery in its simplest form must have existed long before any attempts were made to heal diseases. In most uncivilized nations, and even in many parts of the east, some female relative attends upon a woman when in labour. This was doubtless the practice in early times, and long after the attendance upon women in labour had become a distinct occupation, the office was undertaken by none but females. The bible contains frequent mention of midwives, but says nothing which could lead to the supposition that the interference of men was permitted under any circumstances. Nor is there any evidence to show, as Osiander imagined, that the Egyptian priests ever attended upon women in labour, while the condition of medicine among the Egyptians, and their known ignorance of surgery, preclude such a supposition. Further, the mythology of Egypt and Greece tends to show that women were the sole practitioners of midwifery in those countries, since the deities who presided over pregnancy and childbirth were all females. The two principal of these goddesses were Isis and Eileithyia; the former was implored by Egyptian women, the latter by those of Greece, to preserve them during labour, and to send them a safe and speedy delivery. The actual assistance rendered to women during parturition was small, and probably did not exceed placing the patient in a convenient position, receiving the child when born, or extracting it if not quite expelled, separating and securing the navel string, and perhaps withdrawing the placenta by traction at the umbilical cord. (pp. 1-69.)

Second period. From Hippocrates to the decline of science after Galen, or to the commencement of the third century after the christian era.

The age of fable and tradition past, we come to the first written rules for the practice of the obstetric art. The observations relating to the subject in the true Hippocratic books are but very few, and show that women had the practice of midwifery entirely in their hands, and that the help of the physician was sought only in some extraordinary difficulty.

He could, however, render but little assistance, since he was deprived of every opportunity for watching the process of natural labour. This is fully proved by various passages in the spurious Hippocratic books which treat much more fully on midwifery than those which are attributed to Hippocrates himself. All feet presentations were regarded as extremely hazardous; and whenever the child was living, attempts were made to bring down the head, since in none but head presentations was delivery supposed to be practicable. The manner in which this practice is insisted on in all the oldest medical writers, shows that it must have long existed, and that it was in all probability one of the most ancient modes of manual interference. When the child was thought to be dead perforation or embryotomy was at once resorted to, and the impossibility of bringing down the head in many cases must have occasioned the frequent destruction of infants whose life might have been preserved. The accuracy with which many of the diseases of women are described in the Hippocratic writings, makes it the more to be regretted that their authors had such scanty opportunities for cultivating practical midwifery.

Many of Aristotle's observations on the physiology of pregnancy and labour are very valuable, especially his researches into these processes in animals. His labours, however, excited no influence on the practice of midwifery, which still continued exclusively in the hands of women.

The condition of the art among the Romans was by no means better than among the Greeks. The social state of a people engaged, as the Romans were, in almost constant war, affords but little encouragement to the cultivation of medicine. Accordingly, the Romans, during the republic, possessed but little knowledge of the art beyond the elements of a rude surgery, while they endeavoured to make up for their ignorance by the grossest superstition. Gods almost numberless presided over pregnancy, labour, and the education of children. Lucina was implored under various names, and worshipped, now as Juno, now as Diana. Prosa and Postverta superintended childbirth; the former when its course was natural, the latter when the presentation was unfavorable. The Dii Nixii and the goddess Numeria strengthened the throes of labour, and hastened delivery; Pilumnus, Intercidona, and Deverra watched over the safety of the woman and her new-born babe; Carna protected children in the cradle; Rumina presided over the process of suckling; Educa and Potina took care of their food in general; Cunina watched by the cradle; Ossipaga moulded the bones; Vaticanus waited on the cries of children; and Fabulinus helped them in their first attemps to speak. But we will cease enumerating these gods, the long catalogue of whom is not nearly exhausted. Women were the sole practitioners of midwifery in Rome, and were either wholly ignorant, or in latter ages derived a scanty instruction from Greek physicians or midwives who settled in Italy. Some advances had nevertheless been made since the time of Hippocrates, for we find Celsus advocating version by the feet, a practice, which, if it had been followed, would have saved the lives of many mothers and infants who fell victims to the Hippocratic mode of attempting to bring down the head to the brim of the pelvis. Celsus indeed did not recommend the practice with a view of saving the child, for we find that in the cases to which he refers, the child was supposed to be dead and the preservation of the mother's life was the only point which claimed attention.

Between Celsus and Galen, the names of many pass under review, almost all of whom contributed their quota to the advance of the theory of the art, though they did but little towards improving its practice. Galen indeed, who enriched theoretical midwifery more than any of his predecessors, did almost nothing for its practice, in which he is inferior to Celsus or Hippocrates. With Galen, this period terminates, and we come to a third which offers but little worth recording. (pp. 70-179.)

Third period. This extends from the decline of science to the cultivation of medicine by the Arabs.

This was a time of dark superstition, when faith in charms and amulets and magic prevailed universally among the vulgar, and a blind copying of Galen stood in the stead of scientific investigation. Midwifery continued, as might be expected, the undisputed field of ignorance and charlatanry. The help of the physician was sought only in desperate cases, when operations were performed which always destroyed the child and frequently endangered the life of the mother. One cause of the slight value attached to the life of the child was doubtless the opinion that, while in the womb, it is not endowed with a soul; against which notion and the consequent practice Tertullian loudly exclaimed.

Ætius of Amida was the best writer of the day; a diligent compiler who borrowed from many whose names only are known to us, and among others from Philumenos. One great merit of Philumenos was his recommendation of the operation of turning, which in after years fell quite into disuse till its employment was revived by Ambrose Paré. The rules given by Philumenos for the management of the placenta, are likewise extremely judicious, and most of them are adhered to in the practice of the present day. The last person of any note in this period was Paul, of Ægina, whose investigations into the diseases of women are very valuable. In matters of practical midwifery, however, and especially as respects the operation of turning, we find that opinion had retrograded during the century which had elapsed since Ætius. (pp. 181-240.)

Fourth period. This is that in which the cultivation of medicine was confined to the Arabs; an age in which the advance made was small, though time was occupied, not altogether uselessly, in examining and arranging the labours of by-gone years. The barbarous operations of midwifery were still retained, but attempts were made to lay down laws for their employment. The first author during this period was Serapion, in whose practice we find full proof of the decline of midwifery since the days of Ætius. He forbids the extraction of the child by the feet, and recommends in different labours that the feet should be amputated, with the view of pushing back the child, and then bringing down the head to the brim of the pelvis. Rhazes, who lived about half a century later, notwithstanding his great reputation, has scarcely in any respect improved upon the maxims of his predecessor. One point which cannot escape us in reading the works of those days is the utter ignorance which existed respecting the pelvis. A difficult labour was supposed to be often produced by a small uterus or a narrow vagina, but no conception was formed of hinderances to parturition arising from the state of the bony parts. Instances of this opinion are adduced from Serapion and Ali Abbas. Avicenna is the best specimen we could find of the spirit of the age, and the obstetric remarks in his canon of medicine contain a strange

mixture of good and evil, and show that the bloody and brutal mutilations of the child was still in common practice. For a knowledge of many of the obstetric instruments of those days we are indebted to Abulkasem, who wrote a treatise on midwifery, and illustrated it with drawings.

From the time of Abulkasem to the commencement of the fifth period we meet with few names, and none of them those of men who did much for the improvement of obstetricy. (pp. 241-302.)

Fifth period. The post-Arabian era is made by Von Siebold to extend to the publication by Eucharius Roesslin, of the first printed book on midwifery. It was a time of advancement for medical science generally, but illustrated by few men who devoted themselves to obstetric subjects. The circumstance of medicine being in the hands of the monks necessarily confined the practice of midwifery still to women. The revival of anatomical study, however, overthrew many fallacies, and paved the way for the improvements which commenced with the beginning of the sixteenth century. (pp. 303-360.)

We look anxiously for the appearance of the second volume, which is to introduce us to these better days; but, in the mean while, our readers will thank us for having called their attention to the book. It contains much information of general interest; and to any who are engaged in teaching midwifery it is absolutely indispensable. Its possession will spare them many an hour that they might otherwise spend in turning over the pages of old and forgotten tomes, from which, after all their labour, they would not obtain nearly so much information respecting the midwifery of the ancients as the elegant work of Professor von Siebold will convey to them in half an hour.

Art. VII.

Nosologish-therapeutische Untersuchungen über die brandige Zerstörung durch Behinderung der Circulation des Blutes. Von Carl F. F. Hecker.—*Stuttgart*, 1841. 8vo, pp. 71.

Nosological and Therapeutical Researches on Gangrene from Obstruction to the Circulation of the Blood. By F. F. Hecker.—*Stuttgart*, 1841.

We have, in the work before us, a sensible and practical digest of the present state of science with respect to that form of mortification commonly designated "gangræna senilis," a form of disease which the author tells us that he has had many opportunities of observing. The present production includes the results of his observations, interwoven with a critical illustration of the opinions and views hitherto prevalent respecting this affection: and as gangrene from obstructed circulation is a subject of very considerable interest, and cases of it not very frequently met with in the ordinary walks of practice, we presume that, in furnishing a brief analysis of the contents of the present little volume, we shall perform no ungrateful task to a considerable portion of our readers.

Notwithstanding the efforts of distinguished pathologists of various nations, and the accumulated results of morbid anatomists, the most contradictory views continue to prevail upon many points of this enquiry.

The cause of this continued confusion rests in the common sources of error, an imperfect definition of terms, and the too eager spirit of generalization. One and the same pathological process has been discussed by authors under different designations; and particulars of individual cases have been prematurely considered as characteristic of the affection universally.

The term *gangræna senilis*, as commonly applied, is decidedly objectionable, because it leads to the belief that the affection occurs only in old persons, a proposition which experience by no means sanctions. A variety of other designations, such as gangrene of the wealthy, atrophy from closure of the arteries and veins, gangrene from obliteration of the arteries, cadaverization, gangrene consecutive to arteritis, atonic mortification, and so on, are all too confined, and alike inappropriate, as they imply conditions present only in individual cases. The expression "spontaneous gangrene" is faulty in the other direction, since it would include, besides the form of gangrene under discussion, many others of a totally different nature: besides, gangrenous destruction, strictly speaking, does not occur spontaneously, but as the result of some antecedent morbid process.

Our author, in his own definition, adopts the one furnished by our countryman Dr. Carswell in his work on Pathological Anatomy, and regards the phraseology "mortification from a mechanical obstacle to the circulation of the blood" as best expressing the essential condition of this morbid alteration. The obstruction in question may exist in the heart, in the great vessels, or in their minuter ramifications, and may be the result of various organic changes. According as the special lesions display the phenomena of acute or chronic disease, two kinds of this destructive process are noticed, the inflammatory and the atonic gangrene.

The immediate occasion of this morbid process is most frequently found to rest in such an organic change within the arteries, that the afflux of the vital fluid to a part is either rendered exceedingly imperfect or is entirely abolished; so that nutrition and the other vital actions become entirely suspended. The diminution of the arterial caliber occurs either in the great trunks or in the minuter ramifications, and very often in both at the same time. Such diminution may arise from the presence of coagula, of fibrine, of organized or organizable lymph, or of other extraneous products within the vessels, thereby causing complete or partial closure of their canals. Changes occurring in, between, and about the arterial coats themselves, may also lead to the same result. Now, although it cannot be denied that, in many cases, mortification follows these deviations from the normal condition of the blood-vessels, it is yet true that these do not always originate the affection in question; hence it is certain that some other condition, not as yet so well understood, must at the same time exist; because, in the first place, it is very often observed that extensive derangement, leading to complete or partial obliteration of the arterial canals, takes place without any corresponding production of gangrene, and in some instances without even a trace of disease manifesting itself during life; in the next place, the frequency of cases of gangrene stands in no definite proportion to that of such lesions of the vascular structure, the latter being very often and the

former but rarely observed; and lastly, the intensity of the affection in particular cases does not seem to depend upon the extent to which the great arterial trunks have suffered disorganization. It is requisite then to look to alterations in the capillary system of vessels for the concurrent and more immediate cause of the mischief. M. Hecker controverts, and we believe successfully, the position assumed by Dupuytren, Broussais, Delpech, Bouillaud, and some other French pathologists, that inflammation of the arterial structure forms the proximate cause of the phenomena under discussion. Slow and gradually induced organic changes in the arterial and capillary vessels, as well as the occurrence of active inflammation in these structures, he regards as quite adequate to produce a gangrenous destruction of parts. Hinderance to the supply of blood is however essentially the antecedent condition, differences arising from the mode of its origin. Organic affections of the heart and great vessels, as also any interruption to the supply of nervous influence to a part, constitute circumstances favoring the development of mortification. Inflammation of the veins and absorbent vessels forms only an accidental though perilous complication, and is by no means, as contended by some, the immediate cause of the malady.

The *predisposing causes* of this affection concern the age, the sex, the situation of the parts, the mode of life, and the previous state of health. An interesting statistical table embodying some of these particulars, as they were found in 73 cases, is given by our author, the results of which are, as regards age, that, in 67 of the cases where the age had been recorded,

1	occurred	between the ages of	1	and	10	years.
6	,,	,,	10	,,	20	,,
7	,,	,,	20	,,	30	,,
9	,,	,,	30	,,	40	,,
1	,,	,,	40	,,	50	,,
12	,,	,,	50	,,	60	,,
19	,,	,,	60	,,	70	,,
3	,,	,,	70	,,	80	,,
8	,,	,,	80	,,	90	,,
1	,,	,,	90	,,	100	,,

From these results, obtained indiscriminately from the observations of pathologists throughout Europe, the impropriety of the term "gangræna senilis" is obvious, although the greater proportion of cases unquestionably arise in advanced life.

With respect to sex, although Pott affirms that, of 20 individuals affected with the present form of gangrene, scarcely a female is to be found, and although this statement is confirmed by some others, as Jeanroy and Balling, it is yet certain that this is no universal fact, since in France it occurs almost as frequently in the one sex as in the other; and where the disparity seems to be great, as in our own country, it is probably owing to the peculiar habits of the respective sexes. At any rate, the ailment is not so infrequent in the female sex, according to our author's table, as it has been too readily assumed by some. Of 69 cases, 27 were females. This gives a proportion of males considerably less than two to one.

The parts of the body most frequently involved in this destructive process are those far removed from the centre of the circulating system, as

the hands and the feet; it is but rarely that the nose, ears, lips, cheeks, &c. are implicated. As farthest from the heart, the lower extremities suffer more frequently than the upper, in the proportion of about three to one. Although, in the whole number of cases, this preponderance obtains, yet in the female sex the predisposition would seem to exist mainly in the upper extremities. Referring again to our author's table, it appears that of 17 cases where these latter were the seat of the disease, 10 were of the female sex and 7 of the male. It may not be certain that this result would receive confirmation from an extension of the numbers observed. If it should, however, it is matter for further enquiry, upon what this peculiarity may depend.

The previous habits of the patient, where they have been for a considerable period opposed to a right system of hygiène, furnish strongly predisposing circumstances; as also the long continuance of any pre-existing disease, inducing great impairment of the powers of life, either by direct injury done to the blood-vessels themselves, or by vitiation of the fluids of the body.

The *exciting causes* are not always to be detected. At times the endurance of a high degree of cold in the extremities, at others some mechanical injury done to the parts, seems to constitute the immediate occasion. But very often the attack appears to arise spontaneously, whence proceeds the designation "spontaneous gangrene" sometimes applied to this affection.

The progress of the symptoms may be traced through a succession of five stages: 1st, that of premonition; 2d, the inflammatory stage; 3d, the stage of gangrenous alteration; 4th, that of demarcation and throwing off of the mortified part; and 5th, the stage of cicatrization. Our author regards inflammatory action as preceding, in all cases, the actual occurrence of gangrenous alteration. In most cases no very striking morbid phenomena give warning of the approach of this fearful malady; and, although antecedent disease should exist, no material for a certain diagnosis in the early stage, is afforded. Anomalous gouty attacks often constitute the premonitory symptoms. The second stage begins with a slight erysipelatous reddening of the part affected which, from infiltration into the subjacent cellular tissue, yields on pressure, and it frequently exhibits a tense, marble-like aspect, whilst dark blue spots are remarked upon the affected surface. The principal vessels of the limb pulsate in many cases very strongly; whilst, in the smaller ramifications, little vital action can be noted. *Bruits* of various kinds can often be detected, with or without the stethoscope, along the course of the vessels; and these are often to be felt like firm stretched cords, painful on pressure. Other signs of inflammatory action soon show themselves, both generally and locally, varying in intensity according to the more or less acute character of the case. *Phlyctenæ* are next to be noticed upon the erysipelatous skin, and then the unequivocal display of mortification takes place, evinced by a gradual destruction of all the tissues of the part affected. Only very seldom, after the accession of the symptoms of disorganization, is anything like a reaction—a return to the normal condition of things—to be found. Cases of this kind, however, have been related by Van Swieten, Schenk, Lannelongue, and Vernhes de Rubastens.

The temperature of the mortified parts is ever remarkably low, as may

be perceived by the simple touch. Dupuytren discovered by the thermometer that this was even below that of the dead body; and Chevalier has shown that the heat of the toes, when affected with gangrene, is always below that of the chamber in which the patient happens to be placed.

The gangrenous alteration may limit itself to one or several toes, may proceed along the foot to the ankle-joint, or even, in some rare cases, onwards to the hip-joint itself. Generally speaking the powers of life become entirely exhausted long before any such progress is obtained. A fatal advance of the affection is to be feared, if no cessation or limitation of the erysipelatous inflammation can be remarked. When, on the contrary, the morbid alteration seems for some time fixed to a place, with a gradual decrease of the redness from behind forward towards the toes, and when the accompanying œdema assumes a cooler character, the formation of the line of demarcation and the rejection of the mortified parts may immediately be looked for. This introduces the fourth stage.

A red streak is now manifested, which, in a very short time, circumscribes the parts in a state of incipient gangrene. In a few days, or even in a few hours, a furrow is noticed in the position of the red streak, from which there issues a laudable pus in tolerable quantity. Through this suppurative process, the separation of the dead from the living structures is brought about, occurring in the first instance with the skin and cellular tissue, whilst the fibrous, tendinous, and vascular structures, together with the nerves and the bones, resist for a considerable period the gangrenous destruction. The nervous tissue maintains its vitality the longest, on which account a high sensibility of the mortified part is often retained long after the apparent completion of the process. The final rejection of the dead tissues does not, as Balling maintains, always occur at a joint, for this will not rarely be perceived in the continuity of bone. We have ourselves observed this to take place at the junction of the middle and lower third of the tibia. Parallel cases are recorded by Chelius, Nicksius, Regnault and some others. Upon the attainment of this result, the patient's health generally improves; and it is sometimes most remarkable how vigorously the digestive process keeps up under such circumstances. When an unfavorable issue ensues, it is generally characterized by low typhoid fever.

The fifth stage, or that of cicatrization, presents no remarkable phenomenon; it pursues, with various delays and interruptions, much the same course as in cases of amputation. False anchyloses very commonly form in the joints near to the mortified parts, especially when these happen to be the elbow or knee joint.

When this malady arises in the aged, or in constitutions prematurely worn out, it occurs most usually in a chronic form. In such cases an organic change in the arterial parietes is the most ordinary concomitant. The formation of the line of demarcation, the process of rejection, that of cicatrization, and so on, are in their characters essentially the same in chronic as in acute cases. When the disease exists in children, its progress is much the same as in adults. Jäger, who has observed this form of disease in young children, is of opinion that, in some instances where infants are born with defective extremities and where clear marks of cicatrization seem discoverable at the stump, the defect is attributable to

gangrenous destruction occurring to the fœtal limbs whilst in utero. The water canker of children our author excludes from the present category of disease.

The duration and progress of the affection differ so completely in different instances, each case so exhibiting its own specialities, that nothing definite upon this branch of the subject can be established. Sometimes the process of destruction advances, with unspeakable suffering, in a few days from the toes to the knee-joint, or even to that of the hip; whilst, at other times, months are required to complete the morbid alteration in one or more toes; the difference seeming to depend mainly upon variations in the exciting cause. It is certain that when arteritis, or other inflammatory ailment, occasions its development, the progress is more rapid than when it takes its origin in other organic changes gradually arising. The formation of the line of demarcation, and the rejection of the mortified part, advance with a course corresponding with that of the anterior gangrenous destruction.

Diagnosis. Although no difficulty exists in establishing the correct diagnosis after the appearance of the mortification itself, there is yet sometimes considerable embarrassment in determining the nature of the affection whilst in the first and second stages. The necessity of being able to form something like an accurate judgment at the onset, however, becomes the more obvious, when it is considered that otherwise the subsequent gangrene will be almost sure to be attributed to the treatment employed by the practitioner. Here our author slightly reviews the various diseases with which the first symptoms of the malady may be confounded. The most prominent of these are the various forms of erysipelas. The true erysipelas (Rosa) is commonly ushered in by symptoms of gastric fever, with a throbbing sensation of heat and tension locally, but does not give rise to the same violet or blue spots, as in the affection now under consideration. In the former case the *phlyctenæ* are not filled with a similar turbid, fetid fluid. The erysipelatous affection, moreover, yields readily in most cases to appropriate treatment. In many of these instances, however, considerable difficulty may exist; and, under such circumstances, it will be well to offer a guarded opinion until the local symptoms have sufficiently declared themselves to lead to a more certain diagnosis. In such doubtful examples, the age, the constitution, the sex, the previous habits of life, and so on, must receive a very careful attention. Some confusion may occasionally exist with respect to certain other pathological changes; but, excepting in the instances of erysipelas, the difficulty need be but slight.

The *prognosis* is at all times exceedingly doubtful, but it is not by any means so unfavorable as it is commonly assumed to be. The statistical table before referred to exhibits a proportion of recoveries to the deaths as 43 to 26, a result sufficiently favorable to be at variance with opinions current with respect to the fatality of this disease. In the establishment of a prognosis, regard must be had to all the antecedents and accompaniments; and, even when this is done with the greatest care, a *probable* judgment can alone be formed. The acute form of the disease is at all times more dangerous than the chronic, especially if it run a rapid course, and occur in a constitution previously injured, either by dissipation in early life or antecedent disease. The chronic affection is the more suspi-

cious according as the subject of it is advanced in life, or as the vital energy is exhausted from any cause whatsoever. Death is usually preceded by symptoms of low typhoid fever, and the character of the local disease is mainly distinguished in such cases by an absence of successful attempts at the formation of the boundary line. A succession of two or three attacks is not unfrequently recovered from, but the happy issue of a *fourth* is a rare phenomenon.

Treatment. In the treatment of the present species of gangrene, prophylactic measures are too much neglected, though, after all, they constitute probably the most important feature. All exciting causes, so far as may be practicable, should be withdrawn, and a judiciously conducted hygiène be strenuously enforced. Specifics calculated to prevent the invasion of mortification do not exist, or at least are totally unknown; and the intention of the practitioner must be directed to the obstruction of its progress, the facilitation of the process of separation, and the mitigation of the suffering associated with the organic change. The treatment, as a whole, must aim at, first, the subduing of the phlogistic process; secondly, the favouring of the limitation of the disease, and the rejection of the dead structure; and lastly, the obtaining of a healthful cicatrization, and the removal of any induced deformity which might afterwards limit the function of the parts. The treatment will, however, vary considerably according to the difference in the immediate cause of the disease. If the patient be not far advanced in years, and if the morbid phenomena indicate the presence of acute inflammation in the affected structures, an antiphlogistic treatment will of course be appropriate. This may either be local or general according to the special circumstances of the case. When, in these instances, the use of leeches is indicated, the practitioner must be careful to apply them at some distance from the seat of inflammation along the course of the arterial vessels, as otherwise the irritation caused by the leech-bites might constitute a source of great aggravation. The employment of opiates and other anodynes, both topically and generally, is highly advantageous in almost all cases.

In the chronic form of the complaint all depressing measures must generally be avoided. The sunken powers of life have rather to be supported as much as possible by the employment of such tonic remedies as the peculiarities of the case may indicate. The employment of the preparations of bark combined with some slight acid, as the phosphoric, will usually be attended with advantage. Diffusible stimulants are rather injurious than otherwise in these cases. The local treatment is commonly best conducted by the application of aromatic fomentations and poultices, rendered slightly acid by the admixture of vinegar. Scarification and other analogous means are studiously to be avoided. Notwithstanding that Mott, Crisp, Brulatour, and some others have amputated above the diseased part with advantage, the instances in which this has occurred are, in the estimation of our author, rather to be regarded as fortunate accidents than as recommendatory of the practice; and, in this opinion, he is borne out by the opinions and the experience of most surgical authorities.

Having furnished a brief summary of the contents of the little volume before us, we must, in conclusion, express our great satisfaction with the

work, taken as a whole. Almost everything that is known of this disease is here given in a concise form, together with some excellent observations proper to the author himself. We have some doubts regarding the strict pathological identity of the various cases included by M. Hecker in his category. Mortification occurring in old age, the result of what may be called a senile degeneracy in the texture of the arterial coats, we yet think, is entitled to be regarded as a form of disease sufficiently well marked to suggest the propriety of a classification apart, under the old term of "gangræna senilis." We think, also, that a greater pathological difference will be found amongst the cases included by our author than between some of them and others which he would reject as not properly embraced within his adopted definition. However, there is so much of the practical to approve in the present production that we forbear to press any theoretical criticisms which may seem to detract from our general commendation of the work.

ART. VIII.

1. *Traité Pratique des Hernies, Déplacements et Maladies de la Matrice, &c.* Par P. L. VERDIER, Chirurgien Herniaire de la Marine Royale, des Hôpitaux Militaires de France, &c.—*Paris*, 1840. 8vo, pp. 740.

Practical Treatise on Hernia, Displacement, and Diseases of the Womb, &c. By P. L. VERDIER.—*Paris*, 1840.

2. *Leçons Cliniques sur les Hernies, faites à l'Amphithéâtre du Bureau Central des Hôpitaux Civils de Paris, en* 1839-40. Par J. F. MALGAIGNE, Professeur agrégé de la Faculté de Médecine de Paris; et recuéllies sous ses yeux par M. ED. GELEZ, Interne des Hôpitaux de Paris.—*Paris*, 1841. 8vo, pp. 238.

Clinical Lectures on Hernia, delivered in 1839-40. By J. F. MALGAIGNE; collected under the Professor's superintendence by M. E. GELEZ. —*Paris*, 1841.

3. *A Treatise on Ruptures.* By W. LAWRENCE, F.R.S. Surgeon Extraordinary to the Queen; Surgeon to St. Bartholomew's Hospital, &c. Fifth Edition.—*London*, 1838. 8vo, pp. 632.

4. *Traité de Pathologie Externe, et de Médecine Opératoire.* Par AUG. VIDAL (DE CASSIS), Chirurgien de l'Hôpital de Lourcine, &c. Tome Cinquième.—*Maladies de l'Abdomen.*—*Paris*, 1841.

Treatise on Surgery. By AUG. VIDAL. Vol. V.—*containing the Section on Diseases of the Abdomen.*—*Paris*, 1841.

THE discrepancy which exists in regard to the general statistics of hernia, as supplied from various sources and by different authorities, is, on first inspection, not a little puzzling: but reflection will trace it to its proper source—viz., the generalizing from observations which are either too limited, or of too comprehensive a character. Sufficient importance has not been attached to circumstances of age, occupation, social position, climate, &c.; and the consequence is the contradictory statements to which we allude. Thus we find, as quoted by both Lawrence and Malgaigne, that the returns of the proportion of the ruptured, to the

whole population, varies very greatly, as given by Arnaud, Turnbull, Bordenave, Juville, &c. We cite these after our French author, previously to accompanying him and his contemporary, Verdier, into the more satisfactory details with which they supply us.

The proportions of Arnaud and Bordenave present us with extremes; the former affirming that one in eight of the whole population are affected with hernia, whilst the latter states that one in a hundred is the average. The results obtained by Mr. Turnbull, surgeon to the London Truss Society, after "the most diligent and general enquiries throughout the kingdom," are, that the proportion of ruptured persons is one in fifteen. We need scarcely observe that *enquiries*, such as those alluded to, must necessarily be limited at the least as to station of life, as well as sex; unless, indeed, the information obtained second-hand from practitioners and truss-makers—sources too indirect and equivocal for trustworthy statistics. M. Louis's computation was made from the Parisian hospitals, in which the circumstances of sex, age, and employment were, to a certain extent, indicated; but of course the inmates of hospitals do not afford us the proper means of generalizing respecting a whole population: his numbers are, in the Salpêtrière (women), three per cent.; in the Bicêtre (men), six per cent.; in the Invalides (old soldiers), seven per cent.; in la Pitié (children and youths), two per cent. To these conclusions M. Malgaigne raises objections, asserting rather amusingly that he does not believe M. Louis could have had access to the persons of all the old women of the Salpêtrière, or the veterans of the Invalides. We feel as readily disposed to admit the testimony of Juville, a Parisian truss-maker, as any that we have already cited; his range of observation has at any rate been sufficiently extensive, and is probably not obnoxious to the objection that it excluded any particular class or either sex. In the north of Europe and Germany he computes that one in thirty are ruptured; in Italy and Spain one fifteenth of the population; and one twentieth in France and England. Dr. Knox, in his observations on this subject, in the Edin. Med. and Surg. Journal, states that he examined eighty-six persons of the lower orders without finding one case of hernia. So much for these unsatisfactory results: now let us turn to where we at the least have promises of something more definite.

In reference to sex, we extract M. Malgaigne's introductory remarks and table.

"We have sought to avoid all sources of objection, to discard all such equivocal means of research, and, in short, to have periods of comparison and proper control over our resources, by forming three series of observation corresponding to three several epochs; thus, in October and November 1835, we had collected 435 observations,' of which twenty-five were cases of prolapsus. Deducting these cases, 410 remained which were herniæ; of these 335 were in men, and 75 in women.

In 1836	of 2767	herniæ,	2203	were in men,	564 in women.
1837	,, 2373	,,	1884*	,,	489 ,,

The mean result is about four to one."

The tables of M. Verdier are copious, and include every point connected with the statistics of hernia; and the number of cases upon which his classifications are founded is satisfactory, viz., 1226. We have no reason or right to doubt that they are authentic, and shall therefore avail ourselves of them as such. We find, in the summing up of his

* In the original, 2884; evidently a misprint.—ED.

third table (p. 228), that the gross number of each sex afflicted with rupture is, of males 976, and females 250; a proportion which agrees accurately with that of M. Malgaigne. We shall have occasion, in our discussion of the various forms of hernia, to refer to the details of this and others of M. Verdier's statistical reports. Lastly, we turn to Mr. Lawrence, who gives us an abstract from the register of patients relieved by the London Truss Society, during twenty-eight years. Of 83,584 patients, 67,798 were males, and 15,786 were females. (p. 11.) In this it will be observed that the proportion of females falls very little short of that afforded by the results of our French authors: we may therefore assume that the relation of *four to one* is a correct average as regards the two sexes.

The age at which hernia is most frequent next occupies the attention of M. Malgaigne. After observing that the London Society, already referred to, possesses the only documents that he is acquainted with, where an account of ages has been kept, he gives way to a burst of lamentation that "people but little advanced in civilization should take so little care of preserving a knowledge of their ages;" and thence proceeding directly to generalize upon an amusing story told by one of his own countrymen respecting an English "savant," who, in making some researches in which age was a desideratum, was unable to ascertain that of either his wife or servant,—concludes that probably England may justly be included in the "barbarous" category, and that the London Society's tables are not therefore deserving of the confidence they would seem to claim. We do not pretend to decide upon the relative chronological faculties and facilities of the two nations, but surely our author gives us a bad sample of philosophical deduction, while he is, at the same time, calling upon us largely to give credence to the results of his own impartial investigations. For all practical ends the *exact* age of individuals is by no means essential for statistical purposes in a question like the present: we therefore think the following table of the Truss Society both interesting and important. Of the eighty-three thousand cases already alluded to, 5448 had congenital hernia; 7299 were relieved with trusses under ten years of age; 4551 between ten and twenty; 8715 between twenty and thirty; 13,614 between thirty and forty; 15,627 between forty and fifty; 14,169 between fifty and sixty; 9761 between sixty and seventy; 3866 between seventy and eighty; 442 between eighty and ninety; 23 between ninety and a hundred. (Lawrence, p. 13.) As we have not space to give M. Verdier's table in detail, we insert the account of his 1226 cases as they occurred between the ages set forth in the foregoing table. Under ten years of age the number was 973; from ten to twenty 152; from twenty to thirty 57; from thirty to forty 25; from forty to fifty 11; from fifty to seventy 8. (Verdier, p. 237.) The discrepancy between these accounts proves how difficult it is to arrive at satisfactory results where the object of investigation cannot be reduced to one of simple observation, unembarrassed by circumstances such as neglect, ignorance of the existence of disease, &c., on the part of patients.

But let us turn to the more elaborate enquiry of M. Malgaigne; he does not favour us with a tabular view, but informs us that "his cases

are distributed in three series, and that these three series coinciding, his conclusions are thereby greatly strengthened."

"If, from the total number of herniæ which come under our observation, we seek to fix the proportion of those which occur during the first year after birth, we shall find that it gives a mean of 52 per cent., a thirty-eighth part of this number being males, and a sixty-secondth part females. The proportion is thus much greater for females at this age, than for all the periods of life taken together, a circumstance readily explicable. Inguinal and umbilical herniæ are those which especially exist: to the latter the predispositions are equal, and to the former the descent of the testicles is the only additional predisposing cause, the inguinal canal being found open with equal frequency in the two sexes." (pp. 13-14.)

From the completion of the first to that of the second year, M. Malgaigne has found the proportion considerably diminished, and still more so between the ages of two and five. From five to thirteen this decrease continues in nearly an equal proportion for the two sexes; and he particularly remarks that it is between eight and nine years of age that the fewest herniæ occur. He conjectures that the explanation of this fact is to be sought in the interval which exists between the development of new exciting causes of the disease, and the cessation of those which operated as predisposing causes at the earlier period of life. From the age of thirteen to twenty the proportion is augmented in a marked manner, but involves almost exclusively the male sex; a fact which, we think with M. Malgaigne, may be rightly accounted for by the increased physical exertion of the one sex,—whether it be in the games and exercises of the better classes, or in the laborious employments of the lower —whilst the other is restricted usually from those healthful recreations of the body which are permitted at an earlier period of life, but which are prohibited as unfeminine when the ultimate development of the frame most demands them. From twenty to thirty the proportion still continues to increase, especially during the last two years, and affecting women rather than men. The rarity of umbilical and crural herniæ in females from accidental causes previous to this age (M. Malgaigne has seen but one case), together with the tendency to those forms of the disease dependent on the phenomena of puberty, and especially pregnancy, are assigned by our author as the reasons for the peculiarity alluded to. From thirty to thirty-five, no marked change takes place; but from the latter age to forty the numerical progression is nearly doubled in both sexes, and exceeds the proportion of any subsequent years. Between forty and fifty the average preponderates amongst women; but after that age men are more subject to the occurrence of rupture, the *general* proportion remaining nearly fixed up to seventy; lastly, between seventy and eighty, it drops to one half in men, and one third in women.

In speaking of the causes of rupture, Mr. Lawrence refers them to two heads, viz.: the "occasional or exciting, and those which act as predisposing causes of the complaint." These divisions should, of course, include the consideration of climate, occupation, general habits, and original conformation, to render the enquiry complete. On these subjects we propose now briefly to analyse the contents of our author's volumes.

M. Malgaigne informs us, that among the conscripts of Paris during the years 1816-23 inclusive, he remarked that of the rich one in thirty-seven was ruptured; amongst those in easy circumstances one in thirty-eight; whereas of the poor one in twenty-eight had herniæ. (p. 23.) His further consideration of the relative liability to hernia of different parts of France is more curious than important. The Celtic race, who occupy central France, are the most subject to rupture, whilst the Norman, Germanic, &c., are especially exempt. The comparative frequency of hernia in different countries is a point which involves the influence of too many casual circumstances to offer much temptation to the statistical enquirer. The employments and habits of the natives are, probably, the more correct source to which we may look for an explanation of the varieties in question; for with the enervating and relaxing influence of a hot climate, we find, more or less, amongst all classes a disposition to physical indolence. We have already noticed the relative frequency of rupture in Germany, France, Italy, and Spain, according to Juville's account; we may add that in Switzerland and Holland, the inhabitants appear especially obnoxious to the complaint; a prevalence which, as it affects the former, we think Mr. Lawrence justly coincides with Blumenbach in ascribing to the "universal practice of violent gymnastic exercises by the young lads," the more violent exercise of poising and throwing heavy weights by the same, and their practice of carrying heavy weights on their backs. As to the Dutch, we feel at a loss to account for their proneness to hernia; but we cannot but concur with Mr. Lawrence in smiling at the cause assigned by many authors (Malgaigne amongst the rest), viz., the milk, cheese, and potatoes, and especially the first, which is so common an article of diet in Holland. Mr. Lawrence may well ask jocosely how it is, "that a single Irishman should escape the united operation of the milk and potatoes?" (p. 48.)

M. Verdier found those of the "nervous and lymphatic" class the most subject to rupture. (Tab. v., p. 230.)

In turning to occupation and profession, constituting one class of predisposing or exciting causes of hernia, we do not observe that exact concordance in the different statements which we should have anticipated; we merely give the results. M. Malgaigne divides his tables into two classes: in the one he includes those employments which are prosecuted in the erect posture, and the other comprises sedentary occupations. Between the ages of fifteen and thirty-five, the proportion of the former to the latter, is as 83 to 21; between thirty-five and eighty, it is as 104 to 39 (p. 39): thus a standing employment appears to operate as an important predisposing cause to this disease. The table of M. Verdier differs in giving individual professions and trades, and is too extended to transcribe in full; we will, therefore, satisfy ourselves, by noticing a few of the leading particulars. Of 950 cases, 204 were individuals without occupation or profession, and therefore, it is to be presumed, belonging to the wealthier classes; 158 were bankers, merchants, and trades-people; 141 military, the infantry exceeding the cavalry in the proportion of three to one; a fact which, however, is doubtless accounted for by the great preponderance of the former over the latter division of the army, for it is admitted at all-hands that the exercises of the cavalry, who are called on to dispense with the use of stirrups, greatly

tends to produce rupture. The other classes in M. Verdier's table include almost every variety of employment, and 129 individuals were too young to have any fixed profession or trade. (Table vi. p. 231.)

In addition to these various causes, as predisposing to hernia, we may enumerate local weakness, such as laxity of the abdominal muscular parietes and peritoneum; rapid emaciation after corpulency, (of which Mr. Lawrence mentions a remarkable case, p. 46); and the "injudicious application of tight clothing to the trunk of the body," by which the healthy action of the respiratory organs is interfered with, and the abdominal viscera are compressed and forced downwards. M. Malgaigne arrives, by a singular process of computation, at an opposite conclusion to that above cited: having remarked the altered form of the chest and abdomen produced by the tight zone worn by a Greek, he enquired of him whether rupture was common in his country, and being informed that he was unacquainted with the disease referred to, the professor conjectured that its existence is very rare in Greece, but admits that it is only a conjecture! (p. 28.) The numerical preponderance of right-side over left ruptures would appear to depend on the more ordinary employment of the right side of the body, especially when great exertion is required. This point has been, we think, satisfactorily discussed by M. J. Cloquet, in his researches on the subject. M. Malgaigne seems more disposed to attribute the peculiarity in question in part, at least, to certain circumstances noticed by Camper and Wrisberg in reference to the descent of the testicle, and occlusion of the internal ring. These anatomists found, that where only one testicle had descended, it was more frequently that on the right side than the left, and the peritoneal opening continued unclosed longer on the former side. Our author also admits that, as the result of his own observations on herniæ existing in right and left-handed persons, he found in 136 of the former class, 91 right ruptures; and in 17 of the latter, 10 left ruptures.* (p. 47.) These facts certainly favour the theory of M. Cloquet.

Before we proceed to treat of different forms of herniæ, we will pause just to see what our authors have to say respecting the general mortality in these cases. As we do not allude especially to the fatal termination of cases subjected to operation, a reference to the tables containing the proportion of the ruptured to the whole population at different periods of life, will conduct us to the attainment of the desired information. M. Malgaigne informs us that he has communicated the result of his enquiries on this head to the Academy of Sciences, considering that the subject, treated statistically, is one of the highest practical interest; in proof of which he gives us this summary of his investigation: Up to thirteen years of age, there is a numerical decrease in the proportion of ruptured individuals to *one in seventy-seven;* from which period to the age of forty the ratio gradually increases, until we find one ninth part of the population affected. So far then we may assume, that this disease exercises a very trifling, if any, influence (strangulated cases apart) upon the duration of life, fresh sufferers being added without a proportionate mortality; and this even continues to operate up to the seventy-fifth year, at which age the "ruptured form almost one third of the male population;" but, after this age, the proportion diminishes rapidly,

* These remarks of course bear especially on the inguinal form of hernia.

whence we are naturally led to infer that life, after the age above mentioned, is decidedly curtailed by the existence of hernia; a fact which should "induce us diligently to seek to perfect the palliative treatment, so as to give the ruptured part of the population an equal chance of longevity with others." (p. 21 et ante.) As M. Verdier's tables do not ascend beyond the age of seventy-eight, we are unable to compare them satisfactorily with the preceding.

We now pass on to the consideration of different forms of hernia, in which we shall continue to follow M. Malgaigne in the brief analysis we propose to give of each division of the subject, and, as we have already devoted so much space to the discussion of the statistical portions of their works, we shall confine ourselves to the selection of such points of agreement or contrast in our authors as may best merit attention.

Inguinal Hernia. In the introductory remarks to his lectures on this, the most frequent and important form of rupture, M. Malgaigne considers the several predisposing causes which operate in its production; some of these we have already noticed in our general remarks in the preceding pages, and we now proceed to examine the remaining. In speaking of *hereditary predisposition* to this complaint, the following are Mr. Lawrence's remarks:

"When it is stated that hernia has sometimes appeared to be hereditary, the meaning of the observation must be, that there is a certain weakness in the original formation of the parts, predisposing to the complaint, and that this defect may descend to the offspring, and, in this sense, its truth cannot be disputed. I believe that the word *hereditary*, in its application to disease, has been always used according to this interpretation; and that the employment of it, in its strict sense, has only been suggested by those who wished to show their ingenuity in refuting an absurdity of their own creation." (p. 45.)

If Mr. Lawrence mean, as we can scarcely suppose, by the above paragraph, that *no contracted* disease can be conveyed from parents to children, it is an opinion which general experience and observation contradicts; but as applicable to hernia and other local complaints of a similar nature, his remarks are doubtless correct; and we cannot conceive how any one, with the most moderate pretensions to a knowledge of physiology, could broach a doctrine so inconsistent with its laws as that deprecated by Mr. Lawrence. With this understanding, then, we give an abstract of M. Malgaigne's comments on the subject.

He observes that "it is an important question in relation to which he possesses very significant tables," and that "the hereditary predisposition is one of the most powerful influences." Of 316 individuals afflicted with hernia, eighty-seven informed M. Malgaigne that the disease existed in other branches of their families; a number which, in itself large, would probably have been augmented had the truth been known or elicited in every case. We do not consider it necessary further to individualize the degree of relationship which is given in different columns of M. Malgaigne's pages, but we may notice that by far the largest number was in a direct line from father or mother to children, and that there were many instances in which brothers were the subjects, (p. 42, et ante.) As we do not find that our other authors have any remarks on this point, we may proceed. Height and the form of the abdomen M. Malgaigne has observed to have an important influence in predisposing to inguinal

hernia. To the former his attention had been first directed by an army-surgeon, and his own subsequent observations coincided with those of his informant; of seventy-eight individuals, fifteen were under (misprinted "au-dessus,") five feet (French;) twenty-one between five feet, and five feet two inches, and forty-two above the last height. This yields a proportion very unfavorable to height; a fact which M. Malgaigne connects with "a proportionably greater feebleness of constitution." We should guess our author was not a very tall man. With respect to the development of the abdomen, he has found that the flat-bellied comprise more than half the ruptured, whilst the moderately fat and very aldermanic are comparatively exempt; for the definition of a fourth form of abdomen, entitled "triple saillie," we must refer our readers to the original, and to the City of London.

The immediate or "determining" causes of inguinal hernia, is a subject of sufficient importance to demand some attention. In a great many cases it is of course impracticable to trace the exciting cause of rupture; indeed in many instances none can be said to have existed; but of those which can be determined, the class which contains the highest numbers in the tables of both Verdier and Malgaigne, is that which comprises those who have suffered from lifting heavy weights. Falls, coughs, asthma, &c. are fruitful causes of this, as indeed of other forms of hernia; but next in numerical succession to the first-mentioned cause in M. Verdier's table, is habitual constipation and hemorrhoids. The truth of this last observation our own experience leads us to corroborate; and we should think most practical surgeons must regard constipation as one of the most common causes of what is termed spontaneous inguinal hernia. The exertion constantly employed by those who are habitually constipated to relieve the bowels of their contents, produces a decided tendency to enlargement of the inguinal rings and canal, which is most favorable to the descent of a hernia; and this as frequently in women as in men. We are therefore rather surprised to find that M. Malgaigne overlooks this in his enumeration of the determining causes of the form of rupture in question.

In his next lecture M. Malgaigne comprises the different aspects under which inguinal hernia makes its appearance: he adopts the ordinary division into that form which takes place previous to the closure of the opening of communication between the general sac of the peritoneum and vaginal tunic of the testicle (congenital), and that which takes place after this process is completed. The former of these he subdivides into herniæ which are inclosed in the serous covering of the testicle, and those which do not descend below that portion of the vaginal tunic which invests the spermatic cord. To these heads may be added the encysted hernia of Sir A. Cooper, amongst those forms of rupture proper to infancy. This arrangement tends to a misapprehension which is not intended by the author; for it has long since been shown that the impression of Mr. Hey (who first described this form of hernia,) was incorrect, when he represented it as peculiar to the earliest period of life; the fact being that though the vaginal tunic forms an external sac, the internal or proper sac of the hernia is derived, as in other instances, from the peritoneum at the closed internal ring. M. Malgaigne justly condemns the term "congenital" as it is usually employed, for it implies that which in truth

is scarcely known to exist; but in his speculations upon the question of why a hernia should not descend prior to birth as well as the testicle, he seems to have forgotten that the latter is subjected to a peculiar force dependent on the gradual contraction of the gubernaculum. He well may add that the violent efforts of crying, coughing, &c., (the common cause of hernia in children,) cannot operate before the lungs have been inflated by the first inspiration. The influence of long-continued pressure on the testicle, in producing wasting, or perhaps rather arresting development, has been frequently remarked by M. Malgaigne in testiculo-vaginal rupture. Mr. Lawrence's remarks on the employment and proper application of trusses in these cases demand the attention of the young surgeon; we find that they are closely followed by the French professor.

"Before the surgeon applies a truss for an inguinal or scrotal rupture in a young person, he must satisfy himself not only that the protruded parts are fairly replaced, but that the testicle has reached its normal situation in the scrotum. A rupture may take place in an infant when this gland has not yet quitted the abdomen. . . . . The application of a truss to a young subject, thus circumstanced, might prove injurious by retarding the descent of the testicle. If it should have arrived only so far as the groin, the pressure of the pad on the gland may be attended with still worse effects." (Lawrence, p. 573.)

We pass over the general remarks of M. Malgaigne on inguinal hernia as containing nothing calling for notice, until we arrive at the disputed question regarding the seat of stricture in the strangulated forms of this rupture. We had looked forward to this when we first opened his book, for in the preface we found it announced that "a new doctrine" was to be broached on the subject. This new doctrine proves to be that the true seat of stricture is the neck of the sac! We leave our readers to judge of the *novelty* of this explanation, whilst we proceed to give a few of the professor's remarks. We have said that a variety of opinion has existed, and, we may add, still exists upon this point; for example, our author admits that Dupuytren thought the neck of the sac was the most frequent seat of stricture, whilst Velpeau considers that when such is the case, it is an exception to the rule; and Sir A. Cooper attributes much to the constricting power of the rings. M. Malgaigne justly remarks that in operating it is comparatively of such trifling importance to decide on the tissue which is producing strangulation, provided the surgeon is able to release the contents of the hernial tumour, that it is rarely a subject of investigation (nor could it be very easily) at that time; and if a fatal termination of the case permit an autopsy, the division of textures is too general to allow of a ready determination of that which had been the cause of the mischief. What is also said by our author respecting the condition of the testicle is certainly reasonable, namely, that were the seat of stricture at the rings, this organ ought to suffer by compression of its vessels, &c., which traverse the inguinal canal. These and other considerations, deduced from dissection of old herniæ, have led M. Malgaigne to believe that the neck of the sac, altogether independent of either ring, is the seat of stricture: but the subject is of sufficient interest for us to examine severally the opinions of the different authors whose words we have before us. First of M. Verdier. In his chapter on strangulation we find the following remarks, which are all we have been able to glean from his copious volume, on the subject.

"At all times when called to the examination of a strangulated hernia, the first care should be to form a clear idea of the precise seat of the constriction to which the viscera are subjected; more especially as it may exist in all the parts whereby the organs which form the tumour are inclosed. In recent herniæ which have taken place spontaneously, it depends almost always on the attraction of the aponeurotic fibres which embrace the viscera. But if the hernia is an old one, the fibrous bands are too feeble to produce such an effect. It is then on the thickness and firmness (solidité) of the neck of the sac, that the compression of the protruded organs depends. Sometimes the aponeurotic opening and the neck of the sac may concur in constricting the viscera." (p. 161.)

We have made a literal translation of the above passage, in order to avoid misinterpretation of our author's meaning, which is a little indefinite in consequence of our not being able to fix a determinate signification or position to the aponeurotic fibres he mentions. If by "aponeurotic *opening*" he intends us to understand the funnel-shaped prolongation of the fascia transversalis, usually denominated "internal *ring*," we think he is right; at any rate he is not peculiar in the opinion that this is not an infrequent seat of stricture. But let us turn to the observations of M. Vidal, who treats the subject of hernia generally in a concise and practical manner.

"Strangulation," he says, "is often produced by the ring, [external ?] but even more frequently perhaps by the superior orifice of the canal, [internal ring ?] or by the neck of the sac. Congenital hernia is more often strangulated by the neck of the tunica vaginalis. The superior orifice of the canal (inguinal) being formed on its inner side by the internal portion of the transversalis fascia, mingled with the tendinous insertion and inferior fibres of the internal oblique and transversalis muscles, it may be conceived that the contraction of these muscles may contribute in augmenting the constriction. The inguinal is the only form of hernia which presents this disposition of stricture, and in which a spasmodic strangulation is rendered probable. This hernia is, doubtless, that one most frequently meets with sacs having several necks, and in consequence, numerous points of strangulation." (p. 29.)

The observations of Mr. Lawrence may be embodied in the following brief quotation:

"In performing the operation for strangulated hernia, and especially in liberating the protruding parts from pressure, which causes strangulation, we must bear in mind that the ring of the obliquus externus is not the only, perhaps not even the most frequent seat of the stricture; that the parts may be confined at the upper aperture of the inguinal canal or by the neck of the sac; and that, occasionally the stricture is below the canal, and altogether independent of it." (p. 265.)

Further on, in proof of his impression that the external ring is sometimes the seat of strangulation, the same author gives us a valuable hint as to the diagnostic marks by which we may determine this important point: he says,

"When the stricture is formed by the ring of the external oblique, the swelling of the rupture does not extend beyond that point. The inguinal canal is soft, compressible and indolent; while the ring is distended, tight, and hard. When, on the contrary, the strangulation is at the neck of the hernial sac, the inguinal canal is full, hard, and painful, presenting a firm cylindrical tumour extending from below obliquely upwards and outwards." (p. 266.)

So much for this disputed point: we have only to add our belief that, though the neck of the sac is probably the most frequent seat of stricture

in oblique inguinal hernia, both the external ring, and conjoined tendinous insertion of the internal oblique and transversalis muscles are occasionally found to strangulate the protruded intestine or omentum. Indeed, we can scarcely otherwise account for the frequent examples that we have of the favorable result of the taxis, when aided by measures the direct tendency of which is to paralyse muscular contraction, such as bloodletting, emetics, hot bathing, and even the tobacco enema.

The next subject discussed by M. Malgaigne, is "the conditions of the radical cure of inguinal ruptures." He considers that the desirable result of a permanent cure, is only to be hoped for in the oblique forms of inguinal hernia; and that even in these there are several important distinctions which it is essential clearly to define and establish. In infancy, for example, he considers that a radical cure is most easily obtained, as the indication is only to complete that which nature has left unfinished, namely, obliteration of the communication between the general peritoneal cavity and the tunica vaginalis. The natural tendency to this effect at a very early age, doubtless materially aids the efforts of the mechanist or surgeon; but great care should be taken that the adaptation of the means employed do not aggravate instead of cure the existing mischief, as Mr. Lawrence has pointed out may be the case. M. Malgaigne has found great success attend the proper application of mechanical means between the ages of thirteen and thirty-five; a fact which he attributes to the frequent occurrence of hernia from bodily exertion at this period of life. This explanation is, no doubt, just; inasmuch as the purely accidental production of rupture indicates less of a predisposition to the complaint, than where the disease is spontaneous, which is the case later in life. But some little credit must also be assigned to the renovating power of the constitution, which is more peculiarly the attribute of youth.

We pass by the '*history*' of trusses, to see if we can discover any thing practical worth extracting from our authors on the subject of those now employed. After speaking of the soft leather and linen trusses, which are now rarely seen, M. Malgaigne discusses the merits of the French and English contrivances in steel; and the preference he gives to Salmon's simple and ingenious piece of mechanism, must be gratifying to the inventor, as it is candid in the professor. We say this because he confesses having commenced his examination and practical testing of their relative merits, with a decided predilection for the French contrivances, and a peculiar antipathy to Salmon's truss, based upon a supposition that its theory was not consistent with the anatomical arrangement of the parts to which it was applied. He appears to have tried in vain to modify Salmon's apparatus so as to make it meet his own preconceived notions on the subject; and at last candidly confesses that, with the exception of the badness of the steel, (the last exception we should have anticipated from a continental critic,) it is the most perfect contrivance that has yet been invented. We do not observe that the long and complex account of what a truss should be according to M. Verdier, contains anything particularly worthy of notice. Amongst the various forms of elastic and inelastic pads described by M. Malgaigne, he notices those furnished with spiral springs, and others made with caoutchouc and distended with air; the former he has found only appli-

cable in umbilical ruptures; and the latter, though excellent when new, sooner or later fails from the gradual flaccidity consequent on the invariable escape of the air; he has found them applicable to some cases, but does not specify what sort. After denouncing the compresses of solid wood or ivory for inguinal hernia, M. Malgaigne informs us that he has procured some pads of solid caoutchouc which are in every respect desirable, and appear to be durable: at the same time he admits that a nucleus of wood, well and evenly padded with wool, and covered with soft leather, answers the purposes for which it is destined nearly equally well. We think these points are of great importance in the cure of hernia as well as for the comfort of the patient; and should receive more of the attention of the surgeon than they always do. Mr. Lawrence (who does not appear to give a preference to any particular form of truss, provided it fulfil certain desirable objects which he points out,) considers that the size and form of the pad require especial consideration in reference to the case to be treated; and he justly indicates the evil consequences of neglecting to adapt a compress of a proper degree of convexity; a too convex pad not only often leads to mischief by permitting the escape of some portion of the usual contents of the sac, but also "by pressing the external part into the opening, it keeps them distended, and prevents that contraction on which a radical cure depends." (p. 96.)

The space and attention which have been devoted by M. Malgaigne to the farther consideration of the *form* of hernial pads is an indication of the important share he assigns to them in the radical cure of rupture; we should therefore scarcely do our author justice if we did not analyse his views; the more especially as his experience seems to have been extended, and as he appears to have had but one object in view, that of testing the practical application of such contrivances as theory suggested. First, in speaking of cases of indirect hernia, traversing the whole inguinal canal without having much distended it, and descending into the scrotum, he remarks that "it is upon the entire canal that the pressure should be made, and especially towards the abdominal orifice (internal ring)." (p. 117.) This is satisfactorily effected by the last phalanx of the thumb; but then, as the slightest movement in either direction would suffice to permit the escape of the hernia, M. Malgaigne recommends that the pad should be half as long again as the canal, and half as wide again as the thumb: this surface he has found sufficiently extended for ordinary cases. There should likewise be convexity enough to preserve the walls of the canal in contact. Again, in dealing with an indirect hernia, in which the canal is much dilated, allowing of the ready escape of the bowel from the laxity of the walls of the canal, the small convex pads just described are useless, and obnoxious to the objection we have already quoted as noticed by Mr. Lawrence. In these cases M. Malgaigne employs pads of at least double the breadth of the thumb, "and having an ovoid convexity which is bevelled off toward the internal ring." (p. 119.) But, thirdly, a hernia may traverse the whole canal without causing much dilatation; yet, if it occur in an old man, the tissues may be rendered lax and weakened by age, and even more by obesity: in such cases our author thought that, as he had seen ruptures disappear in individuals nearly seventy years of age, that the small con-

vex pads already noticed might be as successfully employed as in the young. Experience, however, taught him that, on the contrary, these pads first produced absorption of the subcutaneous cellular tissue, and subsequently pressed too forcibly on the weakened aponeurosis, so as ultimately to produce farther distension in place of obliterating the ring. In such cases, therefore, he has found it requisite to employ, for the purpose of procuring a radical cure, very large pads, *i. e.* of three fingers' breadth, and four fingers' length, with a very moderate and uniform convexity. (p. 120.) Some cases, says M. Malgaigne, may, however, be met with which call for a peculiar treatment; "those, e. g., which have become almost or altogether direct:" then the ordinary pressure is of no avail; but a pad framed so as to press in the integument, in the same way as would be effected by one or more fingers, (the prominence in short being digitiform,) has been found efficacious, but they are only to be regarded as "an extreme means when all others have failed." With regard to the *form* of the pads adapted to different cases, M. Malgaigne acknowledges that he has been forced to renounce the circular, semicircular, and elliptic as useless; but that he has found a hernia retained by one of a very singular shape, after a fair trial had proved the inutility of the above. It is described as "*une pelote à bec de corbin*," (p. 122,) i. e. a triangular pad, of which the inferior and most acute angle "rests on almost the whole upper border (hauteur) of the pubis." He does not attempt to explain satisfactorily the modus operandi of this pad; but he says, "I am willing to be satisfied with facts, so that I repeat, you will not succeed in retaining very difficult ruptures with any but the beak-shaped pad, which should, moreover, present a very extended surface." (p. 123.) From a careful consideration of the form of the inguinal canal, in connexion with the influence of pressure, M. Malgaigne has been led to employ three different kinds or degrees of convexity in the construction of pads; of the first the form is elliptic, and the convexity uniform in every direction, by which means an uniformity of pressure is exercised over the whole extent of the canal; this pad is peculiarly applicable where the subject of the complaint is young, the aponeurotic tissues not very strong, and the muscular efforts moderate; a slight increase of convexity towards the centre produces a modification by which the pressure is more decided upon the middle of the canal. Secondly, an oval pad, the greatest convexity of which corresponds to the internal ring: the effect of this arrangement is obvious; and it is found best adapted for individuals in whom the tendinous aponeuroses are strong and resisting, and the muscular efforts considerable. Thirdly, the same as the preceding, but with the addition of the greater transverse convexity of the pad being approximated to the inferior border of the canal: the end proposed to be fulfilled by this form is that, in persons whose muscular development and violent efforts permit the parallel fibres of the transversalis and internal oblique muscles to separate the walls of the inguinal canal, and elevate the pad, sufficient pressure may be made at the inferior border of the former, so as to counteract the above-mentioned tendency, (p. 125;) in short, to concentrate the principal operation of the support to the interval between the lower border of the conjoined tendons and Poupart's ligament; an indication which we should think the contrivance in question well adapted to answer.

In the two succeeding lectures M. Malgaigne treats of the union of the pad and spring of the truss, discussing the relative merits of the fixed and moveable pads; and also notices the accessory appendages necessary to the securing of trusses: but we must pass over these subjects, and will briefly recapitulate, after our author, the directions which he has given for the selection of the appropriate means of support adapted to various forms of rupture.

"1. For a simple, inguinal hernia the best spring is the English spring, which embraces the body on the sound side. 2. If the hernia is oblique and easily retained, the moveable pad should be selected; but where the reverse is the case a fixed pad is preferable. 3. There should be always a decided interval between the extremity of the spring and the internal surface of the pad, whether it be procured by an intermediate pad, or only by augmenting the thickness of the compress itself. 4. It is on the form of the compress that the retention of a rupture principally depends. For oblique herniæ the pad should cover the whole canal, making pressure, according to circumstances, either directly upon the centre of the canal, or with an inclination towards the internal or external ring. 5. In direct herniæ the preference should be given to the triangular pad, (à bec de corbin.) 6. In very thin subjects, the spine of whose pubes is very prominent, and unable to sustain the pressure of the ordinary pad, that filled with air is a resource. 7. The leather strap and that which passes beneath the thigh should never be too tight. The rupture ought to be retained by the power of the spring, of which the other parts of the truss are only auxiliaries. 8. Every truss that allows the escape of the hernia does more harm than good. 9. If a truss hurt or incommode the patient it is bad; at the same time, it must be admitted that there are some herniæ which are extremely difficult to retain, and which therefore form an exception to the above rule, great constraint being the unavoidable result of the application of the necessary apparatus." (pp. 155-6.)

Lastly, before quitting the subject of trusses, we may notice the result of M. Verdier's experience with respect to the radical cures effected by their use at different ages. He thinks that, prior to the age of puberty, we may count upon a permanent cure in three fourths of the cases. During the early adult periods, provided the rupture be recent and the result of accident, about two thirds are relieved. As age advances the proportion decreases; so that, of subjects above forty years, not more than one in ten is radically cured. (pp. 150-1.)

Inguinal hernia in females is next discussed by M. Malgaigne. Nearly the whole lecture is occupied with the subject of the relative frequency of this form of the complaint in men and women; and we are not a little startled by the announcement, that it is more common in the latter than the former; i. e. that the relative proportion of inguinal to crural ruptures is greater in women than in men. This, we need scarcely observe, is so completely at variance with all our preconceived notions, and with, we believe, the experience of every accredited writer on the subject, that we felt not a little curious to see how M. Malgaigne disposed of the mass of evidence which he was certainly bound to face and undermine, if not overthrow, before he could expect credit for his own assertions and inferences: we will accompany him through his lecture for this purpose. After noticing the general opinion respecting the predominance of crural hernia in women, he addresses himself to an analysis of the testimony on which it is founded, viz. that of surgeons and truss-makers. First, with respect to operating surgeons: their evidence is, that almost all the cases

of strangulated hernia which occur in women are of the crural class. This fact is undeniable, and therefore undenied: "but what," says M. Malgaigne, "are we to infer from this? simply that crural ruptures are more liable to strangulation than others." He of course denies the propriety of including strangulated and simple herniæ in the same category, and with at least some show of justice. But then truss makers, who deal with reducible ruptures, also agree that those of the inguinal class are very rare in women, the largest proportionate calculation opposed to his theory being that of the London Truss Society. Of six hundred and ninety-three females with inguinal and crural herniæ, there were only forty-four belonging to the latter class—a proportion of one in fifteen. Now, if M. Malgaigne's assumption be correct, viz. that the compilation of these important tables was left exclusively to the judgment and opinion of the instrument makers exercised upon the individual cases, we perhaps might not be much disposed to quarrel with his inference regarding their value and credibility; but as we have reason to believe that such is not the case, we can hardly admit his summary method of dismissing this evidence by denying the capacity of those upon whose authority the statistical table was drawn up. But again, especial attention has been devoted to the subject by a M. Nivet, whom our author styles "a very distinguished *interne* of the Parisian hospitals;" and his calculation is still in favour of the prevalence of crural hernia, in the proportion of sixty-seven to forty. All that M. Malgaigne says to this evidence is, that it is more in his favour than the generally-received opinion, and hints that the calculation was undertaken to verify the old notion, and also that the period of life at which the cases were examined was more favorable to the existence of crural ruptures. But his toughest opponent he acknowledges to be M. J. Cloquet, whose calculation was made from examination of the dead body: of 121 crural and inguinal herniæ in women, there were but 42 of the latter class. It would indeed appear impossible to get over this obstacle to the establishment of his own doctrine; but M. Malgaigne, nothing daunted, denies the correctness of M. Cloquet's calculation, on the ground that he included every crural hernia, however small, and whether only commencing or obliterated! We may lastly remark, that the combined authority of all our other authors is opposed to M. Malgaigne. M. Verdier says, "inguinal hernia, although *very rare* in women," &c. (p. 28.) Vidal admits that the proportion of inguinal herniæ in females would probably be augmented if the diagnosis were always correct; for in cases of enlarged and relaxed external ring, the hernial sac may be directed over Poupart's ligament, so as to occupy the position of a femoral hernia, instead of the more ordinary course into the labium pudendi. (pp. 12-13.) Mr. Lawrence refers to the tables already noticed, as well as to those of M. Monnikoff, who found, in 885 individuals affected with inguinal hernia, that only 175 were females. (p. 229.) Before we conclude our remarks on this subject we must, in justice to M. Malgaigne, give the result of his own (as we have no reason to doubt) impartial examination of the subject. His first observations were made in 1835, when, much to his own surprise, he found that 54 out of 62 females (with the two forms of rupture in question) had inguinal hernia! This proportion, he acknowledges, exceeds any that his more recent observations have given him;

but still he is borne out in his conclusion, that "inguinal is more frequent than crural hernia in women." (p.178 et ante.)

With regard to the predisposing and determining causes of inguinal rupture in females, the same author remarks, that the comparative narrowness of the peritoneal prolongation in the inguinal canal, (known as the canal of Nuck,) renders females less liable to the complaint than males—the proportion being about one fourth. This is a little augmented in the first two or three years, subsequent to which period, until about the twentieth, the fresh cases are rare, though the crural form is even more so; and even after this period, when the important influence of childbearing is taken into account, and the gross number of herniæ greatly increased, still the inguinal are predominant. As a disposing cause to rupture generally, M. Malgaigne considers that pregnancy absorbs all those circumstances and special influences which we have already noticed in connexion with this disease in males. The relative number of accidental and spontaneous inguinal herniæ he has found about the same in both sexes. One remark which our author makes in reference to trusses for females is worth extracting: where the spinous process of the pubes is very prominent, a special depression on the pad for its reception will be found very efficacious in aiding its proper adaptation:

We pass by the "interstitial or incomplete inguinal hernia," (i. e. where the sac remains in the inguinal canal,) which is more particularly treated of by Mr. Lawrence (p. 220) and M. Vidal, (p. 13,) the authority of M. Goyrand being quoted as having given the fullest account of it in the fifth of the *Mémoires de l'Académie de Médecine*, 1836; but the crural form of rupture will require a short notice at our hands.*

A lecture is devoted by M. Malgaigne to the consideration of the diagnostic marks by which crural and inguinal hernia may be distinguished. He first of all quarrels with all the methods hitherto recommended, before he proceeds to establish his own more perfect system; and first of Sir A. Cooper's signs, viz. 1, The position of the sac's neck above the pubic spine in inguinal hernia, and below and external to it where the rupture is crural; 2, The position of the crural arch in relation to the neck of the sac. With respect to the latter, our author considers it only applicable where the parts are exposed by operation; and as to the former, he thinks that, from the dragging of an inguinal hernia on the inferior pillar of the ring, the neck of the sac is frequently found *beneath* the spine of the pubes; and he denies that it is usually internal to the same point, (an assumption, by the way, of his own, that Sir A. Cooper has stated the affirmative;) and further makes the captious and foolish remark, that incomplete inguinal herniæ seem to have escaped Sir Astley's notice. It would not require a conjuror to detect that these were not crural: but, to proceed. The diagnostic signs of M. Amussat he thinks nearly as fallacious. This surgeon recommends that a line be drawn from the anterior superior spine of the ilium to the pubic spine, and that all herniæ above this line be set down as inguinal, and all below it as crural herniæ. We need not stop to show the fallacious nature of this

* The characters of this form of hernia have been described and delineated by Sir A. Cooper in the first part of his work: a fact which Mr. Lawrence remarks might have saved MM. Goyrand and Dance their reclamations on the subject of priority of discovery in 1835.

test. M. Nivet, prior to his researches already noticed, established a modification of the above, viz. that of tracing Poupart's ligament from the pubic to the iliac spine, and then classing the ruptures according to their relative position: in truth, says M. Malgaigne, this is a sufficient test, *if* you can trace the ligament in question. But it is time that we examine the plan of proceeding which is to obviate all these difficulties.

"Suppose then," says M. Malgaigne, "a hernia of the most equivocal character, a tumour in the midst of the groin, but a little approximated to the pubes, projected on coughing, returnable by pressure, but so suddenly as to defy detection of the point of disappearance," &c., &c. [In short, suppose a case which none of the tests already enumerated would suffice to determine, then the following is the rule of proceeding:]

"You return the rupture; you discover with the right index finger the pulsations of the femoral artery; and, applying the extremity of the finger to the inner side of this artery, you must press backwards until you feel the pubes. Sometimes, in thin persons, you may feel at the same time the crural ring open, with its boundaries of Poupart's ligament in front, the os pubis behind, and femoral artery externally,* then you need proceed no further; in the normal condition the finger can never penetrate thus into the crural ring. But suppose you are dealing with a fat subject and a very small hernia, and that the ring is too deep and narrow to admit the finger, you must press on the pubes, feeling the pulsating artery on the outer side, and then direct the patient to cough. If the impulse is felt against the finger, and the rupture, nevertheless, does not descend, you may conclude that it is crural hernia; but if there is no such impulse, the hernia, on the contrary, making its exit above, you may be satisfied of its inguinal character. But, again, the exit and impulse spoken of may take place together; then, whilst the right index finger is closing the crural ring, place the left thumb transversely about three lines above it, and direct the patient to cough: if the rupture be inguinal, it will be retained; but if crural, it will descend so as to leave no doubt of its character." (p. 188 et seq.)

The other difficulties which present themselves are met, and are said to be obviated by corresponding instructions, which our space will not permit us to transcribe. We leave our readers to judge of the value of these directions, which can only be applicable where the hernia is reducible; where the reverse is the case, our author admits that the surgeon must be guided by tracing the sac to its point of exit.

Of the predisposing causes to this form of hernia, M. Malgaigne reckons frequent pregnancy amongst the most influential, (p. 194:) and he states the results of his experience to be opposed to that of Sir A. Cooper; M. Malgaigne having found that the greater proportion of the patients whom he has examined, attribute their disease to a sudden effort or a blow received on the abdomen. (p. 196.) It is in such cases that he has found the stricture formed by the ruptured cribriform fascia; indeed, he denies, in this as well as inguinal hernia, that the rings have anything to do with the strangulation. (p. 198.) We are, by no means, disposed to pin our faith on M. Malgaigne's experience on this point. He thinks the ordinary method of distinguishing between inguinal hernia and varicocele insufficient to distinguish between the latter disease and crural hernia; he therefore directs that, instead of returning the rupture, pressure should be made upon it, so as to fix it against the pubis, and then that the tumour should be pressed with the thumb of the

* We presume M. Malgaigne has not forgotten that the *vein* forms the external wall of the ring?

other hand. Under this manipulation the varicocele will disappear, the blood flowing into the inferior veins, whilst the reverse would be the case with a hernia. (p. 204.) We find, on reference to M. Verdier's tables, that he gives, as the proportion of cases of crural hernia which have fallen under his notice, one hundred in females, and fourteen in males. (p. 240-1.) As to the palliative and radical cure of this form of rupture, M. Malgaigne denies the possibility of the former, and admits the difficulty of applying any form of truss which shall be fully efficacious even in retaining the protruded intestine; it should be as accurately adapted as possible to the form of the groin, and sufficiently prominent to make pressure directly upon the crural ring.

M. Malgaigne does not seem able, from his own experience, to throw much light on the statistics of umbilical hernia. It would appear that the tables, which have been compiled on the most extensive scale, do not agree with the results obtained by Sir A. Cooper, who found this form of rupture the most common, inguinal excepted. Such, likewise, appears to be the conclusion to be drawn from M. Verdier's tables, who gives as the relative proportions of crural and umbilical hernia 156 of the latter and 114 of the former; 97 of the umbilical ruptures occurring in women, and 59 in men. M. Malgaigne was at first inclined to this opinion also, but subsequent experience has disposed him to coincide with the tables of Monnikoff and others. In regard to the age at which this form of hernia occurs, M. Malgaigne's results are curious and interesting. Whilst in the mass these ruptures are more common in women than men, he has found that the proportion is reversed according to the period of life: thus, from birth to the age of thirty, there are twice as many cases in males as in females, the converse holding good from the latter age to eighty: the explanation given of this apparent anomaly, is that the newly-born of the male sex are "enormously more subject to the complaint than females; he believes they are sometimes strictly congenital." (p. 223 et ante.) The fallacious impression which is occasionally received regarding the exact position of an umbilical rupture when returned, M. Malgaigne attributes to the firm cicatrix resulting from the union of the obliterated vessels and urachus. Speaking of the cure of these cases, he recommends the employment of a compress which must be accurately fitted *into* the umbilical depression so as effectually to *cork* it up: the size of this plug must be gradually reduced as the annular opening diminishes, and it should be confined by adhesive plaster carried twice and a half round the body. (p. 232.) A ball of caoutchouc he has found to answer the purpose very well. Lastly, of ruptures escaping by the linea alba, M. Malgaigne has never seen them uncomplicated with other herniæ, and he believes it to be rarely the case that they exist alone. The largest that had occurred in his practice, was equal in size of a man's head. The radical cure of them he considers hopeless. In the other and more rare forms of rupture, such as lumbar, obturator, &c., our author has had no experience.

In the preceding analysis, we have unavoidably omitted much relating to the subject of hernia, although we have followed M. Malgaigne pretty closely through the pages which compose his lectures; but it would be idle to attempt to touch upon everything within the space allotted to our task; we shall, therefore, confine our closing remarks to

a few points of interest connected with the treatment of strangulated hernia generally. Mr. Lawrence's observations on the various means recommended and employed to aid the taxis in these cases, are so excellent, so essentially practical, that we turn to them with confidence that we shall have offered to us nothing but the sound advice of an able and experienced observer. With respect to *purgatives*, he remarks, "that experience has taught him to repose very little confidence in them: they are not only inefficacious, but actually prejudicial, in the inflammatory strangulation; but in large and old herniæ, where an accumulation of fecal matter, from torpor of the intestine, is the cause of strangulation, and the symptoms are of the chronic kind, purgatives may be employed with success, and those of an active description," &c. Again "an omental hernia is another exception to the general doctrine on the subject of purgatives: if we can clear the intestines completely, the operation will seldom be necessary." (p. 160 et ante.) In speaking of that powerful agent, the *tobacco enema*, Mr. Lawrence quotes the contradictory opinions and results of the practice of different surgeons: thus, M. Velpeau, after having been once successful, tried it in twenty-five cases without advantage; whilst Mr. Key thinks that the objections raised against it are owing to a want of a due precaution in proportioning the dose to the power of the patient. Mr. Lawrence himself says, "we cannot speak so favorably of the safety, as of the power of this remedy, which has destroyed life in some instances, and probably been injurious in others.... Venesection is preferable to it in the early period of strangulation, while the great depression of the vital powers makes us hesitate to use it in a more advanced state of the malady." (pp. 164-5.) Of the dashing cold water on the patient, Mr. Lawrence thinks but lightly; but he ranks the topical application of cold as inferior in efficacy only to venesection and the tobacco, in the treatment of strangulated hernia.

As M. Verdier is a strong advocate for the use of cold effusion in the cure of hernia, we should not do him justice without noticing his observations on the subject. Assuming the production of hernia to be dependent for the most part on the relaxed condition of the muscular and tendinous structures, forming the abdominal parietes, he considers the *douche* bath as peculiarly applicable in restoring their wonted tone. His attention having been called to the efficacy of this remedy by a case of strangulated hernia which was thus reduced under the care of Petit, he employed it without effect for some time in recent cases of reducible hernia; but further experience induced him, after neglecting it for a long period, again to have recourse to the douche, which he now administers in the following way:—"Experience," he says, "has taught me that the treatment of ruptures by cold affusion should be continued for at least twenty days, during which, the patient should each day, and without interruption, receive the douche in the morning." Further on he adds, that the column of water should be directed on the part affected for sixteen or eighteen minutes continuously. If the hernia be scrotal, it should be thrown upon the inside and middle of the thigh of the same side, in such manner that the scrotum and groin may get the benefit of the reflected jet which would thus fall in a sheet over these parts. When the inguinal canal and rings have recovered sufficient tone to retain the hernia, the douche may be applied to the abdomen generally. The

directions for the treatment of other forms of rupture are similar; but it is impressed on us that the column of water should not be thrown point-blank on the affected part, but at an inclination of about forty-five degrees. The beneficial effects of this treatment are illustrated in the eleventh section of the third chapter of his work, in which several cases of different forms of hernia are described as radically cured by it. (p. 213, et ante.)

In the after-treatment of strangulated hernia, which has been subjected to operation, Mr. Lawrence justly dwells on the great importance of procuring a free evacuation of the bowels by gentle purgatives; and remarking upon the persistence of inflammation when once established, even though the exciting cause be removed, he adds that we should be prepared with the necessary antiphlogistics to control this common cause of the fatality of these cases. It is these observations which lead us to notice a form of treatment which we have seen attended with most satisfactory results, viz., the exhibition of small doses of calomel and opium, combined as occasion may require with gentle laxatives or enemata, and repeated every one, two, or three hours, according to circumstances. We do not mean to advocate this treatment to the exclusion of bloodletting, local or general; but we feel confident that the beneficial control exercised over the circulating system by the above combination tends greatly to arrest or even prevent the onset of peritonitis, as well as to promote a healthy action in the intestines, and to subdue general nervous and vascular excitement.

In concluding our notice of the volumes before us, we ought perhaps to apologise to our readers for the length of the article in proportion, (to retain our statistical phraseology,) to the paucity of new facts; a deficit which has also struck us, considering the pretensions of our French brethren. But in truth, former investigators (and we justly rank Sir A. Cooper amongst the first,) have left but little for us to add in the anatomy, and we may almost add surgery, of this branch of practice. With respect to the relative merits of our authors we may remark that, with a large share of vanity even for a Frenchman, and no lack of self-sufficiency in the execution of his task, M. Verdier gives us in his volume much that is useful, though in a vastly more bulky form than was essential. M. Malgaigne has handled his subject as if he intended and expected to have thrown a new light on almost all he touched; but, whatever may have been the impression on his numerous audience, both young and old, students and practitioners, we cannot say that we have been startled by any very remarkable discoveries; but, on the contrary, it has continually occurred to us in collating his works with Mr. Lawrence's to observe the remarkable coincidence in opinions, authorities selected, and practice recommended, between the two authors. We trust we do not make this observation either invidiously or uncharitably; if the authority had been mentioned we should have esteemed the sound judgment of M. Malgaigne in following our countryman; but not having once met with Mr. Lawrence's name in the lectures of the French professor, we cannot help concluding that he is either culpably ignorant of our medical literature, or that the coincidence in question is very singular. Of Mr. Lawrence's copious, systematic, and comprehensive work, we can only reiterate that it is all the student and practitioner can desire.

Art. IX.

Quid faciant Ætas annique tempus ad frequentiam et diuturnitatem Morborum Hominis adulti, Inquisitio Medico-statistica. Auctore E. Fenger, l. m. Univ. Havn.—*Harniæ*, 1840. 8vo, pp. 71.

On the Influence of Age and Season on the frequency and duration of the Diseases of adult Males. By E. Fenger.—*Copenhagen*, 1840.

The importance of all topics connected with public hygiène becomes more appreciated as each succeeding year adds to our stock of information on the subject. The Registrar-general's Reports on the mortality and fatal diseases in civil life, and the Statistical Reports on the Health of the Army and Navy, have recently furnished very extensive data for the elucidation of many controverted points in medical science; but as the former of these refers generally to all classes, and the latter to persons whose occupation and habits differ materially from those of civil life, no accurate deductions can be drawn from them on a subject of vast importance in a commercial country, viz. the health of the labouring classes.

Considering the large bodies of workmen employed by government and various public companies throughout the kingdom, there should be no great difficulty in supplying this desideratum, provided the superintending medical officers of these establishments would carefully register the cases of sickness, and keep accurate returns of the number and ages of those employed. Some interesting information on this subject, obtained from the records of the East India Company's labourers in London, and the principal government dockyards, was published in the supplementary report of the factory commissioners; and we have now to notice a valuable contribution of a similar nature, on the health of the workmen in the royal naval dockyard at Copenhagen, by Dr. Fenger, the medical superintendent of that establishment, a production which does great credit to the industry and discrimination of the author. The object of the present article is to bring the facts adduced by him into comparison with such data as we possess regarding the same classes of workmen in this country; and we hope that our notice may excite the attention of those who possess similar opportunities of collecting information and induce them to make the results of their observations known to the public. Before entering on this comparison, however, it will be necessary to say a few words regarding the description of persons over whom Dr. Fenger's investigation has extended.

The workmen in the royal naval dockyard at Copenhagen are reared at the public expense. Boys are selected at the age of ten, and sent to school, where they are instructed in reading, writing, arithmetic, and the principles of religion. At fifteen they begin their apprenticeship in the dockyard, and on its completion are, at about the age of twenty, enrolled among the workmen, and thenceforth receive pay. They are principally carpenters, smiths, ropemakers, and sailmakers. After twenty years' service they are entitled to their discharge, which, however, is seldom demanded. At the end of this period, unless rendered unfit by vice or negligence, they are, if they wish it, again enrolled; and when at length, from age or disease, they have become unfit for labour, they are transferred to an invalid establishment, and employed in any light work they may be able to perform about the dockyard.

These men are under the same discipline as soldiers, and are governed by military law. They are well paid, housed, and fed. The part of the city in which they reside is appropriated exclusively to themselves; their houses are all similar, being built of stone, two stories high, and occupied by two families who have each two rooms and a kitchen; the upper story has a wooden and the lower a stone floor; to each house is attached a yard, but none have cellars underneath them, which makes the ground floor damp in some cases; the streets are wide, straight, and always kept clean. The men are visited at least once a month by their officers, who see that the houses and streets are clean and in good order. Their food is ample and of the best quality, consisting of flour, peas, meat, butter, &c. They receive also a sufficient quantity of firewood. Their working hours are from daybreak to four p.m., with an interval of half an hour; but from the 19th December to the 20th January they do not attend at the dockyard at all. The men are stated to be rather small, not very muscular, and the muscles of their lower extremities in particular badly developed; most of them marry young, and many are of very dissipated habits. Their number averages about 1240. If attacked by sickness they are sent to the hospital, where they remain till sufficiently recovered to resume their labour; but as a small sum is stopped from their pay, many are unwilling to report themselves sick, unless either unfit for work or afraid of the disease becoming serious. The name, age, trade, disease, and dates of admission and discharge or death of every patient are entered in a register; and as this has been carefully kept since 1825, it affords much valuable statistical information, which has formed the groundwork of the publication under review.

Copenhagen, to the medical statistics of which the present observations more particularly refer, is situated on the east coast of Zealand, in 55° 42′ N. lat., 12° 34′ E. long., and is divided into three parts,—the Old Town, the New Town, and the Port or Christian's Haven. The last is built on the island of Amager, between which and the rest of the town runs a narrow inlet forming the harbour. The walls of the city enclose a circuit of five miles, and the population amounts to about 116,000. The climate of Copenhagen is said to be damp and unhealthy, and the mortality to be greater than in any other town in Denmark.

The total deaths among the workmen in the royal naval dockyard during a period of eleven years (1829-39) have been as follows:

Age.	Years of life.	Died in each period of life.	Ratio of deaths per 1000 living.	Age.	Years of life.	Died in each period of life.	Ratio of deaths per 1000 living.
15-20	1727	4	2·3	55-60	907	49	54·
20-25	1738	7	4·	60-65	677	56	82·7
25-30	1528	11	7·2	65-70	369	48	130·1
30-35	1509	15	9·9	70-75	296	47	158·8
35-40	1372	23	16·8	75-80	215	35	162·8
40-45	1172	25	21·3	80-85	102	24	235·3
45-50	1012	26	25·7	85-90	32	8	251·
50-55	964	36	37·3				
				Total	13620	414	30·4

In the following table we shall compare this mortality with what occurs among the male population of Denmark and Sweden, and the inhabitants of Copenhagen at the same age. As these deaths, however, are

stated in decennial periods, the ratio among the dockyard workmen has been recalculated, so as to include corresponding terms of life in each case.

Age.	Ratio of deaths per 1000 living			
	Sweden.*	Denmark.†	Copenhagen.†	Among dockyard workmen.
21-30	9	9	11	6
31-40	13	12	20	14
41-50	19	20	35	24
51-60	30	31	53	50
61-70	58	55	85	100
71-80	117	107	148	173
above 80	252	216	241	231

From this table it appears that the ratio of deaths among the dockyard labourers is lower than among the male inhabitants of Copenhagen till the age of sixty, when the reverse is observed. It is higher, however, at all periods of life than throughout Denmark or the adjacent kingdom of Sweden ; but as the population of these states is chiefly rural the excess may be accounted for by the difference which is always found to exist between the mortality of town and country districts.

As a means of comparison with a similar class of workmen in this country the following table is submitted, showing the ratio of mortality among the East India Company's labourers in London, a body of men very similar to the Copenhagen dockyard workmen, though probably neither so well fed nor housed.

Age.	Ratio of Deaths per 1000 living among Copenhagen Dockyard Workmen.	Ratio of Deaths per 1000 living among East India Company's Labourers.‡	Age.	Ratio of Deaths per 1000 living among Copenhagen Dockyard Workmen.	Ratio of Deaths per 1000 living among East India Company's Labourers.
15-20	2·3	not stated	50-60	45·4	42·7
20-30	5·5	8·2	60-70	99·4	92·4
30-40	13·2	14·8	70-80	160·5	107·1
40-50	23·4	24·3	80-90	238·8	139·
			Total	30·4	31·3

Thus, with the exception of the period of life from twenty to thirty, in which the Copenhagen workmen have rather the advantage, a remarkable similarity will be found to exist in the mortality up to the age of sixty, when the comparison begins to be materially in favour of the East India company's labourers. The Copenhagen workmen are described as much addicted to the use of spirituous liquors, a circumstance to which the rapid increase of mortality after the age of fifty is probably attributable, as the same feature has been observed among the older classes of soldiers in the British army at stations where intemperance greatly prevails. This point may be illustrated by the following remark in the report on the health of the troops at the Cape of Good Hope. "There seems good reason to believe that a peculiarity thus confined to the troops alone (viz. the rapid deterioration of constitution at the higher ages) may

* Mean of twenty years, 1811-30. † Mean of five years, 1832-6.

‡ M'Culloch's Statistics of Great Britain, vol. ii. Second edition. London, 1839.

in some measure be attributable to habits of intemperance, which, though they add little to the mortality of the youngest class, are likely, if persisted in, to sow the seeds of diseases which develop themselves more fully as the soldier advances in life." (p. 23.)

The extent of sickness and the diseases by which the admissions and deaths among the Copenhagen workmen have been occasioned, are shown in the following table.

Average annual Strength, exclusive of Masters, Foremen, &c. 1100.	Annual average of diseases treated.	Annual average of deaths.	Ratio per 1000 annually. Treated.	Ratio per 1000 annually. Died.
Fevers	125·9	2·7	114	2·5
Diseases of Brain and Nervous System	17·4	2·	16	1·8
,, Windpipe and Chest	69·7	9·5	63	8·6
,, Digestive Organs	45·7	4·	42	3·6
,, Urinary & Genital Organs	19·		17	
,, Integumentary System	87·		79	
,, Fibrous System	46·5	5·4	42	4·9
,, Eyes and Ears	7·1		11	
,, Nose and Throat	5·			
,, Uncertain seat	15·		14	
,, Drunkards	8·8	1·3	8	1·2
Wounds and Injuries	93·6	1·2	85	1·1
	540·7	26·1	491	23·7

Such of the invalids as are wholly unfit for work are not sent to hospital when sick, but are attended by the medical officer in their own houses. Records of the cases among them have been kept, however, for the last four years, from which data, combined with the admissions into hospital for fifteen years, the average number of attacks occurring in one year among all the workmen has been obtained as above stated.

Before making any remarks on the relative prevalence of the different diseases among these workmen, we shall compare the extent of sickness and mortality with what occurs among similar classes in England. This comparison, however, extends only to the Copenhagen workmen under the age of fifty, as after that period of life the results are materially affected by the number of invalids. The cases of wounds and injuries have also been deducted, because they are omitted in the returns of the English dockyards.

	Copenhagen Workmen under 50 1825-39	Portsmouth Dockyard. 1830-32	Plymouth Dockyard. 1829-31	Woolwich. Dockyard. 1830-32	Chatham Dockyard. 1830-32	Sheerness Dockyard. 1830-32	Pembroke. Dockyard. 1830-32	English Dockyds. Cases of Sickness	English Dockyds. Deaths from Disease.
Strength	12525	5948	6186	2233	3941	1422	1338	18835	15705
Attacks of Sickness	4694	2250	2145	......	1939	622	701	7657	......
Deaths from Disease	122	56	51	22	......	......	7	......	136
Ratio of cases per 1000	375	378	347	......	492	437	523	406	......
Ratio of Deaths ,,	$9\frac{7}{10}$	$9\frac{4}{10}$	$8\frac{2}{10}$	$9\frac{9}{10}$	......	......	$5\frac{2}{10}$	......	$8\frac{7}{10}$

The similarity in the amount both of sickness and mortality in the Danish and English Dockyards is very striking, and affords an illustration of the value of such investigations. Wherever we have accurate records of the health of a body of individuals comprehending a sufficient number to obviate any casual irregularity, we may safely make deductions not only with regard to the same body of men in future, but also as to any others similarly situated. On this principle Benefit Societies have been founded, and by extending similar observations over various classes of workmen, as in the present instance, the calculations in regard to these valuable institutions may be placed on a much more accurate basis than has hitherto been found practicable.

In examining the preceding table it must be kept in view that from the 19th of December to the 20th of January the Danish workmen do not attend at the dockyard; and that many if unwell during this period remain at home, and, to avoid being placed under hospital stoppages, do not report themselves to the surgeon. Could we make the necessary allowance for that source of error, the ratio of sickness would be found almost exactly the same as the average of the English dockyards.

We shall now proceed to make a few observations on the various classes of diseases by which this mortality and sickness have been occasioned, comparing the results, where practicable, with those obtained by similar observations in this country. For this purpose we have arranged the principal diseases among the cavalry in the United Kingdom and the members of the Liverpool Friendly Society in the same form as that adopted in the work before us, in the following table.

	Liverpool Friendly Society, 1829-30. Years of life 4615		Dragoon Guards and Dragoons in United Kingdom 1830-36. Aggregate strength 44611.				Copenhagen Dockyard workmen.	
	Cases treated.	Ratio per 1000 of strength	Total cases treated.	Total deaths.	Ratio of sickness per 1000 of strength	Ratio of deaths per 1000 of strength	Ratio of sickness per 1000 living.	Ratio of deaths per 1000 living.
Fevers	338	73	3444	66	77	1·5	114	2·5
Diseases of Brain and Nervous System	117	25	283	29	6	·7	16	1·8
,, Chest, &c.	435	94	6674	363	150	8·1	63	8·6
,, Abdomen	441	96	4845	57	108	1·2	42	3·6
,, Fibrous System	258	56	2252	112	50	2·5	42	7·2
,, Drunkards	9	2	27		$\frac{6}{10}$		8	
Wounds and Injuries	526	114	5619		126		85	
All other diseases	392	85	18320		411		121	
	2519	545	41464	627	929	14·	491	23·7

Fevers. The prevalence of fever among the Copenhagen workmen is considerably greater than among the cavalry in this kingdom or the Liverpool artisans. Of 1877 cases, 902 are stated to have been continued and 706 intermittent; the rest were most probably eruptive fevers. Intermittents are generally rare at Copenhagen, but for six years (1827-32)

that disease prevailed annually in an epidemic form, beginning in March or April and ending in July or August, for which peculiarity no cause could be assigned. Typhus has been included with common continued fever, so that there are no means of ascertaining how many cases of it occurred. The result of Dr. Fenger's observations shows that young men are most obnoxious to intermittents, and that the liability diminishes with the advance of life, while the liability to continued fever reaches its maximum between 20 and 30, and then follows the same rule. By deducting these two classes from the total nearly all the rest, supposed to be eruptive fevers, are found to occur between 15 and 30. But while the prevalence of fever diminishes with age both the mortality and intensity increase—a slight irregularity in this respect between the age of 40 and 50 arising probably from the smallness of the numbers. These results coincide with the observations made on the subject in this country by Dr. A. S. Thomson in his Statistical Inquiry on Fever. (Ed. Med. and Surg. Journal, No. 136.)

Diseases of the brain and nervous system. The principal diseases comprised in this class are headach, apoplexy, paralysis, convulsions, and insanity. Their prevalence among the Copenhagen workmen is greater than among the cavalry in this kingdom, but less than among the Liverpool artisans. The mortality is very high, but this is chiefly among the older men, the ratio among those under 50 corresponding very nearly with that of the cavalry who are also under that age. The proportion of cases will be found to increase considerably with age, for out of every 1000 living at the following periods of life, there were attacked annually,

Age	15-20	20-30	30-40	40-50	50 and upwards.
Attacked	4	8½	18	15	29

The intensity of these diseases follows the same law; for while only 1 case in 24 proved fatal under 50, 1 in 5 died above that age.

Diseases of the windpipe and chest. Under this head we apprehend that so many other diseases are comprised with those of the lungs as materially to vitiate the accuracy of any conclusions we might draw from the preceding results; and unfortunately we possess no means of rectifying this by reference to the specific diseases whereby the admissions and deaths have taken place. Their very great prevalence and mortality among the old compared with the younger classes of workmen, in particular, induces the belief that there has been some important error in the principles of classification, as may be seen by the following results.

	15-20	20-30	30-40	40-50	50 & upw.
Ratio of attacks per 1000 of strength	24	42	56	69	112
Ditto of deaths per ditto	1$\frac{1}{10}$		4$\frac{2}{10}$	9$\frac{3}{10}$	24$\frac{7}{10}$
Proportion of deaths to attacks	1 in 34		1 in 13	1 in 7½	1 in 5

This is so different from what has hitherto been observed in regard to the influence of diseases of the lungs that we fear not only have diseases of the heart been included, but also hydrothorax and other affections of a similar nature, which so often attend a breaking up of the constitution.

We are confirmed in a belief of the existence of some important error by the circumstance, that the mortality from all diseases of the chest between fifteen and thirty is lower than that arising from phthisis alone among the population at the same age of any country of which we have authentic statistical records; and the admissions into hospital are equally below the usual average. In the absence then of detailed information it were useless to speculate on the cause of this exemption, but we trust that our author will minutely investigate the subject and make known the circumstances under which it has taken place, or, if founded in error, that he will at an early period make the necessary corrections in his statement.

Diseases of the digestive organs. This class comprises the diseases of all the viscera contained in the abdominal cavity except the kidneys and bladder. The Danish workmen appear to enjoy very considerable exemption from diseases of this class, but which may partly arise from the slight cases not coming under medical treatment. Though they may be much less prevalent than among persons in this country, however, these diseases are of a more fatal character when they do occur, for the mortality from them among persons under fifty is almost exactly the same as among the dragoon guards and dragoons, while above that age it is much higher. The liability to them, as in the case of the diseases already investigated, increases with the advance of years, for out of every 1000 men the proportion of cases at each decennial period was

Age	15-20	20-30	30-40	40-50	50 and upwards.
Attacked	18	31	38	43	68

The intensity of the diseases also increases with age, for the proportion of deaths to attacks under 50 is only 1 in 29, while above that period it amounts to 1 in $6\frac{3}{4}$.

Diseases of the fibrous system. This class corresponds very nearly with that of rheumatic affections in the military statistical reports. Under the age of 50 rheumatic affections are less prevalent among the Copenhagen workmen than either among the artisans or the cavalry, although the climate, as already stated, is very variable and damp, with frequent fogs. The influence of age on rheumatic affections is as follows:

Ages	15-20	20-30	30-40	40-50	above 50
Attacked per 1000 of strength at each age.	18	26	33	58	74

Thus the prevalence of these affections increases regularly with the advance of age, as is the case with many of the others.

Diseases peculiar to drunkards. We are not aware of any disease which can be classed under this head except delirium tremens. Assuming then that nothing but this disease has been included in the Danish returns, the following comparative statement of its prevalence among the Copenhagen workmen compared with the military in the British colonies furnishes strong corroborative proof of the statement already made regarding the very intemperate habits of the former.

	Ratio per 1000 of strength. Admitted for delirium tremens.	Ratio per 1000 of strength. Died.	Proportion of deaths to admissions.
Dragoon Guards and Dragoons in United Kingdom	·6	·09	1 in 6⅔
Gibraltar . . .	·7	·08	1 in 8¾
Malta	·9	·12	1 in 7¾
Ionian Islands . . .	2·7	·43	1 in 6½
Bermuda	8·6	·77	1 in 11⅓
Nova Scotia, &c. . . .	4·9	·41	1 in 12
Canada	4·8	·30	1 in 16½
Cape of Good Hope . .	·6	·13	1 in 4⅓
Mauritius	16·8	1·64	1 in 10¼
Windward and Leeward command .	16·5	2·02	1 in 8⅓
Jamaica	3·7	·81	1 in 4½
Ceylon	·8	·16	1 in 5
Tenasserim Provinces . .	5·1	·73	1 in 7
Copenhagen Workmen under 50 years .	7·3	1·28	1 in 5⅔

From this table it appears that delirium tremens prevails to a greater extent among these workmen than among the troops in any of the British Colonies except the Windward and Leeward Command, Mauritius, and Bermuda. Nor does this excess seem to arise from the admission of many slight cases, as the intensity of the disease is greater than in any of the colonies except the West Indies, Cape of Good Hope, and Ceylon. It is also greater than in the United States, for of a series of 69 cases published by Dr. Ware of Boston, N. A., 11 died, being in the proportion of 1 in 6¼ cases.

The influence of age on this disease is very remarkable.

Ages.	15-20	20-30	30-40	40-50	50 and upwards.
Attacked per 1000 strength	$\frac{4}{10}$	3·	$7\frac{9}{10}$	$20\frac{8}{10}$	$10\frac{1}{10}$
Died per ditto	$\frac{15}{100}$ (15-30)		$\frac{9}{10}$	$5\frac{1}{10}$	1·

The diminution after 50 is attributed to most of the notorious drunkards having died off before that age, but it may perhaps also arise from the invalids having it more in their power to conceal their habits, and to the effects of intemperance being experienced in a less acute but no less fatal form in advanced life. This supposition is rendered probable too by the great increase in the prevalence and still more in the mortality by "diseases of uncertain seat," after 50 years of age.

Other classes of diseases. We have but few remarks to offer on these classes of diseases. They consist principally of syphilis and skin diseases. With regard to the first of these there is great difficulty in finding any fair standard of comparison, because the dockyard workmen rarely go into hospital for simple gonorrhœa or chancre, almost the only cases referred to under the head of "diseases of the genital organs," being such as are complicated with buboes, phymosis, or swelled testicle; whereas, among the military, even the most trifling case comes under treatment. On the other hand the members of the Liverpool Friendly Society are not entitled to medical attendance for diseases brought on by their own vices. Indeed, with regard to all the other classes of diseases also, it must be borne in mind that every case among the military appears in the returns, while many circumstances concur to prevent any but severe cases being noticed among civilians, a circumstance which must alway

have a material influence on the results, and prevent their being viewed in any other light than as an approximation to the truth.

Under diseases of the integumentary system, among the Copenhagen workmen, are included 370 cases of boils and abscesses, 373 ulcers, and 309 cases of itch, leaving 210 as the amount of other diseases of the skin and glandular swellings. Almost all the "diseases of uncertain seat" occurred among the men above 50; consequently the results under that age are those which can be referred to with the greatest safety, particularly as a means of comparison with the military.

The proportion of wounds and injuries is low, forming only one fifth of the whole cases, while in the Portsmouth dockyard it amounted to one third, but at Copenhagen the workmen labour only eleven months in each year, and the amount must also be materially influenced by the relative bustle and activity in the different dockyards.

As it is of considerable importance to ascertain the duration of sickness, and the proportion constantly sick in a given number of persons, with the view of furnishing data on which the tables of benefit societies and sick clubs ought to be framed, we subjoin the following statement of the duration of each attack among the Copenhagen workmen compared with English observations on the same subject.

Age.	Average duration of each case.	
	Copenhagen Labourers.	East India Company's Labourers.
15-20	19·4	14·
20-30	18·9	18·
30-40	17·4	22·6
40-50	20·	23·2
50-60	22·4	28·6
60-70	26·6	29·1
70 & upwards.	31·4	31·8
Age uncertain	32·8	

After the age of 30 the duration of sickness is greatest among the East India company's labourers, owing probably to a smaller proportion of slight cases coming under treatment. Among the Copenhagen workmen it is materially increased between 15 and 30 by the cases of syphilis which, as already stated, seldom come into hospital till complicated with buboes, &c. thus rendering their cure very tedious; diseases of the skin also affect the results considerably at that period of life, while between 30 and 50 delirium tremens tends to diminish the general average by the rapidity with which it runs its course either to death or recovery.

The average duration of fevers of the continued type among these men has been 16 days, while that of intermittents has been $17\frac{2}{10}$. The great duration of diseases of the integumentary system arises chiefly from the cases of itch and ulcers of the legs, the former averaging $34\frac{2}{10}$ days, and the latter 31; while other ulcers are 21, and boils and abscesses $19\frac{9}{10}$ days. To those accustomed to treat itch in England this statement seems almost incredible, but it has long been known to prevail among the Danes with remarkable virulence.

The average annual "*sick time*" of each man employed in the Copen-

hagen dockyard was $9\frac{9}{10}$ days, but to those under 50 years of age only $8\frac{7}{10}$. Among the workmen in Woolwich dockyard it amounted to $8\frac{6}{10}$, and in Portsmouth to $7\frac{3}{10}$. Among the East India company's labourers under 50 it was $5\frac{2}{10}$, but this arose from the much smaller proportion of cases coming under treatment among them annually. We may however conclude that the sick time to each individual is rather higher among the Copenhagen workmen than among the English.

By multiplying the duration of sickness by the ratio of attacks and dividing the product by 365, we ascertain the proportion constantly sick in 1000 men to be, among the Copenhagen workmen at all ages $27\frac{1}{10}$; under 50, 24; in Portsmouth dockyard $19\frac{8}{10}$; among the East India company's labourers, at all ages $16\frac{5}{10}$, and under 50 years $14\frac{1}{10}$; while among the cavalry in the United Kingdom it amounts to about 40.

The influence of the seasons in producing disease at Copenhagen is shown in the following table.

	Admitted into Hospital.	Treated in Quarters.	Total.	Of 1000 cases there occurred in each month	
				Among Copenhagen Workmen.*	Among the Cavalry in the United Kingdom.*
January	483	21	504	63·8	79·1
February	669	16	685	95·9	81·8
March	710	11	721	91·2	75·2
April	767	7	774	101·2	78·7
May	720	10	730	92·4	87·9
June	720	11	731	95·5	88·
July	770	11	781	98·8	89·
August	776	10	786	99·4	95·5
September	587	5	592	77·4	88·6
October	560	11	571	72·3	81·7
November	558	12	570	74·5	75·2
December	288	9	297	37·6	79·3
Total	7608	134	7742	1000·	1000·

The very large proportion of admissions in April was caused by the prevalence of influenza in a severe epidemic form in 1833. If we regard this increase as accidental, June, July, and August will be found the most unhealthy both among the dockyard workmen and the troops, the latter, however, continue to suffer throughout September also. The very small number of admissions among the workmen in December and January arises of course in a great measure from the arrangement by which during one third of the former and two thirds of the latter month they do not attend the dockyards. By referring to the meteorological table given in Dr. Fenger's work, we find that the period of greatest sickness corresponds with that at which the temperature is highest, and the greatest quantity of rain falls.

The months in which the deaths took place are not stated, but of the 331 cases which ultimately proved fatal there began

In Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
59	29	18	27	25	22	20	35	24	28	20	24

* In both calculations allowance has been made for the different length of the months.

The most striking feature in this statement is the large proportion of fatal cases which began in January, a month when the men are less under medical supervision than at any other period, and would lead to the inference of the great importance of medical treatment in the *early stages* of disease. The influence of the seasons on the intensity of disease is very different from its influence on the prevalence of disease, for there ultimately died of the cases occurring in the quarter Dec.—Feb. $7\frac{1}{2}$ per cent.; March—May, $3\frac{1}{10}$; June—August, $3\frac{3}{10}$; and Sept.—Nov. $4\frac{1}{10}$; consequently in the two quarters when the fewest cases occurred the largest proportion proved fatal.

It now only remains to enquire how far each class of diseases has been influenced by season. As the number of cases, however, is too small to ensure regularity by the monthly results, it becomes necessary to combine them into quarterly ones, making the requisite allowance, as formerly, for the different length of each quarter. In the following table, prepared according to this form, Dec. Jan. and Feb. have been included as winter, and June, July, August, as summer; so that the three coldest months might be brought together in the one instance, and the three warmest in the other.

	Fevers.	Diseases of the chest.	Diseases of the abdomen.	Diseases of the genital organs.	Diseases of the Integumentary system.	Diseases of the fibrous system.	Wounds and injuries.	Other classes of diseases.	Total.
Dec. Jan. Feb.	14·2	20·4	8·8	4·9	20·3	12·2	18·8	13·2	112·8
March, April, May	44·8	22·1	8·	3·9	28·8	12·3	22·9	12·4	155·2
June, July, August	42·2	16·	19·5	4·3	21·1	12·6	29·8	15·2	160·7
Sept. Oct. Nov.	27·1	13·	10·3	7·3	19·	9·6	24·3	12·8	123·1
Mean	32·1	17·9	11·6	5·1	22·3	11·7	23·9	13·4	138·

It must be borne in mind that the cases recorded in the winter must, as already mentioned, have been fewer that what actually occurred, owing to a large proportion of the workmen not being under the usual strict medical superintendence. Could the necessary correction have been made for this contingency it is probable that diseases of the chest would be found most prevalent during that quarter. In the spring of 1833 influenza prevailed, which materially increased the ratio attacked, but in both the spring and winter seasons the cases are considerably above the average. The opposite holds true with respect to abdominal disease, which, as in this country, prevails to the greatest extent during the hot weather. Rheumatism has been less prevalent during autumn than at any other season, but the heat of summer does not seem more favorable to it than the changeable and damp weather of spring. The proportion of wounds and injuries is of course highest at those seasons when the working hours are longest. Fevers have prevailed most in the spring quarter, but this has depended on the epidemics of intermittent before alluded to, for the average number of cases of common continued fever has been

In winter 9·1, Spring 14·7. Summer 26·9. Autumn 14.

A result quite in accordance with observations made in this country.

The influence of the seasons upon disease, however, is materially affected by the age of the individuals, as will be seen by the following statement of the average number of cases occurring in each quarter at three different periods of life.

	15-30	30-50	50 and upwards.
December, January, February .	37·7	32·5	42·3
March, April, May . .	62·7	50·4	41·9
June, July, August . . .	62·7	59·6	38·3
September, October, November .	49·6	41·8	31·7

Thus among the young men the greatest amount of sickness occurs in spring and summer; among the middle-aged in summer; while in the decline of life they suffer most from the severity of winter and the variable weather of spring.

In concluding our remarks on this interesting subject, we cannot but express regret that detailed abstracts of the diseases have not been published, because however valuable the conclusions arrived at by statistical investigation may be, they lose much of that value when the precise data on which they have been founded are withheld. We are moreover prevented thereby from entering upon a most interesting branch of investigation, the influence of climate upon particular diseases, phthisis for example. On the whole, however, the work before us reflects much credit on the talents and industry of the author, and sets an example worthy of imitation in this country, and which we trust will soon be followed by the medical officers in charge of the government dockyards, or of other establishments employing large bodies of workmen.

Art. X.

Guy's Hospital Reports. No. XIII. Oct. 1841.—*London,* 8vo.

The Number before us fully sustains the high character which these valuable reports have maintained from their commencement. We present our readers with the following abstract of its contents.

i. The first paper is part of a communication by Dr. G. H. Barlow, *On certain Diseases originating in early Youth.* We shall defer any notice of it until its completion.

ii. *Medico-legal Report of a Case of Poisoning with Arsenic, by* Mr. Taylor. This is another valuable contribution to forensic medicine, from a gentleman to whom the profession is already much indebted. We shall endeavour to give as complete an abstract as we are able of its leading features, earnestly recommending our readers to peruse the whole.

The case occurred near Wallingford, in Berkshire. The unfortunate individual was a married man, aged 28, who lived on bad terms with his wife, on account of an improper intimacy which existed between her and another man. He was seen by Mr. Breach, a surgeon, on January 27, 1841, apparently, then, labouring under common diarrhœa, which was very prevalent in the neighbourhood, and with no evident symptoms of

danger. Compound chalk powder with opium was prescribed. He died next day; and suspicions being excited, a coroner's inquest was held, and the body examined. The dissection was made fifty-nine hours after death. The body presented no unusual external appearances. The omentum was found highly congested and inflamed. The small intestines were of a vermilion-red colour throughout (internally ?), but diminishing in intensity from the duodenum downwards. There was no appearance of ulceration in the intestinal canal, nor was any part of the peritoneum inflamed, (we presume the *attached* portions of the peritoneum are meant, for it is stated a few lines above, that the omentum was highly inflamed.) There was no effusion of lymph, and no adhesions could be detected. The stomach was removed, and shown to Mr. Taylor five days after the examination, having been kept, with its contents, in a sealed bottle during the interval. The whole of the mucous membrane was found to be of a dull red colour, the dulness being particularly well marked in the interspace between the cardia and pylorus. In the lesser curvature, between these openings, there were three rather elevated lines, consisting of firmly adherent mucus, intermixed with a white substance, having a streaky character, like cream, poured on a dark surface. These lines had a border of deep redness, which became gradually shaded off into the surrounding mucous membrane. On removing the mucus with a spatula, the stomach beneath presented a deep cinnabar-red colour. There were several small patches of ulceration about this part of the viscus; and near the cardiac orifice were two ulcers, almost circular, and each about the size of a sixpence. When first examined they were covered by a white powder, and surrounded by dark effused blood. The rugæ were numerous and highly inflamed at the margins. When first seen, a white pasty matter was diffused in patches over the whole surface. The coats of the organ were not altered in thickness or consistence.

We must pass over the details of the analysis, which was conducted with great care and minuteness. No arsenic whatever was discovered in the fluid contents, but it was most unequivocally detected in the white matter and mucus adhering to the coats of the stomach, by all the tests upon which reliance can be placed. But though we have not room for a full account of the various processes, there are two points connected with them, which cannot with propriety be omitted, because they are of much importance in themselves, and are not yet generally known to the profession.

The first is a simple method, lately recommended by Mr. Marsh, for distinguishing the arsenical from the antimonial stain in his apparatus:

"Place a few drops of ammonio-nitrate of silver on a plate of glass, or in a small saucer, and hold it over the flame, at about half an inch from the point. If arsenic be present, yellow arsenite of silver will be formed; if antimony, according to Mr. Marsh, a white precipitate results: in the latter case, however, I have found the liquid blackened, from the reduction of the silver. I have since tried this with seleniuretted hydrogen, and found the liquid blackened: when the hydrogen is free from any foreign impregnation, the silver is also reduced. Hence, in the effect on the ammonio-nitrate of silver thus applied, we have a strong proof of the presence or absence of arsenic." (p. 271.)

With reference to the employment of Marsh's apparatus, it must never be forgotten that many specimens of sulph. acid, as sold in the shops,

are contaminated with arsenic and selenium, (the latter of which gives a similar stain to the former,) and that frequent trials of the apparatus, *before introducing the suspected liquid*, are therefore absolutely essential.

The second point to which we would advert, is a modification of the copper test, which is not commonly noticed by medico-legal writers, but which, in Mr. Taylor's opinion, may be safely resorted to by practitioners.

"Having thrown down so much of the green precipitate as we possibly can, by adding the copper test to the suspected liquid, we wash, collect, and dry it. A small quantity of it, finely powdered, is then to be introduced into a minute tube, and very gently heated over the flame of a spirit-lamp. When this experiment is carefully performed a ring of small, octohedral crystals will appear, if the salt be arsenite of copper. These become sublimed and fixed on the glass tube, a little above the powder. These crystals of arsenic are plainly perceptible to the eye, from their highly refracting properties; and their triangular surfaces are easily distinguished by a lens of low power. If the heat be rapidly applied, there is produced only a dense, white crust of arsenic, in an uncrystalline state. The application of the copper test is, it appears to me, useless without this corroborative experiment. This is by far the best criterion of the nature of the precipitate; and is much more satisfactory than the plan commonly recommended, of distinguishing the arsenite of copper from other salts of that metal, by its solubility or insolubility in certain menstrua. To the medical jurist the result is interesting; since it is a most satisfactory method of reproducing the poison in the state in which it was actually taken. Arsenite of copper may be reduced by charcoal, (not by black flux;) but the result is never so satisfactory as the production of the crystals of arsenious acid, according to the method above mentioned." (p. 272.)

Proceeding now to the consideration of this case, as one involving a criminal accusation, we meet with several points of much importance, and no small interest to a medical man. The wife of the deceased and her paramour were both committed on a charge of murder, but as there was no direct evidence against the latter, he was discharged by the grand jury, and the female prisoner took her trial alone. Mr. Taylor and Mr. Breach were summoned on the part of the crown, to give evidence of the death by poisoning. It is of great importance that any one who is liable to be cited as a medical witness upon an occasion like the present, should be well prepared for the kind of questions he is likely to be asked, and we shall therefore shortly mention those that were put by counsel on this trial. Mr. Breach was asked, whether the diarrhœa might not have caused the man's death, but we need scarcely say that his opinion was conclusive on this point. He was then asked, whether arsenic might not have been accidentally mixed up with the powders sent by him to the deceased, and whether he kept arsenic among his medicines. The answers were, That he prepared the powders himself—that he had prescribed similar ones on the same day for other patients, without bad results—and that the only arsenical preparation he kept was a solution. He was next required to describe the method of analysis employed—to account for the safe custody of the stomach and its contents, until the analysis was made, and also of the tubes and plates containing the reduced metal. He was then asked the probable quantity of reduced metal in each tube, and whether it would be possible to convert all the poison, diffused through the stomach and intestines, to the state of metal.

The questions put to Mr. Taylor were, chiefly, Whether ulceration of the mucous membrane of the stomach was a common effect of arsenical

poisoning—what was the earliest period at which it might take place—the earliest period for the occurrence of severe inflammation of the mucous membrane—the quantity of arsenic required to kill an adult—how much was found in deceased's stomach, and whether this was sufficient to destroy life? Again: How many tests were employed—which was the most satisfactory—and why, if the test of reduction were satisfactory, were others afterwards employed? "This last," says Mr. Taylor, "was an ingenious question; as, by the application of so many tests, it would seem as if we doubted the correctness of the inference drawn from those first employed. I answered it by saying that the various tests were employed, not merely to corroborate each other, but for the acquisition of experience, on their general efficacy and relative value."

In reference to the question of the earliest period at which severe inflammation of the mucous membrane could occur, Mr. Taylor observes:

"This is a question of some importance, although it does not seem to have attracted the notice of medico-legal writers. The earliest period at which I have heard of inflammation of the stomach being found, was about six or eight hours after the poison had been taken. A very important addition to our knowledge on this subject has been lately made by Mr. Foster, of Huntingdon. He has communicated three cases of poisoning by arsenic, to a medical periodical. A woman took arsenic, and administered the same poison to two of her children, the one aged two years and half, and the other an infant of five months. The mother died in three hours and half, the elder child in less than two hours, and the infant in six hours and half after having taken the poison. The quantity of arsenic swallowed could not be ascertained. The mucous membrane of the stomach, in each case, was found highly inflamed, but less so in the case of the mother than in the children. There was no ulceration. By these cases two new facts are added to our knowledge of arsenical poisoning: 1, that this poison may destroy life within two hours; and, 2, that in that short period acute inflammation of the stomach may be perfectly established." (p. 278.)

Mr. Taylor's analysis of the evidence adduced upon the trial is extremely interesting, and will amply repay the trouble of a careful perusal; we can give but a very short sketch. It was proved that the prisoner, three days before the death of her husband, had secretly applied to her sister to procure for her some arsenic, for the alleged purpose of destroying mice, and that two packages, containing about ʒj. of arsenic, were delivered into her hands about twelve o'clock of the day on which he died. These packages were not forthcoming after his death, and the prisoner affirmed that she had directed a woman, who was sweeping up the house, to throw them into the fire. It was also proved, that on the morning of the decease, before the arrival of the poison, the prisoner told one of the witnesses who went to the house, that her husband was "something better," that about midday he became violently ill, and that he died about five minutes after eleven, p. m. having, by her own admission, suffered severely from pain and vomiting. (It should be observed that all the vomited matters had been thrown away.) No medical assistance was applied for during the time of his severe illness, and when Mr. Breach told her that an examination was requisite, she became agitated, and said "What do the people expect to find? I am sure he came by a natural death. However, *I* gave him nothing!"

We believe there are few who could read even this slight abstract, and still fewer who could peruse the detailed account given by Mr. Taylor,

without feeling at once convinced that the wife was guilty of the charge laid against her. The unfortunate man was seen by no one, except the prisoner, until he was evidently labouring under the effects of the poison, and there was not the remotest shadow of proof that he had either taken it himself or had it accidentally administered to him: nevertheless, the judge stopped the proceedings, and directed the jury to discharge the prisoner, on the grounds, "that this case was one of suspicion only—that there was no proof of any motive, on the part of the prisoner, for committing the act—nor was there any *direct* evidence of her having administered the poison to the deceased." The alleged absence of motive appears to us a very singular reason; we should have thought the adulterous intercourse, which was known to have existed between the prisoner and another man, was quite sufficient. There was probably no *direct* evidence of her having given the poison; but in how few cases can this be proved? Very little precaution is required to conceal the act so far, and in this instance it is perfectly clear that, either the man committed suicide or she was the murderer. Upon the whole, we cannot avoid the conclusion, that the ends of justice would have been better served, by allowing the trial to take its fair course, instead of smothering it in such an off-hand manner as was done on this occasion.

III. *A Case of Abdominal Effusion, resulting from Mesenteric Tumour, by* Dr. H. Marshall Hughes; *with Observations on the Effused Fluid, by* Dr. G. O. Rees. The patient, a young man aged twenty, by occupation a wire-weaver, had been particularly robust and free from ailment until about a year before he came under Dr. Hughes's care. In childhood he had never had any indication of mesenteric affection, or other abdominal disease. When first seen, (Dec. 31, 1840,) he appeared to be suffering from functional disorder of the stomach, which was stated to have been his chief ailment for some months previous, and he was treated accordingly, up to the 26th of January; when, upon being visited at home, he was found labouring under the same symptoms as before, with the addition of considerable tension of the abdomen, arising chiefly from flatulence. No tumour was detected, but there was some indistinct fluctuation. From this date he became progressively worse; the distension of the abdomen increasing with great rapidity, and the body becoming most singularly emaciated. No means of treatment were of any avail, and he died on the 17th of February, "being," says Dr. Hughes, "more, and especially more quickly, emaciated than any individual I had before seen." The body was examined twenty-two hours after death.

"Not a particle of fat was to be seen, either in the integuments or omentum. The peritoneal cavity contained from seven to eight quarts of rather thick and perfectly milky fluid, aptly compared, from its appearance, to almond emulsion. The peritoneum was not vascular, excepting over a depending portion of the ileum, but was universally sprinkled with minute white specks; by far the larger portion of which was easily removed by gentle friction, and consisted of delicate and almost capillary shreds, evidently deposited from the milky fluid, but some of which were as clearly firmly adherent to, if not produced by, the membrane itself. They were translucent, angular, and elongated, rather than rounded; and, in external appearance, bore a much more striking resemblance to the ova of *pediculi capitis* than to any form of tubercles In the centre of the

abdomen, resting on the spine, was a rounded nodulated tumour, as large as a twopenny loaf, which consisted of several agglomerated mesenteric glands; some of which were as large as a small orange, and, when divided, presented a soft, pinkish, pultaceous mass; from which, upon very slight pressure, exuded a white, cream-like fluid, which appeared to constitute a part of the deposit itself; others were of a dull, white colour, drier, and more granular, the whole exhibiting, both exteriorly and upon the exposed section, the general characters of cerebriform cancer. Other glands of the mesentery were more or less enlarged and opaque, some equalling the size of marbles and of pigeon's eggs. Some of the inguinal glands were also considerably enlarged, but contained no heterologue [gous] deposit. Several convolutions of the intestines, and the transverse arch of the colon were adherent to the tumour. They all, however, appeared healthy except the colon, which in two places was contracted and puckered round two white spots as large as a shilling, which were white, firm, and semi-cartilaginous in appearance. Opposite to these spots, the mucous membrane was entirely wanting; and their cut surface presented the same physical characters as the early stage of scirrhous pylorus. One tubercular-looking body *only*, about the size of a pea, was discovered in the mesentery, close to a fold of the ileum. The liver, spleen, and kidneys were healthy. The pancreas was, unfortunately, not examined. Numerous lacteals—large tortuous, varicose, and distended, some with a milky and others with a clear fluid—were observed in almost all parts of the mesentery; but, in consequence of the sections already made, and of the examination occurring in a private house, no attempt at injection, for the purpose of discovering any lesion, could be made with any probable chance of success." (p. 300.)

Some of the fluid was sent to Dr. Rees for examination, who found it to contain *chyle* in considerable quantity; how much he could not determine. When agitated with æther the fluid separated into three parts, of which the upper was a solution of fatty matter, the lower serum, and the middle a floating mass of chylous matter.

This is assuredly a very singular case, but Dr. Hughes is mistaken in supposing it to be unique—the effusion of milky or chylous fluid into the cavity of the abdomen has been noticed by several pathologists, and ascribed to rupture of a lymphatic vessel. (Vide Copland's Dict., art. Dropsy, p. 605.)

IV. *On Acute Aortitis, by* Dr. Norman Chevers. This is a valuable paper, comprising, as the author informs us, some of the facts elicited in the careful examination of nearly 400 aortæ, from patients in Guy's Hospital, in a great proportion of whose cases he had an opportunity of watching the symptoms during life.

Dr. Chevers is of opinion that the arterial tissues are rarely if ever subjected to chronic inflammation, precisely analogous to what occurs in other organs, and he therefore limits the forms under which the inflammatory state developes itself in these textures, to those which are usually distinguished by the terms acute and sub-acute. Our readers will judge for themselves how far the facts related bear out this doctrine.

Acute inflammation of the thoracic aorta appears to occur, in nearly all cases, either as a direct result or as a complication of some other morbid process existing in the system. In the early states very few of the implicated structures become the seats of morbid depositions; but as the diseased action advances, certain effusions take place, the most yielding tissues suffering first. Thus, when the aorta has been

long subjected to irregular distension and irritation, in consequence of hypertrophy of the left ventricle, the outer cellular portion of its sheath is often found œdematous and spongy.

We doubt, however, whether this can be regarded as a mark of inflammatory action; it is probably the result of obstructed circulation through the coats of the vessel. Dr. Chivers has also noticed the existence of numerous spots of red or purple ecchymosis in this coat, varying from one to four lines in diameter, occupying the cells immediately in contact with the denser portion of the external coat, and remaining attached to it after the loose filamentary tissue of its sheath has been torn away. Numerous vessels can always be traced to the circumference of these spots, and they generally appear to have been so recently deposited, as to lead to the surmise that they are formed either shortly before death or during the last agony. These two forms of lesion have usually been observed in cases where the death-struggle has been protracted, as in hydrophobia, or accompanied with much suffocative action, as in cases of obstruction of the air-passages. They are also frequent in extensive thoracic inflammations, especially asthenic pleuritis. Dr. Chevers has never discovered such appearances in the aorta below the diaphragm, nor in any of the smaller arterial trunks. It does not appear that the contractile coat of the aorta undergoes any change in the early stages of inflammation beyond that of softening, rendering it liable to transverse rupture at its upper part. Dr. Chevers believes such an event is more generally attributable to acute than to chronic change. Of the correctness of this opinion we entertain very great doubts; we believe that, in the majority of instances in which this accident has occurred, the coats of the vessel have exhibited decided marks of slow and continued diseased action.

The lining membrane of the aorta and the structures immediately subsidiary to it, present two distinct forms of alteration when acutely inflamed. We shall shortly notice both.

First Type. This appears to result from a very active form of acute, adhesive inflammation; a single layer of coagulated lymph, of variable thickness, being effused over a greater or less surface of the serous membrane. This kind of effusion is, however, rarely seen in its original condition, unless death ensues very rapidly, for it is soon covered by masses of coagulum from the blood. It is occasionally noticed in over-worked horses which have died from thoracic inflammation, and Dr. Chivers has observed it in one case in the human subject. A similar condition exists in the smaller vessels in dry gangrene; the previously diseased walls becoming acutely affected and, as a consequence, covered with plastic lymph, upon which clots are deposited and organized. There is this difference between the two cases, that, while the whole circumference of the smaller vessels is occupied by the effusion, it is generally confined to one side of the caliber of the aorta, and yet the vessel may be materially obstructed, or even entirely closed, by the large size of the organizable clots found upon it. There is a preparation in the museum of Guy's Hospital which well illustrates this point: the aorta is rendered completely impervious, from the origin of the inferior mesentric artery downwards, by a firm mass of pale fibrin. The aortic valves bore traces of acute inflammation, and the lower part of the vessels had undergone

extensive ossific changes. It does not appear that a scabrous state of the walls of an artery renders it liable to the formation of these clots, unless an acute attack supervene; for ossified vessels often remain unobstructed for years.

This form of disease may occur in one of two states of the circulation, or from a combination of both. It may arise while the flow of blood through the vessel is feeble or unnaturally slow, from diminished power of the heart. Thus, in some cases of exhaustion during the latter stages of adynamic disease, inflammation of a low type is set up in the aorta, and lymph being effused from its lining, the current of blood has not strength to prevent the formation of a false membrane, nor to wash it off when produced. Or it may occur from some actual impediment producing a degree of stasis in the current. In certain stages of Bright's disease there is a great tendency to these inflammatory effusions, and the obstructed circulation through the kidneys, liver, and spleen, combined with the languor of the heart's action, gives rise to the formation of the blood-deposits.

Second Type. This bears a close resemblance to the worst forms of erysipelas, in the rapidity of its course, the character of its products, and its selection of the most debilitated and cachectic individuals as the subjects of its ravages. In a man who died a few days after the operation of lithotomy, the aorta presented the following appearances:

"The outer coat was traversed by a multitude of enlarged vessels, many of which would have admitted a bristle, inosculating with each other by minute branches. A few small, light-red ecchymoses adhered to the denser portion of the outer coat. The lining membrane was in some places of a dark cornelian hue; in others it presented a paler shade of red; and in one spot, extending near the origins of the intercostal arteries, this had deepened to a blackish violet tint. The redness was chiefly owing to the presence of what appeared to be a minute network of injected vessels, disposed nearly equally over the whole sub-serous tissue; the partial deepening of colour probably, in some degree, depended upon cadaveric change. The internal coat was neither thickened nor unusually lacerable: its redness extended slightly in some places to the middle tunic, which presented little change beyond an appearance of unusual yellowness." (p. 314.)

This form of aortitis only appears in the most enfeebled constitutions, either during the latter stages of ataxic fevers, or in the subjects of chronic visceral disease, when suffering under the effects of severe operations, profuse hemorrhage, phlebitis, or arteritis. The effused matter is not sufficiently plastic to resist the current of the circulation, and therefore neither lymph nor coagula form upon the interior of the vessel, and the morbid action continues to spread onwards, until nearly the whole of the great arteries are involved in the fatal mischief. It appears to be identical with the erysipelatous arteritis which was observed by Cline and Abernethy after operations for aneurism, and which also occasionally results from slight injuries of the extremities, or even idiopathically from purely constitutional causes.

Pathologists are not agreed as to the nature of the red stains so frequently presented by arterial linings, namely, whether they are results of cadaveric changes, or evidences of diseased action during life. Dr. Chevers has made some interesting experiments for the elucidation of this point, and also for the purpose of ascertaining in what degree various

conditions of the internal membrane before death influence the production of stains afterwards.

"I placed," he says, "four portions of aortæ, from different subjects, in blood recently drawn, (having broken up the clot and added the serum;) and allowed them to macerate in it for twenty-four hours, the usual average time at which bodies are inspected after death. [The experiment was performed during the winter.] When removed from the blood, 1, a portion of pale and recent aorta was found to have undergone no change. 2. In another portion, which, when removed from the body, was slightly stained in numerous patches and stripes, these parts were found to be losing their red tinge, and assuming a blackish or chocolate-brown colour, the intervals remaining clear. 3. A piece of aorta, in which portions of the lining had been red, but were becoming black from putrefaction, was little changed. 4. This was also the case in another vessel, where the inner membrane presented coloured spots, probably of inflammatory origin.

"The arteries were then again allowed to remain immersed for twenty-four hours, and at the end of that period it was found that, 1. All the tissues of the healthy artery were stained of a dull flesh-colour. 2. The surface of the stained vessel offered a generally darker hue, but the brownish stains were less defined. 3. The decomposing vessel was nearly equally throughout of a dirty-gray colour. 4. The inflamed aorta had become little changed, except that its reddened spots were darker, and that the remainder of its interior had lost its polish.

"From this experiment I think we may deduce the following inferences: That, during cold and temperate weather, stains from imbibition will not be produced in tolerably healthy arteries until after the usual time at which post-mortem examinations are made. That stains, very different from those produced during life, result from the imbibition of blood similar to that usually found in the aorta. Again: That a colour produced by inflammatory action becomes somewhat changed after death, by the long application of fluid blood." (pp. 316-17.)

When inflammation has continued long in the aorta, the lining membrane is generally raised in many places by solid masses of lymph in the sub-serous cellular tissue: these generally peel off with the membrane, and, until cut into, appear like red vesicles rising behind it. Dr. Chevers is of opinion, that so long as these masses retain their transparency and florid colour, the disease may be looked upon as recent; for shortly after their deposition a slight cloud of white opacity begins to appear in the centre of each, and gradually advances until the whole becomes of an almost cartilaginous toughness, and then slowly undergoes the atheromatous or ossific change. In these opinions he is directly opposed to M. Bizot, who contends that the masses of lymph are deposited upon the free surface of the membrane, and never undergo the ossific alteration. We believe that Dr. Chevers's views are by far the most sound, but our space will not admit of a full development of the argument.

It is very common to find the lining membrane of the aorta partially reddened, in cases where inflammation cannot be regarded as the cause, and where the stains, from their position and other circumstances, manifestly do *not* result from contact with blood, or gravitation of the fluid contents of the *vasa vasorum*. This appearance is explained in the following manner by Dr. Chevers:

"I endeavoured to account for this kind of reddening by assuming that, during life, the inner surface of the aorta is of a pink colour, resembling that of mucous membranes. Now, it may be observed, that, in the moment of

death, the aorta, contracting, loses its cylindrical form; and, becoming flattened, its opposite surfaces are placed for a time completely in apposition. During a perfect contraction of this kind, the capillaries supplying the lining membrane must become emptied of their contents, which pass into the outer vasa vasis, leaving to the inner part of the tube its usual blanched appearance. But should any circumstance, occurring during or even very shortly after the last agony, as a failure in the action of the organic nerves at any point, prevent the cylinder from exerting or maintaining its tonicity at that spot, the internal surface would there retain its original florid aspect, while the remainder became entirely deprived of colour." (p. 319.)

Ingenious as this explanation assuredly is, we very much doubt if it will stand the test of examination. Is it true that the aorta becomes flattened at the moment of death? We feel convinced that such is not the case. The tonicity of the middle coat of arteries undoubtedly gives rise to contraction, and the cavity of a vessel may even be obliterated in this manner; but what results? Why, that we have a round cord instead of a cylinder; not a flat riband, as Dr. Chevers imagines. Again: supposing the tonicity of any part to be destroyed, (and the *possibility* of such an occurrence might readily be disputed,) would not the stains occupy the whole circumference of the artery, rather than occur in "marginated streaks and patches?" And, lastly, where have we the shadow of a proof of the pink colour of the inner surface of the aorta? But, indeed, a process of argumentation, which is avowedly based upon a pure assumption, is scarcely worth the trouble of demolishing. We are sorry to meet with anything of the kind in so admirable a paper.

In reference to the *causes of aortitis*, Dr. Chevers advances little that is new. The disease is evidently most liable to occur in debilitated and cachectic constitutions, from whatever circumstance such a state may have originated. He is inclined to think, that the attacks are frequently produced either by imperfectly decarbonized blood, as in cases where there is much pulmonary affection, or by a too highly azotized condition of that fluid, as in Bright's disease.

The same may be said of his observations upon the *symptoms* and *treatment;* they differ in no important respect from the accounts given by other writers. We should notice, however, that Dr. Chevers is decidedly opposed to the vigorous antiphlogistic treatment recommended and adopted by some continental practitioners; and considering the extreme rarity of a sthenic form of aortitis, we think his remarks are judicious.

v. *Cases of Malignant Disease of the Lungs, by* Dr. Hughes. This is a paper also which deserves perusal; not on account of anything novel in the statements, but simply because it contains the record of four interesting cases and dissections. The first two of these are evidently instances of cerebriform disease of the lung; the nature of the others, and particularly of the last, is much more obscure; indeed we feel very great doubts as to the propriety of including *it* in the class of malignant affections. A mere abstract of a paper like this would be of little value; and as we have not room for its entire transcription, we must content ourselves with a very few observations.

According to Dr. Stokes, there are two forms of this complaint: in the first, the organ itself is transformed into a cancerous mass; in the

second, a tumour is developed external to the lung, which it ultimately displaces. Dr. Hughes would add to these a third and, as he believes, much more common form, viz. the dispersion of a number of rounded masses, varying in size, colour, and consistence, through the greater part of one or, more frequently, of both lungs, while disease of a similar nature exists at the same time in other parts of the body. Only one, however, of the cases narrated in the communication before us is an example of this variety.

In reference to *diagnosis*, we are unfortunately but little assisted by the researches of Dr. Hughes. The number of cases that have been accurately observed is far too small to admit of any positive deductions. Dr. Hughes thinks that an appearance of the sputa resembling "red currant jelly and water," the existence of perfect dulness on percussion in some part of the chest, obstruction of the superficial veins of the affected side, shown either by enlargement of the vessels themselves, or by œdema, are the chief signs to be relied on. Of course the coexistence of malignant tumours in some other parts of the body will materially aid the formation of a correct opinion, and much may also be gained from the history of each case.

VI. *Report of Cases requiring capital Operations, by* Bransby B. Cooper, Esq. There is little to detain us in this paper. Five cases of operation for aneurism are reported. In these we shall only notice two points.

1. In tying the femoral artery, instead of drawing the saphenous nerve outward, and then passing the aneurismal needle between the artery and vein, Mr. Cooper adopted the following plan:

"Immediately on opening the sheath, he passed an aneurismal needle, unarmed, below the whole of its contents, from without to within, and to such a distance, that its curved extremity appeared close to and on a level with the upper and inner edge of the artery. There were two branches in this case, running along the upper surface of the artery. Having gently detached these, by means of the blunt end of a small probe, Mr. Cooper 'tilted' them over the exposed end of the aneurismal needle; on which, therefore, remained lying only the artery and vein. Between these Mr. Cooper now insinuated the end of the probe; and through the space thus made, passed the needle, having removed it cautiously from its former situation. In this situation the needle was armed by an assistant, and the ligature then secured in the usual way. This method appeared to place the separation of the artery from the nerves and vein more under the command of the operator, while it caused the least possible disturbance to its cellular attachments." (p. 354.)

With all due deference to Mr. Cooper, we must beg leave to differ essentially from him in our estimate of the value of this modification. In the first place, it appears sufficiently plain that it most unnecessarily detaches the vein from its sheath, which is avoided by the old method. Secondly, we cannot understand how the nerve is more likely to escape injury, seeing that the needle is withdrawn from its position after the nerve has been "tilted over" its extremity, and then requires to be inserted afresh. And, thirdly, we should conceive that, when the parts are deep-seated, it would be no easy matter to arm the needle after it has been passed round the vessel.

2. In applying a ligature to the common carotid, Mr. Cooper advises

the first incision to be made nearer the mesial line than usual, in order that the sheath may be opened as much on the inner side as possible, by which means the difficulties often experienced from the overlapping of the jugular vein are avoided. If this method be adopted it will be found impossible to introduce the needle from without inwards, but it can be easily effected in the opposite direction. During the performance of the operation related, a circumstance of some physiological interest occurred. The patient made suddenly a peculiar barking noise, and he afterwards informed Mr. Cooper that it was quite involuntary, and appeared to be caused by something in the wound being pushed aside by the operator's fingers, and that it "was attended with a curious sensation, as of some one squeezing the lower part of his throat, and also a sudden weight at the stomach, and a disturbance in the bowels; so that he was, for a minute or two, afraid that he should not be able to retain his fæces." This was evidently caused by pressure upon the *par vagum*.

Two successful cases of *excision of the elbow-joint* are next related, but present no remarkable features. Mr. Cooper appears to regard anchylosis as the termination to be aimed at; but assuredly, under proper management, a much more favorable event may be reasonably expected. Many cases are on record in which the affected limb has regained powers of motion scarcely inferior to those of the healthy extremity.

The case of *steatomatous tumour* in the gluteal region, which concludes the paper, requires no particular notice.

VII. *On the Structure of the Blood-Corpuscle, by* Dr. G. O. Rees and Mr. S. Lane. According to the observations of these gentlemen, the corpuscle is a flattened cyst adherent at its centre, or axis, to a nucleus, and having a fluid surrounding that nucleus at all parts but those by which it adheres to the envelope. This structure is demonstrated in the following manner: a drop of blood, placed between glass and mica, being examined, and the ordinary appearances recognized, a drop of concentrated syrup was added, and the mixture submitted to the microscope. "The corpuscles had quite lost their original form, being collapsed and, in some instances, doubled on themselves; some of them had the appearance of dried raisins or caraway seeds; others of thin, flattened shreds of membrane, which, as they moved through the surrounding medium, bent from side to side with a waving motion, like that of an empty balloon of tissue-paper floating in the air." Some pure water was then added to the syrup, and the specimen again examined. "The whole of the corpuscles were seen changed: some had returned to their natural form, and were filling and becoming rounded, and thickened on their edges; others had already acquired a marked obicular character, and were so distended, that they appeared to bound, on coming in contact with other distended corpuscles, instead of gliding past them, as we observe in their natural condition." These phenomena were evidently the result of exosmosis and endosmosis, and are not a little interesting. If they shall be confirmed by other observers, they will serve to explain many points now involved in much obscurity.

VIII. Mr. Wilkinson King has described a *Patella*, which appears to have been broken transversely, and is reunited by bone. The ap-

pearances are illustrated by a plate. He makes at the same time a number of desultory and not always very intelligible remarks upon the nature and treatment of such injuries, for which we must content ourselves with a simple reference to the paper.

IX. *A Case of Intestino-vesical Fistula, by* Mr. HINGESTON. We have only room for an account of the dissection of the parts, of which a diagram and drawings are given :

"The colon—hypertrophied, singularly muscular, and in circumference about the size of a man's arm—was, together with a convolution of the ilium, and the appendix cœci, adherent to the fundus of the bladder. The natural course of its canal was impeded by a contraction or stricture, which commenced inferiorly in the rectum, about the forefinger's length from the anus, (just at the base of the triangle formed by the vesiculæ seminales,) and extended upwards for about two inches, barely admitting the entrance of the little finger. A section of the gut in this part resembled scirrhus; and the glands were, with the surrounding tissues, thickened. Immediately above this stricture the coats of the bowel were riddled with ulcerations and openings, leading into a channel which separated the bladder from the intestine. This channel was in fact a feculent abscess, situated beneath the reflected portion of the peritoneum, between the bladder and bowel. It was degenerate in structure, lined with a dark membrane, and filled with a muco-purulent excretion. It opened anteriorly, into the fundus of the bladder; above, into the colon; below, into the rectum; and posteriorly, through the colon into the ileum: so that there was a false passage, by which the natural course of the colon was diverted, and forced between the bladder and strictured part of the intestine down into the rectum below, and at the same place, by means of a fistulous opening, into the bladder in front. The orifice of this fistulous opening within the bladder was curtained by a fungous growth or thickening, which overhung it like a valve. Thus, exactly at this point of the feculent abscess, there was this strange deformation: the colon, the rectum, and the ileum, each, conjointly with the fecal abscess, possessed one common entrance into the bladder itself." (p. 406.)

It is an ungrateful task to find fault; but we feel it would be a dereliction of our duty did we neglect to stamp with our most decided disapprobation the vicious style in which this paper is composed. To give an instance: how can such expressions as the following be defended, either upon the score of taste or propriety? "So *silent* and insignificant was the starting *point* of the disease." "The false outlet through the bladder was lessened or *diverted*." "A more alarming collapse, with a rapid pulse, clammy perspiration, obstinate hiccup, and all the other ominious and foreboding signs which usually *gather about the bed* in these perilous hours." "The *fistulous opening* into the bladder became finally *silent*." We trust that Mr. Hingeston will henceforth avoid such injudicious misapplications of language; we are surprised they should be allowed by the editors to pass.

X. The last paper in this number is a communication on *Chorea*, from the pen of Dr. B. G. BABINGTON. It is well and cleverly written, but contains nothing very novel. The author eulogizes Dr. Marshall Hall's explanation of the pathology of this disease, "that it is a morbid condition of the organ of emotion, which has its seat in the medulla oblongata, and is wholly independent either of the brain or the ganglionic system;" while he, at the same time, somewhat naively acknowledges "that it

must be long before facts are accumulated, sufficiently numerous and decisive to establish or controvert this ingenious supposition." We do not think the cases recorded will avail much towards the establishment of the principle in question. It is clear that the only facts upon which reliance can be placed must be such as prove the existence of disease in the medulla oblongata, the supposed locality of this "organ of emotion;" but the post-mortem examinations before us present nothing of the kind, the nearest approach being the discovery, in one case, of old adhesions between the surfaces of the spinal arachnoid, "especially over the posterior part of the medulla." Disease of various parts of the brain and of the membranes of the spinal cord was indeed found in other cases, but these are manifestly inconclusive. Still the paper is interesting, and will well repay the trouble of perusing. We have room for little more than Dr. Babington's *definition* of chorea, which is somewhat novel:

"I should define chorea to be a disease characterized by irregular, uncontrollable contractions of the voluntary muscles, alternating with their atony, and occurring without pain.............. I have used the word 'contractions,' and have included the atony of the muscles in this definition, because the movements in this disease appear to me to differ essentially from those of convulsions and epilepsy in this, that the stimulus, whatever be its nature, which excites either the whole or a portion of the voluntary muscles to involuntary action, is not more violent in degree than the normal stimulus of the will, or of the excito-motory system; so that movements almost incessant, indeed, but not exaggerated like spasms are the result............ The nerves, in their normal state, are always exercising a certain amount of influence over the muscles; so that where there is antagonism of forces it is only necessary to remove the one opponent in order to demonstrate that the other is in a state of activity. This being the healthy condition, we have a right to consider the diminution of this activity as a morbid state; for although, from the striking effect which a morbid exaltation of muscular force produces, spasm is more directly brought to the cognizance of our senses than atony, still the latter is no less really a morbid condition than the former. I venture, then, to express my belief, that while in true convulsions the muscles, after having been thrown into a spasmodic state, do only return to the normal condition; in chorea, on the contrary, a further diminution of nervous influence occurs, so that the muscles become, in all marked cases, entirely passive and inert in the intervals between their irregular and involuntary actions. This is manifest, from the manner in which the limbs drop from the position into which they have been thus thrown; in which the head, after being tossed to and fro, will fall passively on the shoulders; and from the incapability on the part of the patient to hold anything in his grasp." (p. 113.)

In regard to *treatment*, we do not find anything particularly new, excepting the notice of a mode of practice which has been lately adopted at St. Petersburg, with eminent success, in obstinate cases. The patient is placed in a bath as hot as he can bear it; kept there for half an hour; and, when thus thrown into the most profuse perspiration, is suddenly plunged into cold water. Among the innumerable tonic remedies which have been employed in this disease, Dr. Babington gives the preference to sulphate of zinc and the liquor arsenicalis. He also speaks highly of the shower-bath.

Art. XI.

Essais sur la Méthode Sous-cutanée, etc., précédés d'une Introduction Historique sur l'Origine et la Constitution de cette Méthode. Par le Dr. Jules Guerin.—*Paris*, 1841. 8vo, pp. 126.

Essays on the Subcutaneous Method of Operating, preceded by an Historical Introduction on its Origin and Nature. By Dr. J. Guerin.—*Paris*, 1841.

This is a pamphlet designed to claim for the author the exclusive merit of demonstrating that subcutaneous incisions, made with exclusion of air, do not inflame or suppurate, but immediately cicatrize; and to establish thereon a general principle applicable to a variety of chirurgical operations.

The author sets out by the admission that subcutaneous section of the tendo Achillis was the first impulse towards the formation of the system, and professes to trace by whom and in what manner this operation, "a mere *empirical fact*" attained the practical development it had received when he commenced his investigations.

"The first operators had divided at one stroke the tendon and skin by a transverse incision. The first step towards the subcutaneous method and one which resulted from blind instinct, was the division of the sterno-mastoid upon a director by means of a longitudinal incision in the integuments parallel to and above the muscular fibres; the intention having been to diminish the extent of the inflammation by lessening the size of the wound and to avoid an ugly cicatrix. [We cannot conceive by what justice a modification of an operation founded on such cogent reasons can be attributed to blind instinct.—*Rev.*] Delpech advanced another step towards the method: he proposed, and was the first to execute the subcutaneous section of the tendo Achillis..........by dividing it in situ beneath the integuments through an incision parallel to the tendon, but open on each side. Delpech sought to prevent exfoliation of the tendon by not exposing it.—[In this he was not successful, as suppuration and exfoliation of the tendon retarded the cure many months.—*Rev.*] Subsequently, in 1822, Dupuytren divided one portion of the sterno-mastoid beneath the integuments, preferring this method, (the patient being a young girl,) in order to avoid disfigurement. He made a smaller incision than Delpech did in the case of the tendo Achillis. This celebrated surgeon therefore effected a novel and practical improvement in the operation, but does not appear to have suspected the physiological import of the result he obtained. Stromeyer adopted in 1833 section of the tendo Achillis after the idea of Delpech: [This statement is incorrect as regards the date of Stromeyer's first operation, which took place in 1831, see Stromeyer's Beiträge, p. 60.—*Rev.*] he diminished the size of the openings in the integuments so as to fulfil the indications described by Delpech, namely, to avoid suppuration and exfoliation of the tendon, in which he succeeded. Since this last fortunate modification of Delpech's proceeding, several surgeons, among whom I reckon myself, have endeavoured still further to perfect it by suppressing one of the cutaneous punctures. Such has been the development of the *practical* fact. Its essential physiological signification has hitherto remained what it was prior to the curative attempt of the surgeon of Montpellier." (p. 11.)

Before entering on the consideration of the *principle* contended for by M. Guerin, we must remark that we have always considered the great importance attached to the suppression of one of the punctures by the followers of Stromeyer an injustice to that surgeon. The large number of successful cases reported by him show that it is perfectly immaterial as respects cicatrization of the skin and reunion of the tendon whether one or two punctures be made. It should also be borne in mind that by

his method of operating a second puncture does not *necessarily* result; he mentions its occurrence as an incidental circumstance only. He endeavours to sever the tendon by pressing the edge of the knife against it, and if the whole is not divided when its point reaches the opposite part of integument he prefers permitting the point to traverse the skin, to disturbing the cellular tissue by sawing or hacking movements of the knife. Unquestionably one puncture is preferable to two, being less painful; and as the section can be effected with equal facility with the point of the blade as with the edge, the operation with a single puncture ought to supersede the other. *The subcutaneous section of a tendon*, whether effected by one or two punctures, remains nevertheless the operation of Stromeyer.

After giving his summary of the successive improvements in the operation of division of tendons, Guerin endeavours to prove that Stromeyer and all succeeding tenotomists have, until his own researches, been entirely ignorant of the reasons of the innocuity of subcutaneous sections. What must Stromeyer, Dieffenbach, and the host of enlightened surgeons who have practised the Stromeyerian operation think of the puerile attempt to deny them the credit of perceiving that the safety of the operation depended on the exclusion of air and absence of inflammation in the severed parts? The immediate closing of the puncture and reunion of the tendon contrasted so strongly with the apprehensions formerly entertained respecting injury of these tissues, that every educated surgeon perceived at once without the two years' reflection it cost M. Guerin to arrive at the same conclusion, (p. 20,) that exclusion of the air and immediate union of the cutaneous section constituted the importance of the Stromeyerian method. M. Guerin imputes (p. 13) to Stromeyer ignorance of "the absence of inflammation and of the occurrence of immediate *organization* so characteristic of subcutaneous tenotomy:" he quotes nevertheless, in corroboration of his assertion, the following passage from Stromeyer's first paper in Rust's Magazin and the Archives de Médecine:* "The object I had in view, to make the external wounds as small as possible, in order to prevent entrance of air, suppuration and exfoliation† of the tendon was so completely effected, that the point of the knife alone penetrated the opposite integument, without occasioning a bleeding wound; the puncture at which the knife entered was not larger than the width of the blade." Stromeyer in this sentence distinctly enounces the concatenation of events to be guarded against, entry of air, suppuration and exfoliation of the tendon. He omits mention of inflammation, one link in the chain of phenomena; but does Stromeyer or any other well-informed surgeon suppose suppuration to ensue without previous inflammation, or that suppuration is not a sign of pre-existing inflammation? Certainly not; and consequently it must be believed that the omission of the word inflammation was accidental. The advantage taken by M. Guerin of this omission (p. 13) is unworthy a candid searcher after truth. Stromeyer does not in any of his works, as Guerin mistates (p. 14), mention that "the reaction was weak, insignificant," implying an acknowledgment of the occurrence of

* Republished in Stromeyer's Beiträge, p. 60.

† It may be here remarked that Guerin's translation of this sentence differs from the above purely literal one, by his having placed the word suppuration after exfoliation which alters the import.—REV.

inflammation in his cases, although in a lessened degree. The only passages on this subject in relation to the subcutaneous section of the tendo Achillis, we can find in Stromeyer's works, are in his Beiträge, pp. 71 and 75. "The small wounds healed *by the first intention.* The *adhesion* of the ends of the tendon was so far advanced," &c. &c. "On the fifth day the ends had adhered"—which prove his opinion to have been, that on subcutaneous section of tendons, reunion occurs not by inflammation and suppuration, but by first intention, by adhesion, or by immediate organization, which term M. Guerin adopts to express this process of union.

M. Guerin, as if in anticipation of his claims being disallowed, is very indignant against the awarders of spurious honours:

"When a discovery is effected, the invention may almost always be attributed to somebody else who does not doubt his share in it. The pretended inventor has only to lend himself to these accidental interpretations, and by means of a few vague expressions, detached words, and ambiguous phrases, he may impose the belief that everything had been observed and described before it was done by him, who really did observe and describe everything.......... We repeat for the last time, neither Stromeyer, nor any of those who have adopted his method, have stated that subcutaneous wounds of tendons do not inflame; all have followed Delpech, and preceding writers, and have spoken only of the absence of *suppuration* and exfoliation of tendons, implicitly and explicitly admitting the existence of a certain degree of inflammation. It is therefore evident that in their opinion the essential character of subcutaneous tenotomy is not the production of a distinct order of phenomena, the immediate organization of the divided parts, but the diminution of the inflammatory phenomena of ordinary wounds, that is to say, a feeble reaction, a slight, usually imperceptible suppuration, seldom an abscess: the whole arising from the nature of the tissues divided, and the smallness of the wounds of the skin." (p. 17.)

If the brief quotation we have made from Stromeyer do not enforce conviction that Stromeyer recognized the principle on which the security of his method was founded, we may refer to the opinion of our countryman, Dr. Little, confessedly a close follower in many respects of Stromeyer. He says in his Inaugural Dissertation,* (p. 56,) the first treatise published on the subject in any language, "Thus, usually on the second or third day the small wounds are agglutinated without pain or inflammation." And in his Treatise on Club-foot, &c., reviewed in this Journal, October, 1839, he thus enters more fully into the matter:

"We may explain the reason of the safety of dividing so large a tendon as that of the tendo Achillis, or of puncturing the fascia of the sole of the foot, and dividing the tendon of the flexor longus pollicis, by the facts, that no inflammation follows the infliction of so small a wound, that the skin immediately agglutinates, and the severed tendon is placed in the comparatively safe condition of a ruptured tendon, between which and an exposed wound of tendon there is the same difference as exists between the simple and compound fracture of a bone, with reference to the probabilities of suppuration or sloughing." (Little's Treatise, p. 29.)

M. Guerin, in another part of the memoir (p. 28), avails himself of the same illustration as that above afforded by Dr. Little.

The truly valuable part of M. Guerin's memoir is the generalization of the principle furnished by the mode of healing of subcutaneous section of tendons. M. Guerin lays it down as an axiom "*that all subcutaneous*

* Symbolæ ad Talipedem verum cognoscendum, etc.—*Berolini,* (January,) 1837.

sections, in whatever situation and whatever be the nature of the structures divided, participate in the property of subcutaneous sections of tendons; that is to say, they do not influence or suppurate, but reunite immediately;" and the correctness of this principle is shown by subcutaneous operations on animals, involving in some cases the division of the whole of the muscles, nerves, and vessels, on the posterior aspect of the extremities, and in others by subcutaneous punctures of the articulations, by subcutaneous section of various ligaments in the human subject, and by the subcutaneous puncture of abscesses, subcutaneous section of the tunica-vaginalis for the cure of hydrocele, &c. As further extension of the subcutaneous method of operation proposed by himself or by other surgeons, he mentions:

1. The evacuation of sanguineous tumours formed by subcutaneous effusion of blood and of serous cysts, the occurrence of which he has witnessed after subcutaneous myotomy; restoration being effected by adhesion of the parietes of the cysts without suppuration.
2. The subcutaneous incision of incipient phlegmonous swellings with the view of producing disgorgement.
3. The evacuation of meliceris tumours.
4. The removal of small exostoses.
5. The division of the sphincter ani, for the cure of fissure of the rectum.
6. The subcutaneous section of the stricture of the femoral or inguinal hernia, and adhesion of the neck of the sac.
7. The subcutaneous ligature of veins for the cure of varicocele.
8. The subcutaneous section of the capsular ligament of the knee, for the purpose of exciting effusion of lymph and consequent fixing loose bodies contained in the articulation.
9. The subcutaneous section of nerves, for the cure of neuralgia.

M. Guerin also proposes the evacuation of loose bodies from the knee by a process which has already been often applied by other surgeons with fatal results. It consists in fixing the foreign substance, and forming a large fold of integuments on one side of it; after which the integuments and capsular ligaments require division to an extent sufficient to permit the introduction of forceps to seize the foreign body. The fold of integuments being relaxed, the cutaneous and capsular incisions no longer correspond, and the entry of air is guarded against. We have witnessed the fatal performance of this operation. No necessity existed for the introduction of forceps, as the loose substance slipped out the instant the knife was withdrawn. On the introduction of this operation many years since, it appeared a great improvement; but, in addition to the danger attending it, it is at best very undeserving the title of subcutaneous. Very far different, however, is the elegant, ingenious, and, we conceive, perfectly safe method resorted to by M. Goyrand, of which an account is given in a former Number of this Journal. (vol. XI. p. 526.) Further experience is required to enable us to decide on the general applicability of this operation; but we are much mistaken if M. Goyrand do not earn a lasting reputation in having rendered the removal of foreign bodies from the knee-joint—hitherto considered among the most dangerous operations, and utterly condemned by many of the best surgeons of the present day—one of the simplest, safest, and most effectual.

Art. XII.

A Manual of General Therapeutics, with Rules for Prescribing, and a Copious Collection of Formulæ. By D. Spillan, m.d.—*London,* 1841. 8vo, pp. 458.

We cannot laud the volume before us as one of those performances, masterly at once in the conception and execution, which occasionally surprise and delight us—brightly contrasting with the dull and monotonous mediocrity which precedes and follows them; but with the exception of an occasionally disagreeable poverty of facts, thoughts, and illustrations, we can truly describe Dr. Spillan's work as respectable and useful, though not brilliant or very scientific.

Before proceeding to the examination of the more important and practical portions of the work, we shall briefly refer to one or two statements, which, under the semblance of being philosophical, are, in fact, somewhat crude and shortsighted. At page 2, and elsewhere, Dr. Spillan talks very sneeringly of empiricism. If, on these occasions, he refers (which, however, we do not think he does) to the vulgar empiricism of the regular charlatan, whose treatment is chiefly or wholly that of hazard, and has no foundation in observation and analogy, we agree with his remarks. But he ought not to forget, or to express himself as if he were ignorant, that, in the first instance, the whole art and science of medicine, must have been empirical. When, therefore, the author talks of the "entire process of the treatment," of inflammation, "being decidedly rational," (Advertisement, p. viii.) and asserts that "reason points out the antiphlogistic method of treatment (p. 11) in pneumonia, peritonitis, and all inflammations of organs," he employs the common and non-professional modes of expression of everyday life, not those of science.

We are also surprised at the author's tirades against what he calls the "medicine of symptoms." Can there be, for example, a statement more rashly unqualified than the following? "Though we have taken pains in endeavouring to prove that the most dangerous of all therapeutic methods was the medicine of symptoms, and that all medicinal treatment founded on such miserable indications, tended to the destruction of the patient and the disgrace of the healing art," &c. (p. 20.) In this, as in the former case, we blame Dr. Spillan, for not distinguishing between the symptomatic treatment of the ignorant quack, and "symptomatic medicine," as practised and confided in by the intelligent and educated physician. If his remarks relate to the former, they are stale truisms; if to the latter, they are hasty and unphilosophical dogmas. By what means other than by an observation of symptoms, comprehending, of course, what are called physical signs, can any declension of an organ from its physiological condition and action be indicated or ascertained? Does Dr. Spillan mean to assert that, in any case, the "symptom" is not, if rightly interpreted, or whether rightly interpreted or not, *exactly* expressive of the lesion to which it is due? We maintain it to be so in every case, however anomalous that symptom may seem, or however obscure to *us* may be its connexion with the lesion which causes it.

Let us take, in order to illustrate our meaning, the case referred to by Dr. Spillan himself. A man presents himself with headach from gastric derangement. Were the practitioner to regard the cephalalgia alone,

(and that he should regard this and this alone, Dr. Spillan seems to assume as inevitable,) and to apply leeches, perhaps, to the temples without further measures, such blundering practice would prove, *not* that symptomatic medicine was fallacious, but that the practitioner ill understood the science of symptoms, and grossly neglected to enquire after, and to avail himself of, several signs, *besides* the cephalalgia, which must infallibly have been present in the case referred to. Had he pressed his hand, for example, on the man's epigastrium, it is most likely that tenderness would have been complained of; had he enquired if his headach did not come on at a particular period after eating, was not aggravated by particular sorts or quantities of food; had he examined the patient's tongue, &c. his diagnosis, instead of being erroneous, would have been exact. Yet, be it observed, that the practitioner, all the while, would have been dealing with nothing but symptoms; and his error, if he had fallen into error, would have been owing, not to the absence of "symptoms," or to any fallacy in these, but to his ignorance in confining his attention to one symptom only, and to his neglect of others or his misinterpretation of them. We have only to add that Dr. Spillan's own statement (p. 4) seems directly contradictory of his disparaging opinions elsewhere, in regard to the utility and importance of symptoms: "Thus, then," he observes, "it is not alone the lesions found on the dead body, but those also revealed by the symptoms........which the therapeutist should study, as it is these he is called to treat."

Chapter third opens with the following remark: "All pathologists are now agreed that diagnosis is the most certain of all curative indications," &c. Now diagnosis is not, properly speaking, an indication at all, but implies that observation and comparison of symptoms, on which indication is founded. At p. 34 we find Dr. Spillan remarking: "The changes produced by disease on the appearance of the eyes have rarely any other therapeutic value, save that resulting from their diagnostic importance!" Of a like character, is the remark at page 22: "The yellow colour of the tongue observable in jaundice can have no therapeutic value in itself; the indications are necessarily derived from the diagnosis." True; but is Dr. Spillan prepared to maintain that the appearance of the tongue is absolutely of *no* therapeutic consequence, in contributing *its* share along with other signs, in enabling us to form that *diagnosis?* Surely we have here a very incautious over-statement.

This chapter, which treats of the "Therapeutic Indications derived from Disease," exhibits a view of the indications suggested by the morbid phenomena of organic and animal life, the excretions, and the intellectual faculties. Interspersed through it there are some singular observations. Thus, for example, we are told (p. 22), "The brown or black coating of the tongue is sometimes the result of a concentration of strength;" and at page 30 it is stated that "Percussion of the chest, though so useful in diagnosis, is but of secondary utility in treatment!" At p. 32 we are surprised to find Dr. Spillan stating that "the therapeutic indications derived from the urine are but few and not very important." We are informed, in the same page, that "the yellow and saffron-coloured urine, which generally denotes diseases of the liver, is useful only as a diagnostic sign," as if there were any other way in which it would be so. While hæmaturia is referred to and designated as "but a symptom," no other

morbid state or appearance of the urine is referred to, and the presence of albumen, in this excretion, is not even hinted at. At p. 33 we are told that "a restless and inconstant attitude of body .... can only be useful by aiding in the diagnosis;" what other end it could be expected to serve, we must profess ourselves profoundly ignorant.

We may quote the following observations on the causes of diseases as therapeutical indications:

"But little," observes the author, "is known of their mode of action, and there are but few of them really indispensable to the production of diseases; most of them may give rise indifferently to all affections. ....Let us suppose fifty individuals exposed to the influence of one and the same morbific cause, as, for instance, to the influence of cold; we shall find them all attacked by different diseases. Some will have pneumonia, some pleuritis, some erysipelas, and others rheumatism, which circumstances can only be accounted for by supposing that there exists in each a peculiar predisposition previous to the application of the exciting cause; this predisposition does not constitute a morbid state, as previously to the exposure, they were all in perfect health, and probably without such exposure they would have remained so. Thus these two circumstances are necessary, the one to the other, for the production of the disease; still the predisposition is, without doubt, far the more important of the two." (p. 43.)

We pass over the following interesting subjects, which are treated of in succession, and with varying degrees of copiousness and ability: the causes, nature, seat, course, duration, and periods of diseases, considered as therapeutic indications; the circumstances of professions, habits, temperament, and idiosyncrasy and habits, which modify curative indications. We then come to chapter five, the subject of which is the modus operandi of medicines, and here there are some interesting matters presented to us.

Dr. Spillan points out that medicinal actions are of two sorts; the one confined to the point of application, the other extending to remote organs. The circumstance of medicines producing merely local effects is due to the medicinal substance possessing no peculiar activity, or to its being applied in a small dose or to an important organ. With these exceptions, the effect of medicinal substances is to influence remote organs along with, or independently of, the parts where they are primarily applied; and the question is by what mechanism these distant phenomena are effected. By some it is supposed that the nerves, by a mutual *consensus*, either sympathetic or antagonistic, transfer the local impression from one part to another. Another proposed solution of the phenomena is that of mere proximity; as when substances applied to the abdominal parietes, affect the stomach or bladder. The objections to this theory of medicinal actions are stated to be, first, that the circumstance of several substances as iodine, arsenic, mercury being detectible in the blood, is opposed to the idea that these act only at the point of application, since, circulating in the blood, they must come into contact with all parts of the body. Secondly, that after both external and internal exhibition, several medicinal substances appear in the excretions; another proof that, dissolved in the blood, they must have previously perambulated the body, and consequently have operated at many other points than the one of primary application. These facts suffice to prove that when a medicine acts "dynamically," its remote effects cannot be accounted for by nervous sympathy or "nervous conduction." (p. 76.) We should rather say that

these facts simply prove that there are *other* modes of explaining remote effects than that of "nervous conduction." According to the existing views in physiology there is but one other way of accounting for the phenomena of remote actions, and this is by supposing that the medicinal substances themselves reach distant organs; and how can this happen unless by these substances being absorbed into the circulation, and thus distributed? And that this is the real method of diffusion is proved by the following facts: 1. By the medicinal substances being found in the blood. 2. By their being found in the secretions and excretions. 3. By their being found deposited in the solid parts of the body. 4. By the effects on remote organs taking place at a perceptibly later period than the local effects. (Dr. Spillan assigns this as one proof; but we do not see that the same argument will not equally apply to the theory of "nervous conduction.") 5. By the circumstance that while neither structural nor functional changes occur at the point of application of the medicinal substance, considerable effects take place in remote parts or organs. Dr. Spillan gives several other facts proving that it is by diffusion in the blood that medicinal substances produce their remote effects. We shall refer to only one or two of these. When a substance is introduced into a part, the circulation of which is allowed to continue, but its nervous connexions are cut off, the systemic phenomena are not the less rapidly and completely manifested; while, in the contrary case, namely that of the blood-vessels being tied, and the nerves left untouched, no systemic effects follow the introduction or application of a medicinal substance. Lastly; medicinal substances introduced directly into the blood, act in just the same way as when taken internally, or exhibited in any other way.

The following are the circumstances which distinguish medicinal actions produced through the circulation and those produced by nervous conduction: 1. When the remote action of a medicinal substance is a sympathetic one, it corresponds, in strength, to a greater or less extent, with the local action. 2. The remote action, in the case of nervous conduction, is so much the more intense, in proportion as the organ which is locally affected by the medicine is important. We omit the third proof referred to by Dr. Spillan, namely, the negative one obtained from section of the nerves, between the point at which the medicinal substance is applied and that at which the remote action takes place; and we give the fourth, and last, in the author's own words: "The remote effects occasioned by a consensus of the nerves, and which are solely the consequence of the local medicinal action, follow always immediately after the local action, cannot take place without this, are not prevented by an artificial plethora," &c. (p. 89.)

At pp. 95 et seq., the question is discussed, "How the blood and medicinal substances react on each other when they meet;" and under this head some interesting facts and views are brought forward. The following is a summary of Dr. Spillan's conclusions: Whenever a medicinal substance, howsoever administered, is found in the secretions, excretions, or solids, retaining all its original properties, we must conclude that it has been merely mixed with the blood. But though such substances suffer no change from the blood, the converse of the proposition does not follow that the blood suffers no change from the presence of the

medicinal substance. Thus neutral salts, though undergoing no change themselves, prevent or retard the coagulability of the blood, according to Dr. Stevens's experiments, to which property these salts are supposed to owe their antiphlogistic power; "although," justly adds Dr. Spillan, "for producing such effects they owe something to their property of promoting the secretions." (p. 96.)

Bitters also produce considerable changes on the blood, but the effect is not, the author thinks, so much due to their operation on the constituents of the blood as to their improving the action of the digestive organs. Acids, particularly the vegetable, are always found at least in the urine, combined with bases; and from the fact of mercury never being detectible in the blood, unless the blood itself be entirely decomposed, we must infer that the metal combines intimately with some constituent of the blood, probably with the albumen.

The author then proceeds to the consideration of another very interesting question, the terms of which we shall allow him to state in his own words:

"If we suppose a medicinal substance circulating in the blood, the first question which forces itself on us is, how does this act on the parietes of the vessels? Considering that medicinal action can take place only where there are nerves, we must conclude, *a priori*, that in the larger trunks, and even in the smaller vessels, no medicinal actions go on, there being no nerves on the inner membrane of these vessels. Medicinal substances which enter the circulation can, therefore, only act there, where all change of substance and all reciprocal action between the solids and fluids take place, namely in the capillary vascular system. But it is only the central parts and the peripheric terminations of the nervous system that are in the most intimate and multiplied contact with the capillary system. It is here that the medicinal substances, circulating with other constituents of the blood, enter from these vessels, and, as it were, irrigate the islets of the solid parts lying between them; and through this contact with the nervous terminations, and, one might say, with the interior of the central organs of the nervous system, medicinal action is called forth. Numerous experiments prove that the blood-vessels have no capability of reaction on the application of medicinal substances, one of the factors necessary for medicinal action being wanting, we mean the nerve. The experiments of Fontana, Wedemeyer, Christison, and Macartney establish the same fact." (pp. 98-9.)

Dr. Spillan concludes that medicinal substances produce their actions "through contact with the centres of the nervous system, and its terminations alone." (p. 99.) But he proposes the question whether "all medicines which act on the periphery of the organism, when they come into contact with the nervous endings, will do the same thing when they meet with them, when circulating in the blood;" and thinks, "there are facts which show that medicinal substances taken into the blood excite medicinal actions, in contact with the nervous terminations." (p. 99.) Thus on Dupuytren injecting milk into the veins of a dog, he saw the same movements produced as if it had been applied to his tongue. A strong-scented fluid, injected in like manner, caused the animal to dilate his nostrils and "snuff" around him, in quest of the source of the odour. Hale, on introducing castor oil into a vein of his arm, had shortly after the taste of it in his mouth.

That, in other cases, certain substances act chiefly or solely on the nervous centres, is shown by the fact that opium, introduced into the

carotid, kills much sooner than when injected into any other vessel; and this peculiarity Orfila states as characterizing all narcotics.

Still, one of the most singular, at once, and most important properties of several medicinal substances remains unexplained; that, namely, of their operating on particular organs only. The following facts are considered by Dr. Spillan to prove "that particular organs possess a specific susceptibility for certain medicinal substances." 1. Particular substances occasion an increased afflux of blood to the organs on which they chiefly exert their influence, and even excite in them structural changes. 2. Medicines which act on particular organs are occasionally deposited in them. 3. Medicines which promote certain secretions are excreted with these. This last phenomenon, observes Dr. Spillan, "can only be accounted for by the admission, that the nerves of certain organs stand in a peculiar relation to certain medicinal substances." (p. 105.) The truth of this proposition is, according to the author, proved by Höhler's experiments in respect to diuretic medicines, as the vegetable acids, the diuretic salts, squills, garlic, oil of turpentine, &c. The most important conclusions deduced by the author from these facts are:

"The secretions already formed in the blood are more especially attracted by certain organs. Among the secretions which possess peculiar constituents may be reckoned the urine. In the healthy state, urea and uric acid are found in it. Now with respect to the urea, it has been shown by Prevost and Dumas that it exists already formed in the blood, and that it is not in the kidneys merely that it is formed. With respect to this substance, it must now be admitted that it is eliminated by the kidneys, solely for this reason, that between both of them, there exists a species of elective attraction." (pp. 107-8.)

The particular impressibility of certain organs by certain medicinal substances, Dr. Spillan thus endeavours to account for:

"Light and colour are the natural stimuli of the organ of vision, and no other sense is affected by them; sound stimulates the ear and not the eye; sugar makes no impression on the organ of smell, and he who possesses not this latter sense may bathe in rose oil, without experiencing the least odour. If, then, we can understand that a particle of an odour does not affect the eye, and that the organ of smell is insensible to light, we may easily conceive how it happens that a medicinal substance circulating in the blood, though it come in contact with all the organs, affects only some particular organ." (pp. 108-9.)

Chapter vi. is devoted to a consideration of the surfaces adapted for the application of medicinal substances, and to the particulars which naturally grow out of this subject. It is stated (p. 111), that the surfaces proper for the reception of medicinal substances are ten in number, namely, 1, the stomach and intestines; 2, the large intestines alone; 3, the skin; 4, the surface of the eyes; 5, the pituitary membrane; 6, the interior of the mouth; 7, the surface of the air-tubes; 8, the meatus auditorius; 9, the interior of the urethra and bladder; 10, in the female, the vagina and sometimes the cavity of the uterus. These surfaces might have been more succinctly described as comprising or comprised in the mucous and dermoid membranes. The author thinks that the part which the "system of innervation" performs in the operation of medicine has never been sufficiently appreciated.

"When a medicinal substance traverses the interior of the primæ viæ, it is less on the organic fibres which constitute the tissues of the stomach and intes-

tines that it acts, than on the nervous expansions with which it comes in contact. 1. In the stomach, the divisions of the pneumo-gastric nerve transmit its action to the medulla oblongata: this action may produce some change in the state of the latter, and impart to it a new power and an unusual force of innervation. 2. On the intestinal surface the medicine will come in contact with nerves communicating with the spinal cord. The modifications produced in these nerves will soon become common to the spinal cord, which will take on another mode of action, and give rise to perceptible changes over all the parts of the animal system. 3. Every medicine on coming into the stomach touches the irradiations of the solar plexus, which will of course undergo some modification, and accordingly its influence will assume an altered character. The impression so made on the solar plexus extends to all the plexuses of the ganglionic nerves. The entire system of innervation, in consequence of the impressions of a medicine, thus acquires an accidental or new activity, which is diffused over the entire animal economy. Thus we see the importance which should be attached to the operation of medicinal substances on the nervous plexuses, the spinal marrow, and the medulla oblongata. In conclusion, it may be well to remark, that for the application of certain medicines, there is a vast difference between a surface which corresponds with the medulla oblongata and a surface which has its communications confined to the spinal cord; thus tartar emetic in the dose of one or two grains usually produces vomiting, when given by the mouth: whilst, if given in the form of lavement, in the dose of six, eight, or ten grains, it no longer vomits; or if it do, it is not till six or more hours after the introduction of the substance by the anus, and the effect is then produced by absorption. The reason is this: in the stomach it comes in contact with the divisions of the pneumo-gastric nerve, which transmit the impression to the medulla oblongata; such connexion does not exist for the intestinal surface." (pp. 112-3.)

In chapters vii. and viii. the "effects of medicinal substances" and the "therapeutic action of medicines" are respectively treated of. Both chapters contain some unnecessary refinements and obscurities, and some trite truths. In chapter vii. the effects of medicines are distinguished into primary and secondary; and, so far as we can perceive, the secondary or curative effects of this chapter are identical with the therapeutic ones of the succeeding. At p. 136 the author remarks: "the immediate or primary effects of a medicine are sure to take place whenever such medicine is employed; if any difference should occur, it will be in degree not in kind. Thus an excitant will always stimulate tissues, purgatives will always irritate the intestinal surface. The curative effects are not thus constant and uniform." But, in fact, drugs have in one sense no other than primary effects, whether these take place in the stomach or only after reaching the nervous centres, by the medium of the circulation; for the secondary or curative effect is, in truth, just the physiological one. In other words, it is not the medicinal substance that accomplishes a cure directly; but a cure follows in consequence of certain physiological actions which the medicinal substance has been instrumental in exciting or modifying in the living organs. There is no mystery in this, so far at least as regards the mere relation in which the drug and the vital organ stand to each other.

ART. XIII.

Zur Würdigung des Theophrastus von Hohenheim. Von Dr. KARL FRIED. HEIN. MARX, Hofrath und Professor an der Georg-August-Universität.—*Göttingen*, 1842. 4to, pp. 140.

On the Real Merits of Theophrastus von Hohenheim, [*Paracelsus.*] By Dr. MARX, Counsellor and Professor in the George-Augustus University of Göttingen.—*Göttingen*, 1842.

PROFESSOR MARX has been long recognized in Germany as one of the most erudite men in that country of scholars. And yet, although his works are numerous, he is scarcely known in England except as the author of a treatise on Poisons, and another on Contagion.* We believe, however, that our unacquaintance with his singular merits as a profound scholar, a skilful physician, and a most amiable man, has reached its term, and that henceforth no name in German medical literature will be better known by the medical profession in this country than that of Dr. Marx.

The work now before us is one which will raise the reputation of Dr. Marx, as an industrious and enlightened scholar and medical historian, to the very highest rank. It is indeed a stupendous monument of industry and learning, and cannot fail to be appreciated as such by the learned men of every country. This work was first read, in the form of memoirs, before the Royal Society of Göttingen, and is now reprinted from its Transactions. Slender though the volume be, its composition occupied its author a long time; and well it might, when the incredible mass of books necessary to be consulted is considered. Full half the bulk of the volume consists of mere references to these works; and the comparison of the text with the notes sufficiently proves that they were not merely referred to, but actually read and digested. The consequence has been that we have now, for the first time, a thorough sifting of the life and writings of Paracelsus, and an appreciation of his merits, which may not merely be received as correct, but must be almost regarded as *final*, if we may so speak, inasmuch as the results obtained are of a kind that can scarcely be invalidated, much less set aside, by any subsequent researches.

In the present article we shall present our readers with an abridged outline of the more remarkable facts and doctrines detailed in this extraordinary production; and in doing so, as well with the view of economizing space as of ensuring accuracy, we shall avail ourselves often of the author's own words, or of those of the singular subject of his memoir,

* The following works of Dr. Marx are now before us:

1. Diatribe de Structura atque vita venarum.—Carlsruhæ, 1819. 8vo, pp. 104.
2. Göttingen in Med. Phys. u. Histor. Hinsicht geschildert.—Göttingen, 1824. 8vo, pp. 392.
3. Origines Contagii.—Göttingen, 1824. 8vo, pp. 202.
4. De Euthanasia Medica.—Göttingen, 1826. 4to, pp. 16.
5. Die Lehre von den Giften. Band i.—Göttingen, 1827. 8vo, pp. 270.
6. Die Lehre von den Giften. Band ii.—Göttingen, 1829. 8vo, pp. 580.
7. Allgemeine Krankheitslehre.—Göttingen, 1833. 8vo, pp. 273.
8. Grundriss zur Lehre von der Krankheit und Heilung.—Carlsruhe, 1838. 8vo, pp. 447.
9. Zur Lehre von der Lähmung der untern gliedmassen.—Carlsruhe, 1838. 12mo.
10. Herophilus: Ein Beitrag zur Geschichte der Medicin.—Carlsruhe, 1838. 8vo.
11. Zum Andenken an J. F. Blumenbach.—Göttingen, 1840. 4to.

scattered through the text or given in the notes. We shall seldom interrupt the narrative by any comments of our own, but leave the impression on the reader's mind to take the form which the perusal seems calculated to give.

THEOPHRASTUS VON HOHENHEIM, better known under the name of PARACELSUS, shared the lot of many men of genius and great reformers, only to be appreciated long after his death. The cause of this is too obvious to be dwelt on.

"Opinions," says Dr. Marx, "which have been prevalent during centuries, or which have been promulgated with much confidence by the leaders of the opinion of the day, are successively stamped as infallible; and it requires a conviction based on no ordinary grounds to raise doubts against established views, or such as are supported continually by specious argument. It is difficult to believe that which has been hitherto considered as true to be but illusion and fallacy. I myself had indulged in the usual erroneous opinions relating to Theophrastus, and it was not until I had read his writings, studied the history of his times, and had duly weighed the opinions of his contemporaries and others, that I arrived at conclusions quite different from those which have hitherto prevailed. On this subject there is not to be found in history another man who has been so much misunderstood as Theophrastus. His very name (*Bombastus*) has become the by-name of any extraordinary assertion. The most strange and contradictory opinions and doings were ascribed to him; and far from being judged according to the standard of his age, his real deserts are either not acknowledged or wrongly interpreted, and results which he neither called forth nor intended are laid at his door. It is the scope of this work to rectify these opinions, and to represent the man in the full light of his age and in his individual life and activity.

"The name of a man," continues Dr. Marx, "is in itself generally a matter of indifference; but it is otherwise in the present case. Many believe that nothing is needed but the mere name of the man to indicate what he really was—PHILIPPUS AUREOLUS THEOPHRASTUS PARACELSUS BOMBASTUS AB HOHENHEIM; and they argue that his whole quack-like vanity is displayed in it. But it is to be observed that he himself never used these names, neither was he so called by any of the authors who were more or less connected with him, and it was only the envy, hatred, malice, and uncharitableness of his enemies that were gratified in this strange compound of appellations." (pp. 1-2.)

In the subsequent pages Professor Marx traces the origin of these names. Some were appellatives of the noble family to which Theophrastus belonged; others originated in the peculiarities of his time, it being then the custom for men of eminence to indulge in a variety of pseudonymes, of which Calvin, for instance, had seven. As the term *bombastic*, unquestionably derived from Paracelsus, has become universally the epithet of any extravagant style of writing or speaking, it may be interesting to know that BOMBAST was one of the surnames of the Hohenheim family, there having lived in Würtemberg persons of that name up to the middle of the sixteenth century. (p. 5.) Our author also traces to their origin and explains the other names of Theophrastus; but with these details we need not detain our readers.

Dr. Marx begins the examination of the merits of Theophrastus with the following passage:

"It happened to Theophrastus during his lifetime, and still more so after his death, that he was proclaimed either as the first physician and philosopher, the leader of modern medicine, or as a superlative quack, pietist, imbecile, and

medical heretic.* We judge men in general by their actions, and authors according to their writings; but in order to be able to do this with any degree of propriety, we have to be first convinced that the former have been related faithfully and that the latter are genuine. If, therefore, a careful sifting of things be always necessary, how much more is it so if doings the most extraordinary are attributed to the same individual, and opinions the most contradictory assigned to the same author! In order to prove at once the fertility and oddity of his mind, the falsity of his reasoning, and the confusion of his ideas, it seemed sufficient to point at the astonishing variety of his writings and the vast number of volumes collected and published under his name in folio and in quarto. But those works, which were considered as supplying authentic data for the formation of a just opinion of his merits, have caused the greatest misapprehensions and mistakes; and it would be difficult to find such another example in history, where the good name of a man has been buried under the weight of books and opinions falsely attributed to him. In the first place it is to be considered that the old editions of his works which we possess were not published by Theophrastus, and that it is even doubtful whether any work at all of his was published during his life; or if this was the case, the instances must have been rare. Another important question will be, how it happens that in those works which, on both external and internal evidence, we must attribute to him, at the side of the most pregnant and profound ideas we so often find empty and unintelligible dreams and fancies, and along with the most clear and impressive language we read the most unmeaning and incomprehensible stuff? It is only by penetrating into and appreciating the essential circumstances of his life, the mainsprings of his actions, and the spirit of his age, furthering, assisting, or impeding him, that we shall be enabled to resolve these questions in a satisfactory manner." (p. 10.)

Professor Marx begins his interesting Defence by stating that the external circumstances of Paracelsus were mostly of such a harassing nature that he could not have found leisure to give to his writings the necessary polish; and if, moreover, the MSS. (as seems often to have been the case) were badly written, they were afterwards printed with numberless and most damning typographical errors. At first the oddity and extravagant character of these writings made them to be greatly sought after; subsequently and consequently they were manufactured, as it were, on speculation. Such MSS. were, moreover, for the most part, in the possession of amateurs, as the savants and medical men of those times did not value them, or even thought themselves excused for keeping them in the background, *as not existing.* In this Theophrastus shared but the fate of most reformers; his bold and penetrating assertions, his vindication of the vernacular language as proper for the composition of medical and scientific works, made him a stumbling-block for the many who only exist by the remastication of old materials and a sturdy adherence to the traditional and received. Centuries were required to elevate the scientific alluvium to the height of such a Colossus. "However," continues our author, "the more that traditionary principles of medicine and general dogmatism made way for newer and more pregnant views, the higher rose from out the low level of this transition-epoch of the old to the new times the name of the man who, a contem-

* *Dessenius* in his work, Medicinæ veteris et rationalis defensio. Colon. Agr. 1573, 4to, p. 202, says of him: "Magus monstrosus, superstitiosus, impius et in Deum blasphemus, mendacissimus, nefandus impostor, ebriosus erro, monstrum horrendum." Morhof, in his Polyhistor. (lib. i. cap. 15, § 16,) says on the other side: "Mirabile huic homini, uti nomen, ita ingenium fuit, novus quasi literati Orbis Cometa."

porary of great reformers, had himself contributed a large share to the regeneration of knowledge." (p. 11.)

The attempts of foreigners to translate the works of Paracelsus have never had any conspicuous result; they could not grasp the wild rambling of his ideas; the more respectable amongst them soon gave up the attempt, and consequently his works and his fame soon fell altogether into the hands "of madmen or idiots." His countrymen began to collect his works soon after his death. The first collection, published from 1568-1573, by *Gerard Donn*, is very rare. *Huser* published one at Basle in 1589, in 10 vols. 4to. The same editor published another edition in 1603-1605, in 3 vols. fol. Incorrect and mutilated translations into Latin, for instance by Dr. Zacharias Parthenius, Francof., 11 (?) vols. 4to, also appeared. It is quite evident that Theophrastus did not, and could not, write *so much*, because, although it appears that he wrote and dictated a great deal, he says in the introduction to his *Grosse Wundarzneykunst* (Great Surgery), in his usual eccentric style: "If truth lies in length, Christ has said too little. It is truth which we shall write and expound, and if there be doubt or a want of reasons, we shall forbear from writing, taking an example from the prophets and evangelists, who wrote brief, the cause being that they wrote truth."* Paracelsus seems to have been very fond of dictating; and this he did so rapidly that, conjointly with other extraordinary acquirements, it led to the then belief that he was possessed by demons. We read in the *Vita Oporini*, who was his amanuensis: "Ad dictata excipienda excitabat: quæ tam expedite recitabantur, ut dæmonum instinctu ea suggeri Oporinus se putasse sæpe affirmaret."

In the detailed enumeration of the impediments thrown in the way of Paracelsus, our author mentions (p. 14) the Imperial College of Censorship, which waged war against him, as well as against all the Reformers of those times, until he was protected by the Estates (parliament) of Carinthia, to whom he dedicated some of his works. It would be incompatible with the scope of this notice to relate all the impediments which were thrown in his way; but a summary knowledge of them may be conveyed in the popular expression that the whole age *ran a muck* at him. Universities, learned societies, governments, corporations, physicians and surgeons, clergy and laymen, and last not least, even *Savants* by profession—all condemned his opinions, whilst the latter did not scruple to confess that they had not studied his works, and were not inclined to do so! So say the great Conrad Gesner (Epist. ad Crato a Craftheim, p. 1:) "Theoph. Paracelsi errorum meministi: et petis ut mittam ad te Catalogum ejus scriptorum: quem ego certe non habeo: neque curavi ut haberem, et si facile potuissem, cum illum plane indignum cujus inter bonos scriptores mentio fieret, judicarem. Bonos dico, non solum eruditos, sed Christianos et pios saltem civiliter, sicut et Ethnici fuerunt. Theophrastus vero certe impius homo et magus fuit, et cum dæmonibus communicavit." (p. 18.)

In the following pages Dr. Marx sifts the genuine writings of Theophrastus from the spurious, and comes to the conclusion that the following are alone genuine; and he makes the general remark, that only the writ-

* Such and similar passages may be the reason that Theophrastus incurred also the hatred of clerical men, who charged him with heretic opinions.

ings on medicine or natural history are those which may be fairly ascribed to him. The following list will be interesting to the curious in medical history and bibliography:—

I. Seven books, De gradibus et compositionibus receptorum.

II. Little Surgery (*Kleine Chirurgie*), or books on French disease, &c.

III. Seven books on open ailments (*Offenen Schäden.*)

IV. Three books on the French disease (*Frantzosen*): of the impostures in treating it, of the correction of the medicines hitherto used, and of the recovery and treatment of diseases occasioned by it. This work was printed separately at Nürnberg, in 1530, in 4to.

V. Of the Impostures of Physicians. This work he intended to publish in 1520 at Nürnberg, but the Censure prevented it.

VI. Opus Paramirum; published 1562, at Mühlhausen. (Much of its oddity appertaining probably to the editor.)

VII. On the Baths of Pfeffer, (in Switzerland,) printed 1571, at Strasburg, in 8vo.

VIII. Great Surgery. (*Grosse Wundarzney.*) This is Theophrastus's *opus magnum.* He dedicated it to King Ferdinand the First of Germany. The original edition (Augsburg, 1536, fol.) seems to be very rare.

IX. Nine books, De natura rerum.

X. Three books, Exculpation of medical slander; errors and labyrinth of physicians; on the origin of gravel and stone. Written in 1538.

The following works are also, with some probability, ascribed to him: De morbis e tantaro oriundis. Scholia et observationes quædam. Tract on the Plague. Perhaps there is no better proof how far the renown of Theophrastus extended, than the fact of there being in the library of Gotha a translation of several of his works into Arabic.

In the succeeding pages Dr. Marx endeavours to exculpate the oddity of the titles of Paracelsus's works, and the strange, strong—nay, what may be called, vulgar turn of his style. Still, every one acknowledges that he never wrote a word which could be construed as offensive to morals, or even to the conventional customs of society. Amongst the things most objected to him was the great use he made of new-fangled words, but he himself meets this objection by saying, "they quarrel with me, in that I write differently from what their own writings are; the cause, however, lies not in my ignorance, but in theirs. Because I stand alone, and am new, and write German, is no reason why you should scorn my writings! I use strange words on account of the strange nature of my science. And who shall gainsay, that if there start up something new, it should not bear also a new name?" (Kl. Chir. p. 250.)

Amongst the most false and barefaced accusations against Paracelsus is to be ranked the imputation of alchemistic and theosophic fancies, and the holding him up as the representative of prejudice, astrology, spagiric medicine, magic, sorcery, and mysticism, the very things against which he most forcibly set his face.

"The period in which he lived," says Dr. Marx, "part of the sixteenth century—was one in which the light of knowledge emerged but gradually from great obscurity, when the belief in witches and demons was commanded, as it were, by the ordinances of the church, and confirmed by the decisions of courts of justice. If, then, any man, rivetted to such narrow and confined ideas, had really brought this tribute to the frailties of his times, it certainly would not afford any particular ground of inculpation; but Theophrastus, *on the contrary,* opposed these

views with all his might, and endeavoured to trace out the way for ideas more enlightened." (p. 33.)

We are sorry that space does not allow us to enter any further into our author's details, most valuable as they must be to all who take an interest in medical history, and in the progress of the human mind.

In the second part of the memoir, Professor Marx investigates the medical career and the medical doctrines of Theophrastus. How and where he lived up to his thirtieth year, is scarcely known, but it appears that he passed his earlier days in continual travels and rambles. Wherefore he says, "Art does not go after any one; it is we who must follow her. I have somewhere heard that a physician ought to be a rambler, (*Landfahrer*,) and it is just that which I like best. Diseases travel everywhere over the wide world, and the doctor who would know much must travel much. Can skill be learned at the stove-side as well as by wayfaring? I guess not so. Whoever will indagate nature must trample her book with his feet. Book-learning is got by letter and letter, but nature is known by land and land—for each new land a new page. This is the line *codex naturæ*—thus must we turn her leaves." (p. 58.) Settled, however, we find Theophrastus at length at Basle, whence he issued a programme, dated 5th June, 1527, announcing his intention to lecture on medicine, physic, and surgery. He became also the city physician, and remained there two years. Although his lectures were at first highly popular, and attended even by men of established fame, his opponents (he calls them men, whose knowledge consists in wearing white gloves) succeeded in making him gradually unpopular. "It was especially his simple, rational, and conscientious way of prescribing, which became a source of much annoyance to him." (p. 54.) It was, therefore, that very simplicity of medical prescription, which is a happy characteristic of our present medical science, which made him the stumbling-block of his contemporaries, whose fame and income he seemed to encroach upon by the efficacy of his new method. Not untruly, and certainly with a pith and frankness which belong not to our skipping times, he says, "the longer the scribbling the shorter the intellect; the longer the recipe the less of virtue is in it. . . . Don't be astonished that I write such short prescriptions, the reason thereof being, that whatever might be added would d—n the medicine." Such assertions could not but raise against the poor man the whole wrath of every empty-headed quack, whose ill-gotten gains were thus at stake. His cures are stated to have been astonishing. Among them is related the case of a certain patient who had been given up by the faculty. Theophrastus prescribed for him one day, and invited him to dinner the next.

Of his travels much is said, but whether true or false cannot be known. It is stated that he had passed ten years of his life in Arabia and the adjacent countries; and when we consider what extensive (miraculous, as it was then considered,) use he made of opiates, it may be imagined that he became conversant with the heroic qualities of this drug amongst the Orientals. His travels even in Europe must have extended beyond the beaten track, as it is certain that he had visited many French and Italian universities, and even Stockholm and England. He died at Salzburg on the 24th of September, 1541, at the early age of 48.

Theophrastus paid much attention to chemistry; and, it is stated, that

his premature death was occasioned by the continual inhalation of the noxious vapours of his laboratory, in which he was seen continually occupied in multifarious experiments and operations. It was a matter of controversy with his contemporaries, whether he derived his great wisdom and art from God or the Devil. Others said, that he derived much knowledge from the initiation into secret societies. It is known that such really existed at that period, but its members were tied down by an oath not to reveal their secrets. Agrippa says in his work, De Vanitate Scientiarum, "Permulta adhuc de hac arte dicere possem, nisi juratum esset de silentio." (cap. xc.)

As Paracelsus declared in many parts of his works that he had founded a new school of medicine, which he called his monarchy (*meine monarchey*), his opinions on medical men and philosophers are interesting. These are collected and recorded with astonishing industry by Dr. Marx, but we can only find room for one or two specimens. He says of Aristotle, that the very foundation of his doctrine is wrong; that Dioscorides wrote on plants, but never tried their virtues; to call Galen a prince of medicine, is wrong; the Arabs are monsters, Avicenna being a mere compiler; Albertus Magnus philosophized without experience; Arnold of Villanova gets sometimes on the right path, but soon leaves it. Amongst the recent physicians, he praises Marsilius Ficinus as the best amongst the Italians; Johannes de Vigo did not blush to lie. Of his own powers and exertions he entertained the highest opinion. "They say, that my Physica, my Theoretica, my Practica, are strange, new, wonderful, unheard-of: how can I appear otherwise than strange to those who have never walked in the sunshine?"

In the succeeding pages Dr. Marx dwells upon and explains yet farther some of the strange imputations cast from all sides and quarters on Theophrastus; some charged him with sensuality, others denied him virility; some said he derived his knowledge from demons, others called him an idiot. The particulars of these charges the lovers of medical history may refer to in the work itself: we can only find room for Dr. Marx's closing remarks, which give a very different estimate of his merits and character:

"In appreciating the character of Theophrastus we must ever recollect the period in which he lived, the persons he came in contact with, his unsteady life, his extensive travels, his early death. Assuredly with his departure his monarchy, as he called it (*seine monarchey*), fell to pieces. No one arose through him or after him to emulate his power and spirit, or to raise higher the edifice which he had founded and sketched, and for which he had collected the materials; for it would be idle to speak, in this respect, of those his hangers-on and repeaters, who were only capable of babbling empty words and formulæ, or catching the mere chaff of his writings. Consequently, he appeared as a meteor, glancing for a moment on the horizon of his age, and then vanishing without a trace. And it was left for later generations to recognize in him no idly-burning *ignis fatuus*, but a star fraught with living light and heat and the pregnant germ of a legitimate and progressive development." (p. 86.)

The third part of Dr. Marx's work is devoted to the investigation of the especial merits of Theophrastus as a medical man. The following is a brief summary of Theophrastus's exertions:

"His characteristics were,—a depth of thought, a capacity of appreciating the essential, a power of discriminating the real relations of human affairs. How

much soever he had endeavoured to study nature by way of observation and experiment, it cannot escape us, that with him the individual has but a subordinate value, and that his object is rather to attain to, and to establish ideas, principles, general views, and, as it were, to transmute the sensual into the spiritual. He wishes that the physician should divine the mysteries of disease by reflection and comparison; that he should place every well-established fact in its appropriate position in the great circle of phenomena, and assign to it (the fact) by adequate and impartial judgment, a fixed relative value in the vast stock of knowledge. Not seldom does he address himself, with pithy adage, to our sympathizing, humane feelings, and calls upon the medical man most forcibly to consider his vocation to be that of a careful and soothing helper (*Helfer*). He does not afford instruction by which we may attain, step by step, easily and regularly, to the knowledge of the needful and useful, and become familiar with the systematic commonplaces of the profession; we often miss in him the regular teacher; but on the other hand, we find ourselves carried to heights, where astonishment seizes us at the views obtained; we become transported into regions where luminous flashes of thought and surprising analogies make us forget the want of merely positive information." (p. 88.)

For the sake of showing the merits of Theophrastus the more clearly, our author devotes a few pages to the state of German medicine at that period. The physicians of that country were divided into sects, Scotists, Thomists, Albertists, &c. and "cavilled at words." Up to the year 1520, scarcely any classical medical books were printed. If there had been any demand for them they would have been printed, because they were well known, but it was only books containing instructions on the preservation of health, and collections of recipes, which paid, and they could not be printed fast enough. The instruction in the universities was scanty. Anatomy was taught after Mundinus, and pigs were dissected for demonstration. The best school of surgery was at Strasburg; and amongst the men there educated, Hieronymus Braunschweig enjoyed the greatest fame as an author, as well as for his course of chemical operations. Johann von Gersdorf was also a distinguished teacher at the same school. Still, when we compare all the writings of these men, and indeed all the other works then extant, with the contents of Theophrastus's genuine writings, "we will be astonished by the comprehensiveness of his views, as well as by many new facts related, and also by the pertinent and simple observations made thereon." (p. 90.) Theophrastus had a high idea of the vast extent of medical science, but he maintains that its different departments were united by an essential connexion, and could not be fathomed singly. The philanthropic ideas of Theophrastus were in like manner far beyond the cold spirit of his age. "The highest thing," he says, "which a physician possesses is art, then love and hope. Love teacheth the art, and hope giveth the right confidence. The medical man should be mild, true, serious, and reserved in speech. But genuine art does not consist in knowing, but in doing and accomplishing." (*Grosse Wundarz.* l. 1, c. 1.)

Anatomico-physiological knowledge did not exist then in Germany, and there is scarcely a trace of it in Theophrastus's writings. But these contain a large store of medical axioms, many of which are really astonishing for the time he lived in. We will here transcribe a few of his opinions at random. He says, that for the elucidation of internal disease, anatomy is not essential, as the knowledge of the structure of the brain would never explain epilepsy; still Theophrastus acknowledged its

importance in cases of surgery. He compared the general integuments to a bladder, which perspired some part of its contents by the pores; and states that, by its secretions, the physician may judge of the disease. Menstruation is an excretion of the uterus. Blood attracts nourishment, digests it within itself, and expels the heterogeneous parts. The vessels in the lungs are, as it were, the stomachs of the organ ; in them the pure is divided from the impure; what agrees with them they keep, the remainder they throw out by their tubes through the mouth. Not all heat comes from the heart, but every limb has its heat from itself. The child is developed in the womb of the mother like a germ of a plant in the soil, which passes from one form into another. The secret force which matures every organism to perfection, Theophrastus calls *archäus*, or *adech*. "It is the inside smith who hammers all right out of his own iron; the labourer who, without man's participation, works his own devices." (p. 98.)

General pathology, or as it was formerly called, the philosophy of disease, was one of the favorite topics of Theophrastus. The following are some specimens of notions in this department:

"In every disease, there is to be observed the aberration of nature, or the manner in which nature transgresses the fixed limits; then the hidden essence of the disease, and, lastly, the time, of which no one heretofore had thought. Disease is a very relative state: As there are various degrees of softness in silver, and yet every kind is silver, so is it with the alteration of health. It is as hard to find perfect health as a piece of earth without a weed; disease is essentially immaterial, and as little to be laid hold of as the wind; we must not then expect to remove it by mere material means. Every disease does not manifest itself openly, but frequently conceals itself under a foreign aspect; and this the doctor should well look to. Disease commonly originates in the disharmony of parts, when the individual ones are unable to maintain themselves in due proportion of quantity or quality; consequently, the first origin of all diseases is an *oportet*—a thing that must be (ein Oportet, das ist, ein gemusst Ding.) The essence of diseases is various, and the doctor who knows not this is blind. The predisposition to diseases, as well as to moral conduct, is often hereditary (aller anfang ist im Vatter). The old doctrine of the four elements and the four cardinal humours is all fudge, and the doctors hid their lies in the humours. The humours are, in truth, born of the disease, not the disease of them. The physician who finds all diseases in the humours prescribes nothing but evacuants. The snow makes not the winter, but the winter the snow, and the winter may last, though the snow be gone. Plants have diseases, but we look not in them for black and yellow bile, neither for melancholia, nor phlegma, nor cholera. The perspiration is salt, pressed from the blood which contains salt, but can a salt be considered a humour?" (p. 99 et seq.)

In his zealous but perhaps justifiable admiration of Theophrastus, Dr. Marx goes on to say that he was the first who laid the foundation of a geographical nosology and medical topography. The following passage expresses the views of a philosophical observer and deep thinker, and could hardly have been penned by one who had not travelled far and seen much:

"Every nation possesses its medicine within itself. I can well imagine that my recipes are ineffectual with foreigners, and those of foreigners with us. I write for Europe; whether my prescriptions will do for Asia or Africa I know not. As every day has its own evil, every religion its own wrong, so has every nation, province, valley, and climate its own form of disease. If every physician were to describe the physical properties of his own locality, and this be done

generally, it would be possible to compose a medical work of lands and water as truly as a map of the globe. (p. 106.)

And Paracelsus lost no opportunity of putting his own doctrines into practice. Thus he tells us that calculus is very frequent in Carinthia; while in the Veltlin it, as well as gout, is unknown. (p. 107.)

"We need not expect," says Dr. Marx, "to find any such words as *general therapeutics* in the writings of Theophrastus; the doctrine was of later origin; but the thing itself is there. It is not to be supposed that so thoughtful a physician had not fixed principles on which to operate, or that he examined and knew so well the natural processes of cure without attempting to imitate them. Hear some of his axioms:

"'Merely to cure is not medicine; but well to cure the present and guard against the future malady, that is true art. .... Nature does not allow herself to be forced or drawn to another course; you must follow her, not she you. If you apply treatment which does not suit her, you injure her......... That is good doctoring, to drive out a disease in the way nature likes; but it is a bad doctoring to dare to get rid of it after your own fashion. .... Every one should consider that a physician is only the servant of nature and not her master; consequently medicine has to follow the will of nature.'" (p. 107.)

It is impossible to read these axioms without recollecting the almost identical words of the greatest of all philosophers: *Homo Naturæ minister et interpres*, &c.

"The wonderful and almost divine faculty in the living organism of self-preservation and of remedying disease, was recognized by Theophrastus most strikingly in the healing of wounds. Full of admiration of this independent agency, he exclaims 'Not without reason do I call nature the physician of wounds!' (Ich nit unbillich die Natur ein Arzt in der Wunden heiss.) And in the following axioms relating to these and other surgical affections, he exhibits the same thorough knowledge of the nature of sanatory processes: 'Warily must the chirurgeon take heed not to remove or interfere with nature's balsam, but protect and defend it in its working and virtue. It is the nature of flesh to possess in itself an innate balsam which healeth wounds. Every limb has its own healing in itself: Nature has her own doctor in every limb. Wherefore every chirurgeon should know that it is not he, but Nature, that heals.' What do wounds need? Nothing: for inasmuch as the flesh grows from within outwards, and not from without inwards, so the surgery of wounds is a mere defensive, to prevent Nature from suffering any accident from without, so that she may proceed unchecked in her operations.'" (p. 108.)

We suspect that it is only very recently that the generality of even modern surgeons were so thoroughly imbued with the principles on which wounds should be treated; and we fear that there yet exist not a few whose "nimia diligentia" would do well to be schooled in the doctrine of our Theophrastus's balsam. We cannot refrain from quoting yet a few more of these axioms, all showing the same penetration into Nature's secrets:

"In fractures, also, Nature does the whole work herself; all external help being of little moment. Animals show how little need of art there is in these and other cases. The dog licks his wound well; the broken rib of the ox knits of its own accord."

"Time also," he says in another place, "manifests a corresponding power. As long-continued rains at last cease, so there are certain diseases which last long, and when their evil is spent with time, they also stop at last........... Nature knows when she should cure; but not always the doctor; wherefore his business should be to protect nature..... ...... If the doctors would make

a clear conscience they must confess that many would get well sooner without them than with them, if they would only turn over their work to Nature."

"We must go back to the first causes of the disease, and not dwell on mere symptoms. We do not endeavour to extinguish smoke, but fire; smoke may indicate fire, but it is not the fire after all. Disease cannot cease, if its cause be not removed." (pp. 109-10.)

The question, also, whether the symptoms of a malady are to be combated by such remedies as produce similar or opposite effects, occupied Theophrastus's attention. He did not coincide with the opinion that every symptom is to be combated by producing the opposite; still he did not adopt that strain of argument which modern homœopathists have carried *ad absurdum*, "That contraria a contrariis curantur, i. e. that heat drives out cold, is false, and never was the case in medicine: but health and disease—they are opposite."

The merits of Theophrastus, as far as the *materia medica* is concerned, were the first to be more generally acknowledged. Theodore Zuinger says of him in his Opera physiol. medica, (Basil. 1610, p. 56:) "Pharmaceuticam subtilem et ingeniosam et efficacem primus Theophrastus P. Helvetius novæ methodi legibus adstrictam, ex latebris chymicis produxit, et ex acroamatica popularem et exotericam fecit." He urges the use of vigorous remedies, and endeavours to understand thoroughly their manner of acting. He recommends simple recipes, proper combination, and an accurate fixing of the time they are to be taken. The main object is to rouse strength; maladies being rather dynamic than material (corpora), we must oppose immaterial to the immaterial. He is much in favour of soothing and quieting remedies, "wherethrough nature, like a man mad with drink, was brought again to reason." He denounced those powerful caustics so much used in his time, viz. corrosive sublimate, white and yellow arsenic, alum with vinegar, vitriolum rubrum ustum, &c. He was perfectly aware of the noxious effects of the abuse of mercury, which, he said, caused dysentery, salivation, cough, phthisis, vehement pains and weakness of the limbs, putrescency. The strength of remedies, he says, it is very difficult to determine. The medicine has to act against disease like fire, and is not a single spark sufficient to burn a wood? consequently the dose of medicine is not of great importance. Here he adopts in full the homœopathic principle. Against the druggists of his time he was very wroth, especially for their playing at medical men; not being aware, he says, that the man who can boil a fish may still be no fisherman!

The following admirable remark is even beyond the most liberal views of our own times: "To spread popular medicine is not only desirable, but a duty. The real aim of medicine is to help the sick, for whose sake the knowledge of remedies should be universal. This would not injure medical men, because experience and tact could never become public property. How much would I bear and suffer," he adds, "to see every one become his own shepherd!" (p. 119.)

Theophrastus knew the heroic drugs as well as any of his contemporaries; and it is because he knew them so well that he used them with the greatest caution. As he, however, liked efficient remedies, and thereby effected great cures, the use of poisons was imputed to him.

This imputation was most probably levelled at his using preparations not generally known. Although he was partial to efficient remedies, he did not deny that there were remedies which could not exist in the form of prescriptions. "The whole world," he says, "is a druggist's shop (*apotheke*); it is but one who manages the pestle all over the globe." He laid also a great stress on diet and regimen, saying "that health must be attended to as well as disease." To the use of water, as well pure as mineral, he ascribed great virtues, and he states that the latter could be made artificially; an assertion which has been strikingly justified in modern times by the successful exertions of his countryman Dr. Struve.

The disease which then most strongly occupied the attention of medical men was syphilis; and as its various forms could not be divined by the common semiotic indications, viz. inspection of urine or the feeling of the pulse, Theophrastus called the then usual proceedings of that kind mere impostures. He says that a person may judge from the urine whether there are stones either in the bladder or the kidneys, and of what sort they are. He recommends to medical men the study of the physiognomy of diseases. He says that he who, through a wrong diagnosis, assumes an evil to exist when it does not, plants one in the body. In reference to these and other proofs of Paracelsus's knowledge of pathology and therapeutics being so superior to that of his contemporaries, Dr. Marx says:

"Whoever will reflect on the previous and then state of these sciences, and compare it with the views and proceedings of Theophrastus, will be compelled to admit that an unusual power, nay, if we may so speak, a sort of inspiration of knowledge of things unheeded of until then, was operating within his mind. Notwithstanding the undigested data he had before him, and other obstacles which he had to encounter, he will appear as a man who, whether understood or not in his own time, has been the forerunner of subsequent beneficial reforms." (p. 127.)

The diseases which occasion some solid deposit of matter, and which Theophrastus calls *tartaric diseases*, occupied much of his attention:

"It is stated," he says, "that there are but two cavities where stones may grow, the bladder and the kidneys; but there are far more, and in every part of the body solid substance may be formed; and these vary with the nature of the locality, a fact of consequence for the physician to know, as he must vary his means accordingly.....When sound, the lungs move freely up and down, and receive and emit air; but when the air-passages are obstructed with tartar, then ensue various diseases, variously named by physicians asthma, cough, &c.; but they all are from tartar, and they terminate in phthisis." (p. 135.)

The surgical labours of Paracelsus met with less opposition from his medical contemporaries than did his Medicine. We would gladly enrich our pages with some more of his striking axioms on this head, but our waning space forbids, and we must conclude our hasty and imperfect analysis of this extraordinary production with the following passage, which closes the Memoir of Dr. Marx:

"From his very first outset in public life to his death, the constant aim of Theophrastus was to disseminate his conscientious opinions in spite of all opposition. His memory should therefore be held in honour, and Germany must no longer allow his name to be reviled and scorned: on the other hand the illu-

sion must be abandoned that in his works are to be found proofs of all sorts of knowledge and discoveries. His writings had a great temporary object, and this has been achieved. Neither their form nor the character of their contents make them fit for the study of posterity. The aims of Theophrastus were, to untie the fetters of tradition; to lead the way to new medical truths; to make German physicians conscious of the value of their language, as well as the richness of their own scientific resources, and to oppose every abuse of medical practice. As in the progress of time all these aims (albeit not always in the sense of his intentions and according to the impulse *he* gave) have been realized, and consequently all his wishes and hopes actually accomplished, the sphere of his activity is closed; and history has done enough, preserving gratefully the recollection of his name and deeds."

Art. XIV.

Lectures on the Diseases of the Urinary Organs. By Sir Benjamin C. Brodie, Bart. F.R.S., Serjeant-surgeon to the Queen. Third Edition, with Alterations and Additions.—*London*, 1842. 8vo, pp. 379.

Under ordinary circumstances, when a book has reached a third edition, public opinion has stamped its value, and the praise or blame of the critic is likely to pass unheeded; but the work before us is, to a certain extent, rewritten, and the author has been able to bring to bear a large mass of experience obtained subsequently to the publication of the previous edition upon certain points still undecided, but of great importance; and these form a legitimate subject for examination in the present notice.

In reviewing the productions of distinguished men, it becomes us to differ with humility, to controvert with diffidence, but to offer our own opinions with frankness and honesty. Sir B. Brodie is the first surgeon in a country which has produced some of the greatest names of modern surgery; in his favour popular opinion and the judgment of professional rivals have concurred; and without any of that warmth of manner which excites enthusiasm and calls forth troops of friends, he is placed by common consent at the head of his profession. It has been the lot of many men who have occupied that position to have been forgotten when the grave closed over them; they had contributed nothing to posterity, and by posterity they were not remembered. The sagacious practitioner is not always the successful author, and often leaves nothing by which those who follow him can judge whether success depended on good fortune rather than merit, on personal rather than professional recommendations. It is not thus with Sir B. Brodie; and when as the successful practitioner he is no longer remembered, he will be known as the author of two works, which, on the subjects to which they relate, are the best we yet possess.

Many circumstances have concurred in our day to extend more widely than formerly a knowledge of the literature of our profession; and the standard works of all countries are now studied by the well-educated surgeon. There may thus be found in contemporary publications an acquaintance with the experience of other times and other countries more complete and extensive than that which is exhibited in the work at the head of our present article; but that work possesses merits of a

higher kind than any literary excellence can give, inasmuch as the writer thoroughly understands the diseases of which he has treated, and is acquainted with the best methods of cure; and instead of reading the works of other men in order to write a book, he has from his own experience furnished materials for extending that particular department of medical science to the cultivation of which his life has been devoted.

The earlier portion of the work before us is devoted to the consideration of stricture of the urethra. As compared with the last edition, we do not see in this any important alterations to call for remark; and as our last number contains a review of several works on that affection, we shall offer only a very few observations upon it here. We entirely agree with the author that in a great majority of cases any means with which we are acquainted are insufficient for the *cure* of ordinary organic stricture: by which we mean a contraction, dependent upon the development of indurated matter, in the submucous tissue. Yet how many men there are in this metropolis at the present moment who are quite ready to undertake to *cure* any case of stricture which may come in their way! Where the disease is recent, where the new matter has not acquired great density, there can be no question that the pressure of a bougie may be sufficient to cause its removal, just as an induration of an inguinal gland may be dissipated by the pressure of a truss; but then great care in the application of the pressure is necessary, or the evil may be increased. The rule for the guidance of the practitioner should be this: Let the bougie be introduced as often as it can be borne without occasioning much and increasing pain or inconvenience; and the same circumstances must determine the length of time during which it should be retained in the canal. There is an objection, however, to too rapid or "vital" or "permanent" dilatation, as it has been sometimes termed, and it is—that the canal more quickly resumes its contracted condition than when it is more gradually effected.

With reference to the treatment by *nitrate of silver*, we do not think our author has devoted sufficient space to the subject, taking into the account the potency of the remedy, and considering its importance. Whatever he may think of this mode of treating stricture, it is necessary, at least we think it would have been better, that the *pros* and *cons* should have been more fully stated, because the enthusiasm once excited in its favour by Home is not quite extinguished. It is hardly correct to say that cauterization was first proposed by Mr. Hunter. It is certain that caustics were used by Alphonso Ferri, by Paré, by Soyseau on Henry IV. of France, and by our own Wiseman, long before the time of Hunter.

Sir B. Brodie almost limits the use of the nitrate of silver to the treatment of those strictures which are attended with much irritability or spasm; and certainly the effect is then, in many cases, very remarkable. Sometimes a single application is sufficient to dissipate the irritability even when it has been so great as to occasion spasm, whenever the urine came in contact with it, as well as to remove any induration which may exist: just as the hardened edges of an irritable ulcer will give way to the application of lunar caustic over the surface.

So much of inaccuracy pervades the ideas commonly entertained with regard to cauterization of the urethra, that we are induced to make a

few remarks, with the hope of correcting the evil. One of two objects is to be attained by the application of nitrate of silver upon the mucous membrane of the urethra: either a modification of an exaggerated sensibility, or a destruction of substance. With respect to the attainment of the first object, it is no longer a matter of doubt. With regard to the second, some observations are necessary. The induration is rarely, if ever, confined to the mucous membrane, which in many cases appears to have undergone little or no change; it is found to be very much confined to the submucous tissue. Now if the nitrate be employed for the purpose of effecting the destruction of the indurated mass, it must be evident that before this can be accomplished, there must have been complete destruction of the entire thickness of the mucous coat, there must have been destruction of the submucous hardened tissue, and there must be a subsequent contraction of the surface in the work of cicatrization: in fact there will be a *new* contraction which may require for the remainder of life the daily use of a bougie to overcome. We apprehend, therefore, that the plan of treating stricture by destruction, through the agency of nitrate of silver, is one which must be abandoned. Although this is the principle upon which that plan of treatment is supposed to be founded, we doubt whether it has often been carried out. We think that the mode in which it has been attempted to carry it out can only very rarely have accomplished the desired object, and for these reasons: the mucous membrane of the urethra is as thick as that which lines the cheeks; now if a piece of lunar caustic be kept in contact with the latter membrane for a minute, a slough will be formed; in a few hours it will be thrown off, and if in a couple of days afterwards the part be examined, no change of texture will be detected, and many applications upon the same point at short intervals will be required to produce any considerable change of structure. Although, then, we do not deny that the complete destruction of the mucous membrane may be accomplished by the repeated application of lunar caustic, we believe it to be happily a rare occurrence.

As to *cutting instruments*, we have always regarded their employment with much apprehension: the chance of injury to healthy parts is so great; the probability of the incision, when made, being confined to the proper point, is so small; the means of attaining the object by safer agents are so obvious, that we wonder the lancetted stilette is any longer found in the armamentarium of prudent surgeons. If, says our author, any cases occur in which this method may be useful, they are undoubtedly very few in number.

We would desire to impress upon young surgeons the following advice by one so competent to advise on the subject:

"But I cannot too strongly impress it on your minds, that in the treatment of stricture you ought not to use violence under any circumstances. Your success in the cure of this disease will depend very much on your attending to this important rule. Whether you use a bougie, or a sound, or a catheter, let the instrument be held lightly, and as it were loosely, in your hand; it will then, in some measure, find its own way in that direction in which there is the least resistance; whereas if you grasp it with force, the point can pass only where you direct it, and it is just as likely to take a wrong course as a right one. A stricture will invariably resent rough usage: it will yield to patience and gentle treatment." (p. 68.)

In Chap. vi. there is a section of great value, showing that diseases of the kidney may be manifested prominently by symptoms affecting the bladder, while the ordinary symptoms of diseased kidney are marked: "Whoever is much engaged in this branch of surgical practice will meet with a number of facts which cannot so well be explained on any other hypothesis, and which collectively form such a mass of circumstantial evidence as is almost irresistible in favour of the opinion that the worst symptoms of irritable bladder may occur as a consequence of disease of the kidney, the bladder itself and the organs in immediate connexion with it having been free from disease in the first instance." (p. 125.) Cases are mentioned which seem to afford sufficient evidence as to the existence of symptoms referred to the bladder and urethra in some cases of diseased kidney: "But with my present experience I am led to this further conclusion, that a very large proportion of the cases which have usually been confounded together under the general appellation of irritable bladder are really of this description." (p. 131.) But then it is equally certain that the kidney has been found much diseased while the bladder was free from trouble. "The question therefore arises, in what particular cases of renal disease it is that the secondary affections of the bladder are likely to occur," and to this point all our inquiries should tend:

"I have already explained that where the urine is overloaded with acid, showing itself in the form of lithate of ammonia, or brown or red sand, or where, being alkaline, it deposits crystals of the triple phosphate of ammonia and magnesia, it acts as a stimulus to the parts with which it comes in contact, and that an irritable state of the bladder is the consequence. But there is no reason to doubt that other unhealthy secretions of urine may produce the same result; and I am much inclined to believe that such is the real explanation of the affection of the bladder in the cases which are now under our consideration. In such of them as have fallen under my observation, the urine has been always altered from its healthy condition, and its sensible qualities may be described as follows: There is usually a copious secretion, the specific gravity being below the ordinary standard. But there is some variety in this respect; and I have known the specific gravity to be as high as 1030. When tested with litmus paper, it is generally found to be slightly acid, but occasionally it is alkaline, or it is sometimes alkaline and sometimes acid, and, as I shall explain hereafter, the disposition in it to become alkaline increases as the disease advances. When first voided, the secretion is of a pale yellow colour, opaque, and turbid, sometimes having minute flakes of lymph floating in it. On the addition of nitric acid, or on exposure to heat, there is an abundant coagulation of albumen. When allowed to remain at rest, there is a deposit of opaque matter, and not unfrequently of pus. The urine is always albuminous, but quite different in appearance from that which is secreted in the cases which were first described by Dr. Bright, and to which the attention of physicians has been of late years so much directed. The albuminous matter seems to be mechanically suspended, and not intimately blended and assimilated with it; as if the kidney were in a state of chronic inflammation, secreting urine from one set of vessels, and serum or even pus from others. Such, probably, is the real nature of the disease when once established, whatever it may have been in its origin." (pp. 131-3.)

Though we must admit the practical force of much of our author's reasoning on this very important subject, it is not to our minds conclusive; and we think further opportunities of observation will serve still more to widen the question. We apprehend that many different affec-

tions of the kidney may produce a similar condition of the functions of the bladder. We have seen symptoms of disease of the bladder in what appeared to be simple nephritis, the urine having undergone little or no change; in calculus of the kidney, where there was for years no pain in the lumbar region: they may also happen in pyelitis, as well as in chronic albuminous nephritis. As the disturbance in the bladder may occur under such varied circumstances, and may also be absent under each of them, it must be evident to everybody that, though Sir B. Brodie has done much to throw light on this obscure point of pathology, still more remains to be done to determine what particular abnormal condition of the kidney manifests itself by the development of those remarkable symptoms in the bladder.

Besides the case cited from Morgagni there is another (Ep. lxii. sect. 5) observation by Harder, related by Bonnet, (Sepulchretum, lib. iii. sect. xxvii. obs. 10.) A paraplegic child, three years old, was attacked with convulsions, and showed by its gestures that it suffered violent pain in making water, without ever having complained of renal pains. After death the bladder was found to be healthy, but a large quantity of sand could be squeezed out of the mammillæ. Lowdell (Mem. Med. Soc. vol. i. p. 319) describes the case of a woman of twenty-two, who suffered much from pain in the bladder, but who never complained of pain in the region of the kidneys. The calices of both kidneys contained large irregular calculi. Mr. Howship mentions a case (Urinary Organs, p. 40) where there was great pain at the neck of the bladder, with frequent desire to make water, the urine containing thick mucus. The bladder was found perfectly healthy; the right kidney was converted into a purulent sac. It is no doubt rare to find similar facts; but it is not so rare to find considerable disease of the kidneys in persons who never complained of lumbar pains, and who had suffered from irritation in the bladder. How many persons have undertaken to extract calculi from the bladder after carefully examining into the evidence of renal disease, and satisfying themselves that they had nothing to fear in that quarter—have well performed the operation, and quickly lost their patient, and after death have discovered inflammation, pus, or calculi in the kidney! In the present state of our knowledge the difficulty is to point out what particular condition of the kidneys it is which determines the prominent bladder-symptoms. It is clear that it must be some element common to the several affections which have been alluded to, and probably it is a particular form of inflammation.

There are not wanting books to prove that *chronic enlargement of the prostate* is a *very curable* disease; and as patients are apt to take their opinions in such affairs from those who promise most, and as young practitioners are not unfrequently led to encourage hopes which end in disappointment, we would particularly direct their attention to the following remarks, in every word of which we concur.

"At different periods of human life different changes take place in the condition of the organs of which the system is composed; and none of these are more remarkable than those which show that the individual has entered on that downward course which is to end in dissolution............ When the hair becomes gray and scanty, when specks of earthy matter begin to be deposited in the tunics of the arteries, and when a white zone is formed at the margin of

the cornea, at the same period the prostate gland usually—I might, perhaps, say invariably—becomes increased in size. This change in the condition of the prostate takes place slowly and at first imperceptibly, and the term, chronic enlargement, is not improperly employed to distinguish it from the inflammatory attacks to which the prostate is liable in earlier life." (p. 152.)

This is a condition almost as little under the dominion of art as the deposits in arteries.

"If you bear in mind that the chronic enlargement of the prostate gland to which I called your attention, is not an accidental disease, but one of a series of natural changes which the system undergoes after the middle period of life, you will not be surprised to find that it is but little under the dominion of art. .... We are not acquainted with any method of treatment which is capable of restoring the gland to its original condition............ Nevertheless, in these cases, much may be done by means of proper surgical treatment. The prostate of a man advanced in life cannot be rendered like that of a young man, any more than his gray hairs can be converted into black; but the train of evils which the enlarged prostate produces by its influence on the urinary organs may be in some instances altogether prevented, and in others very much diminished, so as to remove the patient from a state of extreme and even immediate danger to one of comparative security." (pp. 173-4.)

There is a caution with respect to *retention of urine from enlarged prostate*, to which we particularly direct attention.

"There are some [cases] in which much discretion is required in resorting to the use of the catheter. What I am about to state is not an opinion formed hastily, but a deliberate conclusion to which I have been led, after having had for many years no small share of experience in the treatment of these disorders, as well as considerable opportunities of investigating the morbid appearances which they leave behind them in the dead body. If in a case of chronic enlargement of the prostate the patient has been allowed to go on for two or three years, or longer, without the use of the catheter, and in consequence of this neglect the quantity of residuary urine in the bladder has gradually increased, so that at last one or two or more pints are accumulated in it, the kidneys having at the same time become diseased, the introduction of the catheter according to the rules formerly laid down, so as to empty the bladder two or three times daily, is likely to be injurious rather than beneficial. The patient is, it is true, relieved of many of his distressing symptoms. He is no longer tormented by a frequent desire to void a small quantity of urine, nor by an involuntary dribbling of urine during the night; nor does he suffer the uneasy sensations which, in a greater or less degree, always attend an over-distended bladder; but in the course of a few days it is observed that he avoids his usual exertion, that he seems languid, and loses his disposition to take food. Then the other symptoms of disease of the kidney, which were imperfectly developed before, become distinctly marked, and he gradually sinks and dies at the end of a month or six weeks from the time of the catheter being first employed." (pp. 189-90.)

Our own experience enables us to confirm the correctness of this statement; but why it happens is not easy to say, though it is probably owing to the sudden removal of pressure from distension of the kidney, to its inability at once to adapt itself to its altered circumstances, and to a consequent more or less complete suppression.

"Here then arises an important practical question. The patient has no chance of recovery without the use of the catheter. Are we to leave him to his fate? or are we to empty his bladder at certain intervals, at the risk of hastening the period of his dissolution? I have no doubt that we may, in many instances at least, obtain the good and avoid the evil by a slight modification of

the treatment. Let the catheter be introduced at first so as to draw off only a portion of the contents of the bladder, and let several days be permitted to elapse before it is completely emptied; care being taken at the same time to uphold the general health by the exhibition of ammonia, quinine, and other tonics, exhibited according to circumstances, and combined with the prudent use of wine or brandy, and a plain but nutritious diet." (pp. 191-2.)

The portion of the work devoted to stone in the bladder is particularly interesting. Our author adheres to the commonly received, but to our mind incorrect, opinion that among the lower classes children are *much more* liable to calculi than grown up people. "You know how large a proportion of hospital patients admitted are children." Now how stands the fact? We take one collection of cases amounting to 5967, and we find them thus divided: under 14, 3031; above 14, 2396 cases; no great difference. But when we dissect the returns made up from different countries, we are struck with the great variation in the proportion of children to adults in the different returns.

Thus of 356 operations	reported by	Dupuytren	97 only	occurred in	children under 15.
1629 ,,	,,	Saucerotte	1195	,,	,,
478 at Norwich	,,	Marcet	227	,,	,,
666 at Naples	,,	Renzi	315	,,	,,
643 ,,	,,	,,	321	,,	,,
469 St. Petersburg			357	,,	,,
175 Austria			55	,,	,,
239 Bavaria			116	,,	,,
106 Bohemia			28	,,	,,
145 Denmark			14	,,	,,
42 Egypt			1	,,	,,
2822 France			1347	,,	,,
1058 Lombardy			796	,,	,,
49 Rome			10	,,	,,
140 Sardinia			97	,,	,,
127 Ulm			64	,,	,,

Taking large numbers it would appear that calculus is little if at all more frequently found among the children than the adults who are treated in hospitals. If we examine into the frequency of its occurrence in children's institutions, we are struck with the comparative immunity from it enjoyed by them. In 27 years 1151 children were admitted into the Foundling hospital, and in the same time only three cases of stone occurred there. In the Military Asylum at Chelsea, out of 6000 admissions not one case was furnished. In the Hôpital des Enfans, at Paris, where 3000 children are received annually, during the last 7 years the average has been under four. At the St. Marylebone workhouse, where there has been on an average during many years between 400 and 500 children, there has not been a single case of stone either among them or the aged persons during the last twenty years.

There is something very obscure about the formation of calculi: why they prevail so extensively in some districts, so unfrequently in others; why London should be so free from them, while in Norfolk they are so rife; why in particular localities the majority of sufferers are children, while in others they are a small minority,—are matters which in the present state of our knowledge are inexplicable. The numerical results show that infancy is the period of life in which stone in the bladder is most frequently observed, though the comparative frequency has been exaggerated. Mr. Cross attributes the frequency of the occurrence of stone in

the bladder in Norfolk and Suffolk to the influence of the cold north-east winds, which prevail in those counties, over the functions of the skin. This opinion may be reconciled to that of Magendie, who endeavoured to account for the frequency of calculous disorders in old men by the diminished temperature of the body, which contributes to favour the concretion of uric acid.

"There is a class of cases which being of rare occurrence do not seem, in the present state of our knowledge, of much practical importance, but which I am unwilling to leave altogether unnoticed, especially as they exhibit a phenomenon of much interest in pathology." This is the disintegration or spontaneous breaking down of stone in the bladder. A case is related from Heister, where it was supposed to have been brought about by the use of mineral water—another from Dr. Prout where no mineral water had been used—and two from Mr. Cross, in one of which it is inferred that the breaking down had been the result of a jolting on horseback, and two or three others. Sir B. Brodie says, "I give you these facts without any comment. Future observations are required to enable us to give a satisfactory explanation of them." Facts of this kind are not however of such rare occurrence, neither are they of such recent observation as seems to be thought. They have been observed by Borrich; Detharding, (Haller's Theses, 110;) Geoffroy, (Acad. 1739;) Whytt, (Edin. Essays, vol. vi. p. 263;) Velpeau, (lib. iv., cap. 37, p. 333;) Deschamps Morand, (Mém. de l'Acad. 1740-1;) and Rousseau and Civiale.

In some cases it occurred during the use of alkalis or acids; in others without any plan of treatment; in others "by rubbing against each other;" in others by contraction of the bladder upon them. Each of the alleged causes applies only to a small number of cases, while in similar or identical cases the agent has been without effect. Thus much we know—that a disaggregating influence was exercised on calculi submitted to the action of Vichy water—that a bladder containing calculi is often hypertrophied—that it acquires an increase of contractile power—and that a certain pressure may thus be exercised on the contained calculi. Covillard and Fabricius Hildanus mention the cases of some patients who could hear the calculi rubbing against each other in the bladder. In a case described in the Phil. Trans. 1731, the patient's bladder contracted and produced a sensation of something breaking in it, and the next minute he passed many fragments with the urine.

Before the operation for extracting the stone be undertaken, it is proper that every probable means of ensuring success should be taken. And it is therefore very important to establish what are the circumstances, if any, which should induce the surgeon to refrain from the attempt to remove a calculus from the bladder. None of those circumstances demand more serious attention than the state of the kidney.

"Success in lithotomy must undoubtedly depend in a great degree on the manual dexterity of the surgeon, and on the mode in which the operation is performed; but it depends still more on the condition of the patient with respect to his general health, especially on the existence or non-existence of organic disease. . . . . Before determining on lithotomy, you have no more important duty to perform than that of enquiring into the state of the kidneys. . . . . One thing to be especially attended to with a view to a correct diagnosis, is the state of the urine. The urine may be alkaline, and thus in an unnatural state, and yet the kidneys may be free from organic disease and the

patient a proper subject for the operation. It is purulent and turbid urine loaded with albumen by which your apprehensions as to the result of an operation will be chiefly excited." (pp.243-4.)

Mr. Edwin Lee* does not seem to regard the existence of such organic disease as a bar to lithotrity. "In some cases of this description," he says, "lithotrity might be had recourse to with advantage, the shock to the constitution being comparatively small, and the irritation from this operation being less likely to extend to the diseased organ." We are, however, of Sir B. Brodie's opinion "that a small shock to the system will sometimes destroy the life of a patient who labours under renal disease, and it will be often more prudent to trust to the means which we possess of palliating his sufferings than run the risk of shortening his life in the endeavour to obtain a cure."

After a good description of the operation of lithotomy, we are carried by Sir Benjamin into the consideration of its dangers; and the most pressing of them is summed up in these words:

"All that I have been able to observe for many years past has confirmed me in the opinion, that an incision of the prostate extending into the loose cellular texture surrounding the neck of the bladder is replete with danger to the patient. Such a division of parts is never necessary where the calculus is of moderate dimensions, but it cannot be avoided where it is of a very large size; and hence the extraction of stones of this description can never be accomplished without a great probability of the patient not surviving the operation." (p. 327.)

This view of the case, as to the common cause of failure, is confirmed by the experience of Dupuytren. "To determine," says he, "why one in five or six die, it is necessary to ascertain what are the ordinary causes of death, and we find that among those who die after lithotomy, more than half, near three fifths, perish from inflammation about the bladder, the cellular tissue of the pelvis, the kidneys, &c." But what are the causes of those accidents? Sometimes we find the neck of the bladder and the left half of the prostate largely and neatly divided by the cutting instrument, the incision extending into the cellular tissue which surrounds the fundus and the sides of the bladder. "In fact these large incisions determine those inflammations, because they pass the limits of the neck of the bladder and that of the prostate, and because they allow the urine to come in contact with the pelvic cellular tissue."

The danger of large incisions is not a new discovery; we do not owe it either to Scarpa or Chaussier, though they may have more particularly pointed out the reason. It was a matter of long and very warm discussion between Lecat, Louis, and Frère Côme. The former composed three volumes to show the advantage of small incisions.

Now as to the extent of the incision there is much to be said in another point of view, the proportion of the bulk of the calculus to the incision. On the dead body of a child an incision of the prostate to the amount of three lines will allow of the extraction of a calculus of eight lines diameter. In a man of twenty an incision of six lines through the substance of the prostate will permit us to extract a calculus of fifteen lines diameter. In a man of forty an incision of eight lines will admit a cal-

* Prize Essay on the comparative advantages of Lithotomy and Lithotrity. (Ed. Med. Journ., No. 150.)

culus of eighteen to twenty lines to pass. Many apparently brilliant lithotomists are very unsuccessful, and we believe this is owing to the fact that they make an incision *so free* as to allow of the easy and immediate extraction of the stone; these free incisions pass beyond the limits of the prostate, penetrate beyond the pelvic fascia, and open a communication into the pelvic cellular tissue. All goes on well for forty-eight hours or more, and in as much more the patient dies, because the operator has followed the rule to cut the prostate gland "*through the whole extent of its lateral lobe*," and something more. It is surely quite time that the rule *not to cut to the extreme limit of the prostate* should be universally admitted and acted upon. It is true there are some cases where such an incision will not make the necessary space for the passage of the calculus, but they are not many; and as soon as the operator has satisfied himself of that by passing his finger into the bladder, he should enlarge the opening into the bladder by incising to a sufficient extent the opposite lobe of the prostate. Whether this be done in the ordinary way with the bistoury, or whether by the Celsian method,* but with other instruments, as practised by Dupuytren, is a matter upon which we cannot enter here; it is for the principle alone we contend, that one of the most fertile sources of evil in the operation of lithotomy is the extent given to the incision through the prostate.

We now proceed to that part of the volume which relates to the value of the operation of Lithotrity; and coming as it does from a person who has considerable experience on this much-debated subject, it is entitled to our most serious attention. It will assist us in estimating the confidence to which are entitled the opinions of those persons who would use lithotrity in every case, and of those who would reject it in all.

So much incorrect information exists on the subject of lithotrity, that we may be pardoned in offering a few remarks as to the history of the operation. First, it is a mistake to carry it back to the time of Celsus. The sentence so often quoted does not apply to lithotrity, but to lithotomy, when the stone is too large to be extracted by the wound: "Uncus injicitur calculo, ut facile eum conclusum teneat, ne is retro revolvatur; tum ferramentum adhibitur . . . quod admotum calculo . . . ictum fendit," &c. This practice might be carried back long before Celsus. Ammon of Alexandria being unable on many occasions to extract the stone through the wound, broke it down with a sculptor's chisel, and was therefore called Lithotomos. Whether Alsaharavius performed the operation in the twelfth century we cannot tell; certainly he described it: "Accipiatur instrumentum subtile, quod nominat *Mashaba rebilia* et suaviter intromittatur in virgam, et volve lapidem in medio vesicæ, et si fuerit *mollis* frangitur et exibit; si vero non exiverit cum iis quæ diximus oportet incidi," &c. C. F. Martius (Revue Med., July, 1837) extracts the following statement from an Arab author's disquisition on precious stones: "Un precieux avantage du diamant dont parle Aristote, et que l'expérience confirme, c'est son usage dans les affections calculeuses. Quand un malade est atteint d'une pierre soit dans la vessie, soit dans l'urètre, on prend un petit diamant, qu'on fixe à l'extremité d'une petite

* "Cum jam eò venit (calculus) ut super vesicæ cervicem sit, juxta anum incidi cutis plagâ lunatâ usque ad cervicem vesicæ debet, cornibus ad coxas spectantibus paululum."

tige metallique de cuivre ou d'argent; on l'introduit dans l'organe qui contient la pierre, et on la reduit par un frottement répété." Ebn-ab-Harrar states that he employed this plan on a servant in a case of very large calculus; he reduced it to so small a size that it could be expelled with the urine. Antonius Benivienius, who died in 1502, in a work intituled "Antonii Benivienii Florentini Medici ac Philosophi, de Abditis Nonnulis ac Mirandis Morborum et Sanationum causis liber," published after his death in 1506-7, mentions a case of a woman who for some days was affected with retention of urine, "propterea quod ipsius URINÆ ITER calculo obstrueretur." The disease not having yielded to any of the means tried, the author performed an operation, which is described in these terms: "Uncum calculo injicio, ne scilicet concussus iterum *in vesicam revolveretur.* Tum ferramento priore parte retuso calculum ipsum percutio, donec sæpius ictus in frusta comminuitur." The patient was cured as soon as the fragments came away with the urine.

Haller alleges that an instrument of three branches, and a central perforator, was used by Sanctorius. We think it cannot be denied that the monk of Citeaux relieved himself of a calculus by a percuting instrument. We then come to Martin, Gruithuisen, and Elderton, with whose merits all are familiar, to Leroy, Civiale, and others, whose claims to priority are hardly yet decided, though there is no doubt that with Civiale rests the priority of operating on the living subject.

All the known plans of operating resolve themselves into the following methods: successive perforations—breaking down by crushing—wearing away from the circumference to the centre. 1. To the first plan we may bring the methods of Sanctorius according to Haller and that of Gruithuisen. It is practised with the three-pronged instrument, and it is relative to the priority of invention of this instrument, that MM. Civiale and Leroy have so long been at issue: this was the method employed between 1824 and 1830. 2. *Crushing* either by pressure or percussion: a defective instrument for this purpose was invented by Amussat in 1822. An instrument little more convenient was the *brise-coque* of Heurteloup, presented in 1828. These were followed by two other instruments destined to change the character of lithotrity, the *brise-pierre articulé* of Jacobson, and the *percuteur* of Heurteloup; the first acts by pressure, the second by percussion, but it has been modified to act by pressure also. 3. The third plan, by *excentric destruction*, was invented to get rid of the difficulties and injuries of fragments, but it has not succeeded; it is that of Martin, Meyrieux, Tanihon, and Rigol.

We have now to enquire into the circumstances which justify us in having recourse to the operation: we will, in the first place, quote Sir B. Brodie's opinion; in the second, we will venture to give our own.

"It would be a great error to represent it as preferable on all occasions, but it is so in a great many instances; and I shall next endeavour, as a guide for your future practice, to explain by what signs you may distinguish from each other the cases to which it is applicable, and those to which it is not. In boys under the age of puberty, lithotomy is so simple, and so generally successful, that we ought to hesitate before we abandon it for any other kind of operation."

In this way more than half the cases are turned over to the old operation.

"There is also a manifest objection to lithotrity in these cases on account of

the small size of the urethra, which is such that it would not admit of the introduction of instruments of sufficient strength to crush a calculus of more than moderate dimensions. .... In cases in which the calculus has attained a very large size, it is often difficult to seize it with the lithotrity forceps; the operation of crushing requires to be repeated a great number of times, so that many weeks may elapse before the cure is accomplished; a larger quantity of fragments is left in the bladder, of which the necessary consequence is a great liability to inflammation of the mucous membrane, and of course the inconvenience produced by the passage of the fragments along the urethra is multiplied as compared with what happens when the calculus is smaller. These circumstances form a sufficient objection to the operation of lithotrity in these cases. It is true that they are unfavorable cases for lithotomy also, but I have little doubt that the latter method is the safer of the two. . . . . . The operation of lithotrity is not well adapted to those cases of enlargement of the prostate gland in which the patient is *enabled* [qy. *unable?*] to empty the bladder by his own efforts, unless the calculus be of a small size, so that there may be no great difficulty in washing the minute fragments into which it has been crushed, out of the bladder through a large catheter. There is also another objection to the operation in some cases of enlargement of the prostate, namely, that the tumour which projects from it into the cavity of the bladder makes it difficult to elevate the handle of the forceps sufficiently to seize the stone easily in the usual manner." (pp. 375-6.)

"With the exception of such cases as those which have been enumerated, there are few to which this method of treatment may not be advantageously applied. It may be said that the exceptions are numerous, but they are the result chiefly of delay. If a patient seeks the assistance of a competent surgeon within six or even twelve months after a calculus has descended from the kidney into the bladder, the urine having remained acid, it will rarely happen that he may not obtain a cure by a single operation, and with so small an amount of danger that it need scarcely enter into his calculations. As time advances, the facility with which he can be relieved diminishes, and after the lapse of two or three years, especially if the urine has become alkaline, it is probable that the calculus will have attained such a size as to render the old operation preferable, and that the access of disease in the bladder or kidneys may render any operation hazardous. It would be absurd to say, and it would be unreasonable of human kind to expect, that an operation which has for its object to relieve them of a disease so terrible as that of a stone in the bladder, can be always free from inconvenience, and difficulty, and danger. Nevertheless, from what experience I have had, I am satisfied that the operation of lithotrity, if had recourse to only in proper cases, is not only much more successful than that of lithotomy, but that it is liable to fewer objections than almost any other of the principal operations of surgery." (p. 379.)

There is no difference of opinion on one point, and that is, that when a person is suffering from vesical calculus, if his general health and the state of the urinary organs be satisfactory, he should submit to some operation for its removal. It is now generally admitted that, spite of the brilliant prospects to which it gave birth, the operation of lithotrity is not applicable to every case; and as the fruitless attempts to relieve a patient from his suffering by this operation are not always free from danger, it is very desirable that the circumstances which justify a recourse to it may be placed on an intelligible basis. This Sir B. Brodie has sought to do; but unfortunately in the present state of our knowledge on the subject, it is very difficult to determine with any certainty the rules which should be applied, and which should never be based on any but a large number of observations fairly taken; and which if now laid down may require hereafter very considerable modification. Whether

future observations shall restrain or extend the usefulness of this operation, it must still be considered as one of the most important discoveries of surgery in the present century.

In general terms the operation of lithotrity is indicated, in adult and aged persons of good general health, provided there be no great hypertrophy, or contraction, or paralysis, or increased sensibility of the bladder, or enlarged prostate, or diseased kidneys, provided also that there be a single and not very large or very dense calculus. It is objectionable in children, and where the texture and functions of the bladder are much changed, where there are many calculi, and where they are large or very hard. We do not mean to say that in many of the cases just enumerated lithotrity might not be practised with a chance of success. Numerous facts prove the contrary, but that is not the question ; what we want to know is whether in those conditions, lithotomy would not do better—and this might easily be proved. A large-sized stone is opposed to lithotrity; though we by no means deny that large stones have been successfully removed by the process, but a large stone generally supposes a small contracted bladder; when the bladder has not undergone those changes a stone of twenty or more lines in diameter has been removed. The shape of the stone is also important; the number of stones is no less important, because many stones presuppose many sittings; but if they are soft or friable this may not be the case. Civiale has crushed forty at a single sitting—some of them may have been fragments. Great density is an unfavorable circumstance; the softest are the triple phosphate, next come the lithic acid, lithate of ammonia; the hardest are the oxalate—by these, perforators have been blunted and percutors have been resisted.

The ordinary consequences of the operation of lithotrity must be materially influenced by the circumstances already detailed. In many cases a single sitting is enough. In the 14th observation of Civiale's Parallèle, twelve sittings were necessary for perforations alone. Many short sittings are less objectionable than a few long ones, the patient suffers less; but all this must depend upon the condition of the patient—some can bear without apparent inconvenience a sitting of a quarter of an hour, others are fatigued with two or three minutes. In some the detritus passes quickly, in others not till after twenty-four or forty-eight hours: in some not at all. When many fragments have passed the bladder must be examined with great care, because the smallest particle may become the nucleus of a new calculus. It is one of the objections made to the operation that it thus facilitates the reproduction of calculi. This is an inconvenience which cannot be denied; but whether it attaches more to lithotrity than to lithotomy—whether, in fact, when the bladder is supposed to be completely relieved, the recurrence of calculus is more frequent after lithotrity than lithotomy, is a question we cannot determine. M. Civiale says no.

The occasional consequences of the operation are fracture of instruments, perforation of the bladder, tearing off portions of the mucous membrane of the bladder, rupture of different parts of the urethra, peritonitis, infiltration from injury to the urethra, swelled testicle, articular inflammation, fever, nervous excitement, inflammation of the bladder, kidneys, or prostate. But of all the accidents attendant upon the operation, that which causes most embarrassment, if not danger, is the infarction of

fragments of calculi in the urethra. Leroy admits that it happens in one case in four; and that he has either broken up or extracted at least six hundred fragments from the urethra (1837). This is certainly one of the greatest inconveniences with which the operation has to struggle. Sir B. Brodie thinks that the means of preventing this evil are very much in our own power. "A state of perfect repose in the recumbent posture, except when it is necessary to remove from one room to another, should be considered as indispensable after the operation; and I venture to say that when this rule is observed it will very seldom happen that the passage of the fragments along the urethra is productive of any serious inconvenience." Still it does happen, and in the hands of very prudent surgeons.

Let us say a few more words on the subject of this important operation. At its first introduction it was fondly thought that all the pain and danger of lithotomy could be avoided. Experience soon proved that the hope was delusive, by unfolding a long catalogue of accidents which, like those of lithotomy, often terminated in the death of the sufferer. To the early enthusiasm succeeded reflection and the necessity of a fair comparison of the new operation with that which it had been destined to supersede. But we are not yet in possession of the data necessary for this comparison. For the complete solution of the problem there should be two series of cases, placed as nearly as may be in the same condition as to age, general and local health, and physical condition of calculi: a similar number should be treated by each method and the results should be exactly followed. That has never been done, and probably never will be. A considerable experience, if not a rigorous statistical examination, has served to produce, in the minds of some of the best surgeons, the opinion that in the condition described as favorable for lithotrity, though it is not free from the chance of accident, it is at all events much more favorable than lithotomy. We have also stated what may be expected from the operation in complicated cases; and we may remark that if lithotrity has so frequently failed it is to be attributed mainly to the unfavorable circumstances under which the operation is undertaken. On the one hand there is the natural tendency of inventors and enthusiasts to exaggerate the benefits to be derived from their invention: on the other hand there is the obstinate determination of many patients to reject a bloody operation, which they dread, to prolong this state of things. But when lithotrity has become an ordinary surgical operation, when more numerous observations, authentic in every particular, shall enable us to appreciate the limits of action of lithotrity, we have no doubt that its results will be much more favorable than they are at present: but to accomplish this its application must be restrained. We think there should always be a choice exercised; and as it is in the earlier periods of the disease that the most favorable conditions are presented, we may say that a comparison between these two methods of treatment can scarcely be instituted; they are applicable to different periods of the same disease. The cases which might be rejected by the lithotritist would fall into the hands of the lithotomist: that operation would then be more unsuccessful than it is at present, because it would only be used in complex cases.

Art. XV.

1. *Die Perkussion und Auskultation.* Von Dr. Joseph Skoda.—*Wien*, 1841. 8vo, pp. 271.

Percussion and Auscultation. By Dr. Joseph Skoda.—*Vienna*, 1841.

2. *A Practical Treatise on Auscultation.* By MM. Barth and Roger. Translated, with Notes, by Patrick Newbigging, m.d.—*Edin.* 1842, Small 8vo, pp. 398.

The several articles which we have already devoted to auscultation and percussion have detailed so fully the principal improvements and additions which have been made, up to a late period, that in recurring to these subjects at present, we have but to make a few gleanings from such of the works and essays as have fallen in our way since the date of our last article, containing anything that claims particular notice.

The treatise of Dr. Skoda makes more pretension to novelty in auscultation than we expected to meet with so soon, in a field so long explored by the active imaginations and diligent ears of the multitude who have been on the alert for every sound and fancy that might chance to occur. We wish we could compliment the author on the extent and accuracy of his acquaintance with acoustics, and on the significance of some of his new facts, as much as we feel inclined to do on the general faithfulness of his account of what has been determined by the labours of those who have preceded him. In fact it is just because this faithfulness proves him to have a good practical acquaintance with the subject, and is likely to be a passport to his book to the good opinion of the public, that we think it worth while to devote some of our space to a consideration of his errors.

The chief peculiarity of Dr. Skoda's work lies in his explanation of some of the most important phenomena observed in the voice and respiratory sounds. He has few reasons to question the received opinions of the manner in which sounds created in the larynx are made more audible than in health on that part of the surface of the chest which lies over a condensed portion of the pulmonary parenchyma, as in the case of tubercular deposit and pneumonia. He thinks that the well-known facts (1) that sound is propagated the farthest in the same medium in which it has been produced, and (2) that it is weakened by conveyance through a denser medium, in proportion to the difference of density between the media, overthrow the view which is generally entertained, viz. that bronchophony, as a sign of condensation of the pulmonary parenchyma, is a result of the better conducting power of the pulmonary parenchyma when converted into a solid; for, he argues, as the voice reaches the parenchyma through air, to wit, in the bronchial tubes, it must be conducted better to the surface of the chest by the air of the small bronchi and air-cells, than it can be by a solid interposed substance, which, according to the principles of the received law, takes up and transmits the sound in a weakened state, from the rarer medium in which it was produced. Dr. Skoda, therefore, propounds a new method of explaining the occurrence of bronchophony, and indeed of all the vocal and respiratory sounds which occur in the chest, whether in health or disease. He conceives that they depend on what is known in music by the name of *Consonance*, and is familiarly illustrated by the experiment of sounding a note on any in-

strument in the neighbourhood of a tuned guitar, when the string of the latter, which is capable of yielding a tone that corresponds with the note in question, emits a musical sound. This phenomenon he applies in the following way:

"The air contained in the trachea and bronchi can so far consonate with the voice as the surrounding walls have the necessary capability to reflect the sound; some, to wit the walls of the larynx, the mouth, and the nasal cavities, have such, or an analogous property. In the bronchi, whose walls consist of cartilaginous rings, the voice consonates almost as strongly as it sounds in the larynx. In a degree little less must consonance take place in the two branches into which the trachea divides. At their entrance into the parenchyma of the lungs, the bronchi, as is well known, have no more cartilaginous rings lying close to one another, but the cartilages form irregular thin plates which lie in a filamentous tissue. The plates, as the bronchi subdivide more and more, become smaller, thinner, and rarer. The fine bronchi exhibit merely thin membranous canals. Within the bronchi which run in the normal parenchyma of the lungs, the voice accordingly consonates more feebly than in the trachea, and indeed so much the weaker the more the cartilages disappear. The conditions under which the voice can consonate more strongly in the air within the bronchi in the parenchyma of the lung are—that their walls consist of cartilages, or, in case they are membranous, that they are thick, or that the surrounding tissue of the lung is devoid of air; in all these cases the walls reflect the sound more strongly than the membranous walls of a normal bronchus; it is also necessary that the air within them be in communication with the air in the larynx. When the air in a confined space is put into vibrations, primitive or derived, usually the containing walls are also put into the like vibrations, and so much the more easily, the less stiff and hard they are. . . . . The larynx vibrates at each sound, and its vibrations even allow themselves to be felt through a layer of flesh several inches thick. The walls of the bronchi running within the parenchyma of the lungs, when the air contained in them consonates with the voice, are also thrown into vibrations as well as the larynx, and these vibrations can spread through layers of flesh several inches thick, or layers of fluid, even to the walls of the chest, and we can perceive in the walls of the chest, the sound consonating in the bronchi." (p. 35.)

Then follows an account of the circumstances in which the membranous bronchi in the parenchyma of the lung may be made to reflect the sounds, transmitted from the larynx, better than in their healthy state, and thereby to produce a louder consonance. "The reflection of the sound, and the strength of the consonance are so much the greater, the firmer the parenchyma becomes." Hence the increased resonance of the voice in tubercle, pneumonia, hepatization, &c.

It may be well, before delivering our opinion of the doctrine developed in the passages we have quoted, to illustrate Dr. Skoda's capacity to give a disquisition on acoustics, by a few examples of a popular kind, with which his book furnishes us. We do this the more willingly because young and enthusiastic students or practitioners are apt to found much of their faith in novelties, on the appearance of depth and learning which their instructor exhibits in matters of which they may happen to be ignorant. In explaining his views of the general nature and laws of consonance, Dr. Skoda takes occasion to remark:

"A tuning-fork sounds much weaker when held in the air than when placed on a table or box. The table must therefore strengthen the sound, and must also make the same vibrations as the tuning-fork; it must *consonate.* The sound of a Jew's-harp is scarcely perceptible in the air, it appears much stronger when the instrument is set in motion within the mouth; the air in the cavity of

the mouth must therefore increase the sound of the Jew's harp; it must *consonate* with it. .... The air contained in the sounding box of a guitar, violin, piano, &c. consonates with the tones of the strings, whilst the free air does not increase their sound." (pp. 32-3.)

It has been well said of the Hindoos that their religion is so incorporated with their philosophy, that if the latter were exploded the former would share its fate. We augur the same of Dr. Skoda's new *auscultology;* not on the ground that he has been unhappy in his illustrations, but because they prove that he does not understand what consonance really is. The air within a guitar or violin has nothing to do, directly, with the increase of the sound of the strings, which primarily and mainly depends on the vibrations of the *wood;* and just as little has the air in the mouth to do with the augmented sound of the Jew's harp, which, as in the former case, depends chiefly and essentially on the vibrations of the bony walls of the mouth. As this little instrument affords a good illustration of the true principle which operates in the several examples, we may observe of it, what Dr. Skoda may easily put in practice, that the increased sound is produced only when its prongs are in contact with the teeth, and thereby communicate the vibrations of its tongue to the bones of the jaws. The pulses from the vibrating walls of the mouth, guitar, and violin being communicated to a larger body of air than that which is in contact with the steel spring, or the strings, is one main reason of the increased sound, and we are surprised that Dr. Skoda should have missed this fact, with the example of the tuning-fork and table, without any confined air, staring him in the face. The confinement of the air does indeed contribute most notably to the increase of the sound; but simply because it is instrumental in carrying from side to side the pulses of the vibrating walls, and communicating them in endless multitude and succession from one part to the other, and thereby increasing the strength of their vibrations, and thus also of its own. Now in all these examples no one but Dr. Skoda would ever dream of recognizing anything but the mere *transmission* of vibrations from one substance to another. He may choose to call it *consonance*, yet it is not the consonance hitherto known by that name; if it were, consonance would be but another name for simple transmission of sound. The wood of a guitar will give sonorous vibrations in unison with any tone of any of the strings; but the note A, B, or C of one octave will *consonate* with only the note A, B, or C of another. This new theory of the phenomena of auscultation, therefore, is founded on a misconception of the laws and phenomena of acoustics, and the *consonance* of Dr. Skoda is but the ordinary *communication of sound* of other authors.

It is clear that if bronchophony in a piece of diseased pulmonary parenchyma be produced by the same means as the full sound of a guitar-string, it must be mainly owing to vibrations in the walls of a bronchus, which correspond to the wood of the guitar; but this is at variance with Dr. Skoda's theory and with fact. Again, if it be produced, independently of vibrations of the bronchi, in the column of air which is continuous with the air in the larynx, where the sound is created, it cannot be from consonance, else one end of a string, piece of wood, &c. may be said to consonate with the other, and there can be no such thing as simple transmission of sound. All that he has taken the trouble to say about the

loss of strength in sounds transmitted from one medium to another, owing to the reflection that takes place at their surfaces of contact, and which he illustrates by referring to sounds produced in water and in wood, which are well known to lose a great deal in the transmission, has little to do with the circumstances in which sounds are propagated through the chest. These sounds are propagated in confined air, the vibrations of which are constantly striking and restriking against the containing walls, and are not driven off into free space, as a sound produced in the open air is from the surface of water or wood. That solids surrounding confined air in a state of sonorous vibration take on the vibration in much force is well exemplified by what occurs in the nasal cavities. Let Dr. Skoda put his hand on the top of his head, and direct the nasal sound of the letter *n* into these cavities, and he will become sensible of a force of vibration that would penetrate a Bœotian skull. It is precisely on the same principle that bronchophony is sensible to the ear over a piece of hepatized lung. The many reflections and multiplied vibrations of the contained air set the bronchial walls into vibratory motion, and then the more solid the tissue on the outside of them the better will the sound be conducted. A worse conducting medium than the healthy parenchyma could scarcely be invented. It is true that the uninterruptedness of the column of air from the larynx to the peripheral surface of the lungs must conduce to the facility and force with which sounds created in the larynx are transmitted to the walls of the chest; yet but a small portion of that sound is so conducted, since the vibrations conveyed by all the tubes, with the exception of the minute twigs, which terminate in the lobules that compose the exterior surface of the lung, must be damped and interrupted in their progress to the surface by the innumerable alternations of membrane and air in the deeper lobules. The continuous consolidation of such a tissue by a substance which would render the medium of the transmission of sound from deep-seated bronchi of some size, to the surface, of a greater and at the same time more uniform density, cannot but contribute to augment the strength of the sounds heard at the surface, and we are constrained to abide by the opinion of Laennec, and stethoscopists in general, that such is the true cause of bronchophony and bronchial respiration. As to the objection of Dr. Skoda to the better-conducting power of solids, implied in his reference to the general preference of hollow stethoscopes, we have to observe only that solid ones certainly answer very well; and if there really be a good reason for the preference, which is doubtful, we think this may be explained on other principles than those of Dr. Skoda.

Among the valuable practical observations of Dr. Skoda on the phenomena of the voice in hepatization, and the same applies to tuberculation, of the lung, one which has been very generally overlooked, even by writers on auscultation, is the effect of mucus in the bronchial tubes in suspending bronchophony. No one can have studied the signs of condensation very often without having been perplexed by the occasional absence of bronchophony and bronchial respiration, while the other signs and the general symptoms indicated advanced pneumonia or phthisis. Coughing and expectoration usually restore the suspended sounds, and betray the cause of their temporary suspension. In consumption, expectoration is not always so complete as to clear the tubes sufficiently, and

then in not a few instances, in which impaired percussion-sound and small mucous rattles are the only direct proofs of the condensed state of the parenchyma. We have noticed also this presence of mucus in the bronchial tubes of tubercular condensation prevent the communication of the vocal thrill to the surface of the chest, over the condensed part, or greatly to lessen it. The tendency of condensation is to increase the power of the parenchyma to conduct the voice to the surface, and the vocal thrill, perceptible by the fingers, consists of the identical vibrations which constitute the sound. The force of the voice itself cannot be much augmented by the condensation without the usual thrill being also increased; and we conceive the difference of opinion which has existed respecting the state of the thrill in condensation to be owing to inattention to the contents of the bronchi. We have known the thrill on the side on which tubercular condensation affected the upper part of the lung to have been so strong as to be felt with inconsiderable force on the extreme point of the shoulder.

We cannot follow Dr. Skoda through the details of his work; but before quitting that part of it which is devoted to diseases of the respiratory organs, we must protest against the sentiment that liquid effusion in the pleura is usually a cause of bronchophony, and that several pints of it frequently coexist with that phenomenon. MM. Barth and Roger express themselves on this point much more accurately than Dr. Skoda, whose observations are too general by far:

"Some authors," say they, "consider the existence of bronchophony in pleurisy as very common, and explain it by the compression of the pulmonary parenchyma and the smaller branches of the bronchi, whereby the vocal vibrations are concentrated in the large bronchi. For ourselves, however, we cannot admit that proposition without restrictions. We do not deny the existence of *bronchial resonance* in pleuritic effusions, but we hold that it is distinguished by peculiar characters from true bronchophony, such, for instance, as is observed in pneumonia.......... Bronchophony does not properly belong to pleuritic effusion: this latter state is more clearly characterized by another vocal resonance, more remarkable for its tone than for its loudness, viz. ægophony." (Newbigging's Translation, pp. 125-6.)

These authors attach, perhaps, too much importance to this last sign as a result of pleuritic effusion, though they admit that it may depend on effusion of coagulable lymph alone, yet we conceive them to be much nearer the truth than Dr. Skoda, when he says that he has noticed ægophony as often without fluid in the pleura as with it, in pneumonia also, and in tubercular infiltration of the lungs, with and without excavation (p. 58); and when he states that three or four ounces of fluid cannot of themselves give rise to ægophony, and that when it is not present naturally, as he maintains it to be in certain children and females, it cannot be heard when fluid is in the pleura, except when the quantity of fluid is so great, that a piece of lung, containing a considerable bronchus, is compressed and destitute of air, (p. 62.) In short, Dr. Skoda makes it merely an accidental variety of bronchophony which may be audible in any of the diseases which can produce the conditions necessary for his consonance. We believe that there are more circumstances than fluid or coagulable lymph in the pleura, capable of giving rise to ægophony; but we doubt much the accuracy of Dr. Skoda's statements, to the sweeping extent we have noticed, in respect to true

ægophony. We have heard something like it produced by the acute female voice in a healthy state of the lung, and something like it, also, in œdema of the lungs, but certainly never the true tremulous bleating sounds, where there was much fluid, or in pneumonia. A strong objection to Dr. Skoda's view of its production by the compression of a part of the lung containing a considerable bronchus, by a large effusion, lies in the fact that when ægophony is audible after a good deal of fluid has collected, it is not at the point where the compression is nearly the greatest but at the upper margin of the sphere of impaired percussion-sound, where the stratum of fluid is thin, and the compression consequently slight.

We pass on to Dr. Skoda's account of auscultation of diseases of the heart, of which we have a few words to say. The chief novelties which Dr. Skoda announces in respect to the phenomena of diseases of the heart, relate to the intensity of the second sound, unchanged into a murmur, as heard on parts of the chest corresponding to the aorta, and to the pulmonary artery. It had been observed by former writers, and is particularly stated by Dr. Hope, that in certain circumstances the second sounds, in the aorta and in the pulmonary artery, underwent an increase. He mentions this in general terms, as the result of hypertrophy of the ventricles. Dr. Skoda is more particular, and affirms that the augmentations of the second sound in the pulmonary artery can be distinguished from that in the aorta, by being heard best to the left of the sternum, while the latter is audible with most intensity on the bone, and even to the right side of it. We are satisfied of the accuracy of these statements, which we have often tested when such diseases were present. in the one or in the other ventricle, as were calculated to produce a difference in the strength of the second sounds of the two vessels. The level of the third cartilage to the left of the sternum, or of the interval between the second and third cartilages, is the proper point for ascertaining the intensity of the second sound in the pulmonary artery; and the left side of the sternum, or its middle on the same level, or even further at the bone, is the proper place for studying the intensity of the second sound in the aorta. The more the arteries are distended by the force of waves of blood propelled suddenly into them, the more forcibly do they react, and the quicker and greater is the tension of the semilunar valves. So far, that is, to the extent of determining the degree of power with which the ventricles, or either of them, contract, the intensity of the second sound is a valuable criterion, *provided* there be (by percussion of the region of the heart and otherwise) evidence that the bulk of the heart is increased. This limitation is essential to an accurate diagnosis, for, even when the left ventricle is of its natural size, an increase in the strength of the second sound, of the aorta often, if not always, occurs as a consequence of widening of the ascending part of the vessel, an occurrence overlooked by Dr. Skoda.

An increase in the strength of the second sound in the pulmonary artery is dwelt upon by the author as of great consequence in the diagnosis, not merely of hypertrophy of the right ventricle, but of the origin of valvular murmurs. His fundamental doctrine on this particular is, that incompetency of the left auriculo-ventricular opening allows of so much regurgitation into the auricle, and therefore causes so great a hin-

derance to the transmission of blood through the pulmonary veins and pulmonary artery, that a distended condition of the latter is maintained and a more forcible reaction of the blood within it produced, in consequence, against the valves, when the systole of the ventricle is over. Whether this is the true explanation of the increase of sound, and not rather that it is owing to the hypertrophy of the right ventricle which so constantly attends incompetency of the left auriculo-ventricular opening, we shall not pause to enquire, but proceed to examine the significance of the sign in pointing out valvular disease. When, says Dr. Skoda, there is a murmur in the left ventricle during the systole, it may be concluded to depend on incompetency of the mitral valve, if the second sound in the pulmonary artery be increased; and if it be not, then the murmur signifies roughness of the aortic valves, or of the inner membrane of the ventricle, near the mouth of the aorta. (p. 184.) Now, if we admit with Dr. Graves (Dub. Journal, May, 1842), that a murmur created in the aorta may be heard chiefly in the region of the left ventricle, and allow also that this may coexist with hypertrophy of the right ventricle, a circumstance which may certainly occur, of what value does the increase of the second sound in the pulmonary artery become? It then will depend simply on the state of the right ventricle, and would undoubtedly mislead if Dr. Skoda's rule were trusted to. The increase of the second sound in the pulmonary artery cannot therefore be considered as more than a curious phenomenon, certainly not as a trustworthy guide to the diagnosis of the diseased orifice from which a murmur proceeds.

At p. 185, Dr. Skoda observes that when there is neither any natural sound (ton) nor murmur audible during the contraction of the left ventricle, it is of especial consequence to take notice of the second sound in the pulmonary artery, and that a natural sound or murmur occurs during the diastole of the ventricle, for this end, that if the second sound in the pulmonary artery be not increased, and the natural second sound in the left ventricle be audible, it may be concluded, according to his experience, that the mitral valve is competent; and, on the contrary, if the second sound in the pulmonary artery be increased, and a murmur occur instead of the natural second sound in the left ventricle, that the mitral valve is not competent. To both of these propositions there are such objections as prove them to be no better than ingenious refinements, the offspring of closet-meditation. In respect to the first, an assertion of his own at p. 185 shows the weakness of the conclusion. He there states that sometimes the mitral valve is incompetent, while yet no murmur attends the regurgitation into the auricle to prove its incompetency, and we have already seen that mere hypertrophy of the right ventricle may produce an increase in the second sound of the pulmonary artery. There will, then, be no murmur anywhere; the second sound may be present in the left ventricle, and increased in the pulmonary artery, yet incompetency of the mitral valve may exist. If the second proposition be not a misstatement, owing to a typographical error, it must be laid, with the other, to the account of the author, as a semeiological mistake. A murmur instead of the second sound in the left ventricle, if it ever proceed from the auriculo-ventricular opening, cannot be caused by *incompetency* of the mitral valve, though possibly it may by narrowing or roughening of the orifice, which is not necessarily connected with insufficiency of the

valve. The increase of the second sound, in this proposition, may depend, as in the other, on hypertrophy of the right ventricle.

There is much more of this pseudo-generalization respecting the sounds of the heart; but we suspect that by this time our readers are satisfied with our notice of Dr. Skoda.

We gave a brief notice of the work of MM. Barth and Roger in our last Number. It is, as we then stated, a sensible and accurate monograph on the subjects of which it treats. Dr. Newbigging's translation, as we then stated also, is remarkable for its faithfulness, and cannot but prove acceptable and highly useful to the English student. Indeed, the faithfulness of the translation is its only fault; and a very intelligible one it is, in the case of an over-anxious and modest translator, who has more respect for his original than confidence in himself. In his next edition, Dr. Newbigging will doubtless be less timid, and translate more freely, as his knowledge of the subject entitles him to do. He might add much to the value of the work by notes on the diagnosis of aneurism more especially, for which he will find ample materials in the journals, and by a chapter on percussion.

Among the more recent contributions to our journals on the subject of auscultation, we may name that of Dr. Bennett, in the Edinb. Monthly Journal, for February, 1842, as one of most pretension, and, to say truth, also one of the best. The readers of Piorry, however, will find in it much less of novelty than its general tone would lead to infer. The most important novelty in it is the account of a new hammer by Dr. Winterich of Wurtzburg, for the practice of percussion. In common with most of our brethren in this country, we have been accustomed for many years to employ the fingers of the right hand as a percussor, and one of the fingers of the left as a pleximeter, and we still think this method sufficient in all ordinary cases, as it is decidedly the most convenient to the physician and least formidable to the patient. We have, however, often used other pleximeters of ivory, wood, leather, India-rubber, &c., and we think them preferable to the finger in certain cases and under certain circumstances. We have also used, occasionally, one or other of the numerous hammers that have been proposed, by different auscultators, from time to time. But these last we have always shortly relinquished, from conviction that they were not merely unnecessary but disadvantageous. To say nothing of the trouble of carrying a large additional instrument, and the effect of so formidable an apparatus on the mind of timid patients,—minor evils we admit, and which of course must give way to great and positive advantages, if such are found to attend its use,—we have been accustomed to regard the loss of the tactual impression conveyed to the percussing finger involved in the use of the hammer, as a fatal objection to its employment. We must confess, however, that our experience of Dr. Burne's, or any other of the hammers heretofore in use, has not been, for the reasons mentioned, very great, and that our employment of Dr. Winterich's is still only of very recent date; we therefore do not feel ourselves justified either in condemning the use of this mode of percussion generally, or in denying to the hammer of Dr. Winterich the great superiority which Dr. Bennett claims for it. We shall not fail to give this last a fair trial; and in the meantime we recommend our readers to consult Dr. Bennett's account of it and of its

great advantages. It is more especially recommended to the clinical physician who has to enable others to hear as well as himself.

By the way, Dr. Bennett is unhappy in one of his designations for the characters of the sounds yielded by percussion; we allude to the word *humoral*. If he intend this to correspond to the *bruit humorique* of Piorry he has mistaken the nature of the latter, which is better indicated by the synonyme *hydro-pneumatique*, occurring as it does only when air and liquid are in contact. If, on the other hand, he intend it to signify a sound peculiar to liquid alone, we suspect that it is no improvement on the general term dulness, the dull sound of accumulated fluid possessing no difference from a thoroughly-dull sound depending on any other cause.

The student must not be scared by the apparent intricacy of mediate percussion as detailed by Dr. Bennett, for we can assure him that not a few of the directions are over-nice, and some of an impracticable exactness. Of the latter sort is the following reference to two or three ounces of liquid effusion in the cavity of the pleura: "It is readily distinguished," he says, "posteriorly, from the dulness of the liver on the right side," independently, as appears from the context, of change in the posture of the patient, and owing to the supposed humoral sound and diminished sense of resistance. Abundant observation convinces us that so near the liver there is not such a diminished sense of resistance on percussion as can be of value in forming the diagnosis.

Art. XVI.

Observations on Tuberculous Consumption; containing new views on the Nature, Pathology, and Cure of that Disease, being an attempt to found its Treatment on Rational Principles, deduced from Physiology and confirmed by extensive application. Illustrated by coloured drawings. By J. S. Campbell, m.d., Senior Physician to the St. Marylebone General Dispensary, &c.—*London*, 1841. 8vo, pp. 404.

On our first rapid survey of the pages of Dr. Campbell's work, our eye fell upon a passage, which might very fairly have been placed in the title-page as the epigraph of the volume, so thoroughly does its spirit represent that pervading the matter with which it is associated. "He who founds his conviction in medical investigation," says Dr. Campbell, "on physical evidence alone, must be content to remain a sceptic on more questions than the present." Considered simply and abstractedly, nothing can be more true than this; no proposition could be pointed out which we should be less disposed to contest: the *animus* in which it is to be understood, however, makes all the difference in the world; and, like many other noted epigraphs, its real signification, had the author adopted it for his motto, must have been sought in the chapters that followed. Having now perused and closely examined the volume, we are enabled to interpret, as follows, the phrase we have quoted: "It is essential that an explanation be found for every phenomenon, and a theory framed to promote the understanding of every class of facts which are presumed to have been observed or to exist, (whether they actually have been so observed, or do really so exist, is a matter of very second-rate

importance ;) if 'physical evidence' of a direct kind be obtainable in support of such explanation or such theory, well and good, let it be given ; but if not, let the investigator not be disheartened, experiments can be made, and facts can be tortured, and speculations can be dressed in plausible gear, and where is the theory which will refuse to stand when propped up, with the help of a little ingenuity, by such solid and imposing stays as these ?" Such is the faith which guides Dr. Campbell through his "new views on the nature, pathology, and cure [!]* of tuberculous consumption ;" a faith which in our minds, and in the minds of all those who with us believe that in pure observation and pure induction lies the true, the only means of advancing medicine,—is among the clearest and least endurable heresies. To dilate upon the comparative merits of *à priori* and *à posteriori* reasoning, however, is as much beside our present purpose, as the undertaking itself would argue instability of conviction upon, perhaps, the most perfectly established doctrine in the whole domain of philosophy; and we shall therefore, instead of indulging in platitudes on the subject, pass at once to detailed examination of the work of which we have now conscientiously, and as we believe correctly prefigured the prevailing spirit.

The author sets out with an introduction upon the "connexion between blood and life." Here we find the importance of arterialized blood for the maintenance of life dwelt upon, if not with much novelty of view, at least with much vigour of purpose. Originality may, however, be claimed within certain limits for the following notion : "One fact," says the author, "which seems to strengthen the idea that the red blood really carries from the lungs to the system, a 'something' which is the *cause* [the italics are Dr. Campbell's,] of life, is established by the ligature of a large artery. If the arterial current be arrested in its course by the application of two ligatures, the portion of blood between them speedily looses its redness, and becomes black. Are we to suppose that the life-giving ingredient has evaporated by the coats of the vessel when prevented executing the duty it is destined to serve?" The cause of life is thus made an actual and material entity; elsewhere in these pages, the inclination seems to be to regard it as a condition or effect, "evolved by the reciprocal action of the fluids and solids." With speculations of this stamp we are not disposed to delay, and refer those, for whom they may possess interest, to the original volume. We pass on to certain passages propounding doctrines, as avowedly deduced from previous statements, which are declared to have close connexion with the practical tenets of the book.

"It would appear then," says Dr. Campbell, "from all which has been said, that the varying velocities of the heart and lungs, and the varying celerity with which the functions of these organs are augmented in different individuals, are attributable to one of two causes, each dependant on the relations in which the lung stands to the system as regards either extent of surface, or capacity of action. In one case the pulse and respirations are always relatively quick, and easily accelerated, because the systemic vessels, either from inherent excitability to natural action from slight causes, or from morbid increments of action, as in fever, inflammation, &c., consume in a stated time a relatively larger quantity of red blood than the lungs, without undue action, can create, or the heart, without undue action, distribute. In the other case these functions are unusu-

* Upon Dr. Campbell's meaning of the word "cure" more hereafter.

ally readily accelerated, because, without the existence of any undue extension of systemic action, the pulmonic surfaces, whether from limitation of surface consequent on disease, or from presenting a natural structural disproportion to the body, are incapable of meeting, without inordinate increase of their functions, such quantity of red blood as the system consumes. Finally, we consider that there may exist a third cause of morbid acceleration of the functions alluded to, having its seat partly in altered organization of the lung, and partly in a changed condition of the arterial blood consequent on this, by which that fluid is so deteriorated in its vital properties, that a larger quantity is demanded to sustain a stated quantity of action, than when it is presented to the aortic capillaries in a perfect state." (p. 73.)

The author proceeds to "a short notice of *one* of the causes which appear to influence the transmission of the red blood." This cause he believes to be "a moving force resident at the circumference of the arterial tree," a doctrine which, as he observes, has been taught more or less distinctly by Bordeu, Bichat, C. Bell, and Hunter. Their doctrine of the independence of the capillary system as an agent in the accomplishment of the circulation, if not novel in itself, is however supported by novel argument. We ascertain by the microscope, the author alleges, that in the "final vessels" of the body, "the transmission of blood is accompanied by an alternate contraction and relaxation of the vessels along which it flows." In what manner of animal Dr. Campbell may have made this observation, we are puzzled to divine; we recommend to his consideration page 231 of the first volume of the translation of Müller's Physiology, wherein he will discover that the experience of persons somewhat versed in the use of the microscope, is rather at variance with his announcement. The fact is, that wherever arguments are to be found in favour of intrinsic circulatory force in the capillary tubes, they are not to be found in the *microscopical* examination of their conditions of being. "The capillary vessels," observes Müller, "present not the slightest change in their diameter, when the animal under observation is tranquil." Assuming, however, the fact as unopen to objection, Dr. Campbell proceeds, in accordance with the spirit of the epigraph we would recommend for his second edition, to lay down the cause of the said alternate contraction and relaxation. This is "the occurrence, at each motion of each minute vessel, of an entire or partial vacuum within its cylinder. And," continues this ready speculator, "when we come to consider that even a small artery divides into *many thousand branches*, it is easily conceivable how the aggregate influence exerted by those should largely contribute, on physical principles alone, to the progress of the red blood, however small the power exerted by any one may be."

The reality of this vacuum (would the reader believe it?) is forthwith assumed as established, and its cause in turn sought for. Hence the author's difficulty is one of selection; he suffers from a positive *embarras de choix;* for two efficient explanations present themselves at once: one of these, however, he inclines to adopt on the force of an analogy borrowed from the condition of the larger arteries. Those anxious to learn what the chosen explanation is, may gratify their curiosity by a reference to the 79th page of the work. We confess these *nugæ physiologicæ* (for such do the gravest questions appertaining to animal function become when handled in speculative mood alone) are to us not only without attraction, but positively repulsive. The dignity of science is offended by

their promulgation as established positions, and the advancement of real knowledge retarded in the precise ratio of the hold they take of general opinion. This is said quite irrespectively of the correctness or fallacy of the inferences to which they lead, either in the present or in any other instances; these inferences, of course, may or may not be in conformity with the reality of things, just as *chance* wills they shall be.

So much for the introductory matter of Dr. Campbell's volume. In the early pages of the pathological division, we find him stating the signification in which he understands the term phthisis. He limits its application to the disease marked by the deposition of tuberculous matter in the lungs, according herein with modern usage, and at once proceeds to an examination of the three opinions which have been held respecting the origin of tubercle.

We have some doubts of the utility of resuscitating the long-buried question of the " inflammatory origin of tubercle;" but whatever be the view taken of the propriety of arguing the point, there can scarcely, we think, be any difference of opinion as to the radical imperfection of Dr. Campbell's mode of conducting the investigation. He manages to omit the only evidence affording irresistible proof that inflammation is not a necessary condition in the evolution of tubercle, namely, the clinical results obtained and promulgated by M. Louis: results made so accessible to all by the periodical journals and otherwise, that we should have fancied them familiar to very " babes and sucklings" in pathology. Here is, it must be allowed, a serious error of omission; and we are very much disposed to regard as one of commission the introduction of M. Cruveilhier's statement respecting his facts in the generation of tubercle by the injection of mercury into the bronchi of animals. What is the object of bringing once more into the light fallacies which the common sense of mankind had long consigned to obscurity? Had there been any shadow of doubt as to the fact of the imagined tubercle produced in the experiments of that pathologist being anything more than pus, had one conscientious opinion appeared in defence of the startling mistake he committed in taking common abscesses for agglomerations of tuberculous matter, then might Dr. Campbell's repetition of the oft-repeated experiment have been valuable. As it is, he has added nothing really new to what was already established. Yet there appears an affectation of importance in the announcement of these experiments by Dr. Campbell, as if the world had deemed them actually required for the settlement of debated points. And in the same spirit we find an entire page teaching, in a tone appearing to assume novelty for the idea, that judicious practice requires a strict attention to the distinction of tuberculous cases, accompanied or not with inflammatory action. Who is ignorant of this? There is not a British author who may claim any acquaintance with the subject of phthisis who has not inculcated the principle, nor, we venture to affirm, a properly-educated British practitioner who does not uniformly act upon it.

Dr. Campbell next enquires, Does tubercle originate in an error of the function of " perspiratory secretion?" That it does, is the opinion held by Andral. Our author refuses to admit this doctrine, on the ground that if so, tubercle of the skin should be extremely frequent, whereas its extraordinary rarity is matter of notoriety. The perspiratory

secretion, it is true, goes forward in all the cellular tissues of the body, but it is infinitely less active in these than in the skin: if, then, we admit tubercle to be a result of an error in the function of perspiration, we have the difficulty to contend with, that the effects of the error are most frequently observed in those situations in which the healthy function is least exercised. Such is Dr. Campbell's argument. But why not suppose that the perspiratory function more readily becomes disordered in the cellular tissue than in the skin? Hypothesis is the order of the day, and this, which seems quite as good as its fellows, gets over the difficulty in a very satisfactory manner.

The third opinion as to the origin of tubercle, an opinion which has been generally adopted, and which received the assent of Laennec, is that it is the result of some error in the process of nutrition. Dr. Campbell assumes the advocacy of this doctrine, but is not content with the simple statement of arguments calculated to support it; he justly observes that Laennec, no less than all others, has left us entirely uninformed, not only as to how this error is remotely produced, but also as to the immediate mode in which it determines the deposit of the foreign matter. Of these points he proposes to attempt the elucidation. In this attempt lie the novelty and the main claim to attention of Dr. Campbell's book, and we shall here, therefore, accompany the author closely.

In accordance with the arrangement adopted by Dr. Campbell, the first point for enquiry is the "nature of that action originating apparently in the system, and directed towards the lung, of which the deposit of tubercles in the latter organ is the result." Two important points are necessarily involved in such an enquiry. First, "the nature and character of the primary constitutional disturbance which tends to the production of the local affection; and secondly, the mode in which this actually leads to its establishment." The first involves the consideration of the phthisical diathesis, the tuberculous cachexia of Sir James Clark; the second may with equal propriety, observes the author, embrace a consideration of the physical characters of tubercles, the probable mode of their formation, and the tissue which they occupy. Dr. Campbell begins with the last-mentioned subject, by an examination of the question of the identity of pulmonary granulations and crude yellow tubercle. Having stated the arguments adduced by Laennec, and some of those of Louis, in favour of that identity, the author enumerates the counter-arguments of Chomel and Andral. Appearing to consider these latter satisfactory, he yet "desires to add another, derived from the action of reagents in the first place, and the simple process of decomposition in the second, on lungs which present the two varieties of morbid growth." When slices of lung are placed in water for a length of time, he observes, and the bottle is frequently agitated, they ultimately form by the process of natural decomposition a dark slimy solution, with fibres or threads diffused through it—the coats of vessels or minute bronchial tubes. Now when the lung contains semi-transparent granules, these are found to decompose with as much facility as the natural tissue, while crude tubercles may by repeated maceration be separated from the pulmonary substance in the perfect state, as regards colour, shape, and consistence. This difference between the gray granulation and crude tubercle in re-

spect of proneness to putrefaction appears to Dr. Campbell to strengthen the arguments in favour of their non-identity, but he admits that it by no means proves it. It might be argued that the component material was in both cases the same, but in its early stage the granulation too soft to resist effectually an amount of maceration exercising no notable influence on it when fully consolidated. The action of alkalies points, likewise, to some difference in the pulmonary granulation and the crude tubercle: the former is simply softened and swelled, without any other change in its original appearance, while yellow tubercle forms a clear saponaceous solution. To our minds these facts, for such we presume they are, prove nothing more as to the fundamental question than does simple inspection of the two substances. That there is a difference apparent on the simplest examination is as obvious as the day, and it would be infinitely more extraordinary if the agents mentioned by Dr. Campbell acted in the same way on two substances of such strikingly different aspects. But this difference of aspect furnishes no shadow of proof that one is not an advanced stage of the other, and the chemical difference by the author's experiments fails as completely to offer such demonstration. The chemical composition of cartilage and bone differs, yet it will not be denied that the former constitutes the first stage of the latter; or, in other words, the necessary stroma for its development. This is all which is contended for by Laennec and M. Louis in respect of the relationship of the gray granulation and yellow tubercle; and Dr. Campbell's experiments modify in nowise, that we can perceive, the terms of the question. He starts, however, with the notion of his having proved satisfactorily that the gray semi-transparent granulation has no necessary connexion with pulmonary tubercle, but that this latter substance *first* shows itself in the lung as a yellowish friable matter, &c. To its precise characters we shall by and bye have to return. Meanwhile we pass with Dr. Campbell to what he rather obscurely and affectedly terms "the question of tubercular locality;" that is, the determination of the immediate seat of the deposition of tuberculous matter.

To the probability of the opinion of Dr. Carswell that tubercle occupies the air-cells, he raises the "serious objection" stated by Lombard, namely, that as those cavities communicate with the trachea, their contents would necessarily be occasionally detached and "extruded" in the early stage, "a thing well known never to take place until they have passed into a softened state." Dr. Campbell considers this objection strengthened by the fact, that the matter of tubercle adheres with much tenacity to the lung when first formed, a point in their anatomy easily ascertainable by the examination of a tuberculous organ. Notwithstanding these objections, our author still inclines to the conclusion that tubercle is "usually, if not always, associated with the true breathing portion of the lung:" the motives for this rather paradoxical inclination, as it is at least under the circumstances, are stated to be "the form of tubercles, and many other circumstances connected with their history." Then arises, it is admitted, the question, whether any means may be found "of reconciling this opinion with the obvious difficulties to which it is liable."

Andral many years since pointed out a fact in the anatomy of phthisis which may be readily verified. If a tuberculous lung be insufflated,

dried, and sliced into layers, certain air-cells with dilated cavities may be seen, having thickened walls, which present a peculiar yellow tinge; this tinge is deeper in some points than others, and by care a "number of minute yellow round bodies, which are evidently tubercles," may be distinguished in the thickened parietes. The yellow tinge Andral looks upon as a change "preceding the secretion of tubercles;" Dr. Campbell regards it as produced by bona-fide tuberculous matter. This matter he believes to be seated in the interior of the vessels ramifying in the walls of the vesicles; he could often perceive by the aid of a moderately strong glass, lines of yellowish vessels pervading the lining membrane of the air-cells. The yellow matter is with difficulty detached from the membrane; and Dr. Campbell's "conviction" is, that this depends on its being actually contained within the vessels.

Here is the prominent idea in this author's volume: he believes that tuberculous matter, instead of being effused from the blood-vessels into the inter-vascular spaces at its origin, is retained within them, and that, by the accumulation of this matter in several adjoining vessels, a mass is at length produced sufficiently large to attract the naked eye,—in other words, "a tubercle" is formed. We shall state, as plainly as we can, the substance of the evidence brought forward in support of this doctrine. The discovery, as alleged, of "yellowish vessels" has, we confess, little importance in our eyes: Dr. Campbell saw yellow *streaks*, and calls them *vessels*,—and this is all.

"When a minute tubercle is submitted to the microscope, it is readily perceived not to be the homogeneous nodule which it appears to the naked eye; it presents a round surface, made up of other round particles or molecules, which adhere together by a delicate filamentous membrane; occasionally these molecules appear to make lines, forming a chain of soft hair-like threads, with round swellings at certain distances, so interlaced with other lines of the same description as to render their isolation under the eye very imperfect, but still distinct enough to convey the idea of a body formed of innumerable molecules adhering to each other, not as a consequence of their own cohesion only, but through the intervention of a tissue which forms their seat." (p. 140.)

These observations are stated to be confirmatory of others made by Kuhn, the allusion to whose inquiries is followed up by the *naïf* remark, that "the idea does not seem to have occurred to this observer that the globules he describes might possibly be extremely minute particles of a white matter, contained within the caliber of small vessels, whose coats formed the delicate line connecting them. And yet, if he had endeavoured, from imagination only, to describe the probable appearance of the inner membrane of an air-vesicle, whose minute vessels were obstructed by an abnormal deposit, he could not have conveyed a clearer picture." Dr. Campbell, himself, so clearly states the inadequacy of any observations of the stamp of those just detailed, that he will not assert their power of proving his position; yet affirms that, coupled with the arguments before brought forward, they go far to support the probability of the opinion. Under these circumstances, it is unnecessary for us to show, what indeed must be obvious to the reader, that they are incapable alone of demonstrating the point at issue; of the "arguments before brought forward," we have already disposed.

It is known, observes Dr. Campbell, that a certain relation exists in the healthy state between the caliber of the capillary vessels and the bulk

of the blood-particles; that if substances capable of thickening the blood be injected into the veins, death ensues, and is very probably caused by an obstruction arising in the extreme vessels of the pulmonic circle. Here the obstructing cause is probably general; were it partial, it is inferred that a sufficient number of vessels would remain pervious to maintain the purposes of respiration. But the normal relation above referred to may, *conceivably*, be altered in two ways: that is, the vessels may be too small, or the particles may be too large. With these facts and conceptions to work upon, Dr. Campbell has instituted a series of experiments on injection of the lungs after death; "and if it can be shown by these that there exists a vast difference in the facility of transit of the same compound injection, though the capillaries of different lungs,—that this facility is least when the evidences of tubercle are most distinct, and much diminished where the evidence of *tendency* to tubercle, as derived from other sources, is considerable; then do I conceive that facts have been adduced corroborative, to a great extent, of the opinion, on other grounds maintained, that tubercle is the result of obstruction in the minute vessels of the pulmonary circulation." The injection employed was composed of mutton suet, with a fifth of olive oil, and some vermilion, and was found to pass with facility through the pulmonary artery, and through the veins into the left auricle, in some instances having undergone no change in the transit, in others having lost some of its colouring matter. The loss of colouring matter being only apparent in the injected material in the auricle and veins, Dr. Campbell ascribes its absence there to retention by the capillary vessels, whenever their caliber falls below the ordinary standard, either in the entire pulmonary system or in limited parts of this. There is one rather important consideration which Dr. Campbell seems to have very fortunately, at least for the existence of his theory, forgotten; and this is the simple fact, well known to practical anatomists, that nothing is more common than to observe the vessels of an injected limb deficiently supplied with or altogether free from, in certain points of their course, the colouring particles which have been used for the manufacture of the injected material. To infer that this sort of filtration depends upon a narrowness of capillaries, itself evidence of tuberculous constitution, would be at variance altogether with observed facts; the phenomena referred to having been witnessed in subjects of every variety of temperament, and enjoying every variety of health.

These matters, however, being established to the author's satisfaction, a new point for investigation presents itself; namely, "whether the relative facility with which the constituents of an injection are separated in different cases, is susceptible of being connected with appreciable peculiarities of constitution, in the individuals whose lungs form the subjects of examination." Dr. Campbell seems aware of the large number of observations which would be required for the settlement of the question here started, and he admits that what he has to say on the subject is more to be considered as proposing than determining it. He has subjected the lungs of thirty-eight individuals to the process of injection, some of them healthy, the greater number variously diseased. In examples where no perceptible disease of the lung existed, and where it is alleged "the individual showed no signs of a strumous constitu-

tion," the red injection entered by the pulmonary artery and returned through the corresponding veins, having minutely filled the extreme vessels. In a subject of strumous constitution, whose lungs were nevertheless, "as far as could be traced," free from tubercle, (a child aged four years,) the majority of the pulmonary veins were filled with the red matter, altogether unaltered, while in some instances they contained the white portion only. In a child aged five, who died of tuberculous softening of the brain, and whose lungs were besides studded with tubercle, the quantity of injection deprived of its colouring material was considerably greater. From these facts, as well as others which he does not think it necessary to refer to, Dr. Campbell concludes that "throughout the series a marked difference has appeared in the extent of separative influence exercised by the pulmonary vessels of different subjects; and that this has borne a strict relation either to the existing tubercular disease of the organ, or to the constitutional tendency to phthisis deducible from evidence less conclusive."

The variable degree of facility with which particles of a certain size traverse the capillary vessels of the lung being admitted, it remains to be enquired how this applies to the formation of tubercles, or throws any light on the nature of that condition which has been conceived to establish a tendency or predisposition to their evolution. This is simple: grant that the pulmonary capillaries of strumous subjects are of unusually small caliber, generally or partially; and admit that particles foreign to the healthy nature of the blood occasionally exist in that fluid, and nothing can be more clear than that the smallness of the capillary bore must constitute the so-called "predisposition" to pulmonary *tuberculization*, while the actual formation of *tubercles* will depend, even in a subject having blood impregnated with tuberculous matter, more immediately upon the degree of smallness of the capillary vessels, than upon the amount of that tuberculous impregnation.

Two objections, it is remarked in anticipation by Dr. Campbell, may be urged to this mechanical theory of the formation of tubercles out of tuberculous matter. The first is, that "the theory leaves it unexplained how such an abnormal condition of the pulmonic capillaries should occur at all; and still less how it should only be found in some portions of those which pervade the same organ." This objection we think of comparatively no importance; the great point is, whether the alleged abnormal condition exists at all; if it do, we need care little for the actual cause. But the second is an objection of more weight: "the very existence of the globules which we have supposed to be the determining cause of the disease is, so far as we have yet gone, assumed, but not proved." Dr. Campbell seeks to divest this objection of its force by allusions to the growing belief in the soundness of humoral pathology; to the notion vaguely expressed by Andral, that tubercle may be liquid at the moment of deposition, and rather more definitely affirmed by Cruveilhier; and to the recognition by Dr. Carswell of the blood as one of the localities in which tubercular matter is occasionally formed. We confess ourselves unable to detect any semblance of proof in these surmises of various writers; and how the fact that humoralism is gradually ousting its rival solidism from the field of pathology, advances *demonstratively* the views which Dr. Campbell undertakes to establish,

we are, sincerely speaking, unable to perceive. Still we have not exhausted the author's budget of reasons for teaching the origin of tubercle to be in the blood: "the strongest facts in its favour are undoubtedly those furnished by the microscope." The facts here referred to are, that, "under some conditions of disease actually formed, or in some states approaching to disease," particles are found in the blood different from those which belong to it in health. These have been called pus-globules, but their name Dr. Campbell thinks a bad one, because it identifies, or rather attempts to identify, the nature of a fluid of which the character is unknown. The important point in connexion with them he believes to be, that they have hitherto been found chiefly, if not solely, in the blood of persons who labour under some form of cachexia, and hence frequently when no decided symptoms of phthisis have been present; "but, on the other hand, where the tendency of this disease is strong or its presence unequivocally announced by decided signs, they will, *I am disposed to think*, be seldom absent; and they thus appear to constitute one of the elements demanded for its full and perfect formation." We cannot take the favorable view of the importance of this train of argument evidently taken by its author: beyond the announcement of a *remote possibility*, we can detect nothing in this exposition of his opinions.

Dr. Campbell continues his investigation, however, as if his path were smooth before him: having passed from the formation of tubercles to the accumulation of tuberculous matter in the capillaries, and from this to its primary and invariable existence in the blood, his ambition is to rise a step further, and determine the cause of such existence. This he finds in a "primary error of digestion which lies at a point in its series of actions anterior to that of final sanguification"—by the term digestion being understood the sum of changes undergone by the food from its first reception into the stomach to its final identification with blood by the respiratory function. This connexion of indigestion with the development of tubercles has, as is well known, been taught by others; Dr. Campbell gives no new evidence of the correctness of the doctrine, but, in commenting upon it, makes a statement respecting the history of phthisis, practically considered, which we cannot suffer to pass unnoticed. He speaks as of an every-day occurrence of meeting persons "exhibiting, perhaps, all the characters of the diathesis, but in whom the symptoms are still insufficient to justify more than a strong suspicion [of phthisis;] and the physical signs are *still less decided*." Now, we affirm, on the contrary, that cases where the physical signs are less decided of the existence of tubercles than the symptoms, are of most extraordinary rarity; nor can we conceive the assertion of Dr. Campbell to be the deliberate convictions of any man who has carefully availed himself, in every case submitted to his examination, of the numerous sources of information supplied by physical investigation in its existing state of advanced culture. But, as we shall hereafter find, Dr. Campbell's *forte* does not lie in his familiarity with the niceties of physical diagnosis.

But for the theory: let us consider its successive steps, and note, as we pass, the degrees of solidity of each. First, tuberculous matter stagnates in the capillary vessels of the pulmonary air-cells; of this, as we have before said, we can discover no proof but that Dr. Campbell and others have seen yellowish matter or specks in the walls of the air-

cells, and that Dr. Campbell considers it advisable to call these specks vessels. Secondly, tubercles appear to be formed of a number of these vessels containing tuberculous matter in the mode just adverted to; it is sufficient to say that the appearances, here referred to vessels crammed with tuberculous matter, have been by others—or at least by one other observer—described, on quite as good grounds, to be produced by the interlacement of the filaments of a species of conferva! Thirdly, the tuberculous matter stagnates in the pulmonary capillaries, because, in scrofulous subjects, they are of morbidly small caliber; the evidence of this being the result of certain injections with suet and vermilion performed on the lungs of healthy and diseased persons. The answer to this view we have already given. Fourthly, tuberculous matter exists from the first in the blood, and its presence there is a necessary condition of the formation of tubercles. The only proof attempted to be adduced of this most important notion is, that "pus-globules" have been found by certain manipulators with the microscope in the blood of cachectic subjects; and the proof amounts simply to this, that Dr. Campbell finds it convenient to consider certain particles—of which he himself affirms no one knows the real nature—the primary constituent elements of tubercles.

If this be not a fair exposition of the leading points of Dr. Campbell's theory, we regret it, and can at all events declare that our wish has been to represent his hypotheses as they appear under his sanction. If, on the contrary—as we believe is the fact—we have correctly described them, then can we only pleasurably anticipate the satisfaction of the solid pathologist, who may for the first time have met with them in these pages, as he discovers this new evidence, that flimsy though ingenious speculation cannot stand in lieu of sound observation and cautious induction.

In the chapter describing "the causes and character of the morbid reactions exerted by tuberculous lungs on the organs and functions generally," the same tendency to speculation encounters us in almost every page; this is the more to be regretted as there is here much matter of a practical kind worthy of attentive study. We cannot afford space for the consideration of the topics therein touched upon, and pass to the doctrines upon the treatment of the malady in its "formative stage," at the commencement of which we find the author discoursing upon the signification of the term *cure*.

"In its broadest sense, as usually employed, the word *cure* appears to express the *complete* restoration of a part or of the whole body, which had previously erred, either in function or structure, to a normal state; and this without its necessarily implying any connexion between appreciable cause and effect, inasmuch as such restoration is frequently induced without our being capable of explaining it. But again, the term is commonly and, as regards the medical art, more properly used to express a direct connexion between the means producing and the effect produced—to indicate not only the fact of restoration, but the agencies, be they medicinal or dietetic, on which the change is presumed to depend; and it is in this sense that I mean to employ it in the succeeding pages............... But whilst we thus define the term cure as in strictness implying the *entire* restoration of function and organization, there exists a more modified sense in which it may be and often is employed. Perfect integrity of organs or perfect integrity of action in these is found, it may safely be averred, in very few human beings after life has a little advanced, and it hence follows, that the term may be legitimately used to express, not indeed complete

restoration, but only such an approach to this as enables the individual, under certain limitations and modes of management, to carry on life without inconvenience even to a protracted period." (pp. 221-2.)

We deny that the term *can* be legitimately used in any such sense, and we resist its employment thus on logical, common-sense, and moral grounds. We resist it on logical grounds, because it is a palpaple absurdity to define by the same word two things which, as is distinctly admitted and as could not be glozed over, are essentially different. We resist it on common-sense grounds, because common sense and the experience of mankind concur in showing that, even understood in the latter of the two senses stated, a cure of phthisis (though for inexplicable reasons an occasional *chance* result,) cannot be regarded in the remotest degree as a consequence, predicable *à priori*, of any mode of treatment, however judicious and however sustained. And finally, we resist it upon moral grounds, because the blazonment of the phrase "cure of consumption" in the title-page of a book and the columns of a newspaper is—viewing the matter in the most favorable aspect possible for those who authorize both—a heartless cajolery of the myriad of sufferers from the disease; a mockery of those who, interpreting the word as their dictionary and common usage teach, straightway imagine that at length a panacea has been found for the malady that destroys them, and eventually sink more rapidly to the grave from the sudden prostration of the hopes they were deceived into cherishing.

The indications of a "cure" derivable from the views of Dr. Campbell respecting the malady are, he states, these:

"First, to counteract that morbid state of the digestive organs, without which tubercles cannot be produced. Secondly, to accomplish a solution of this matter after it has passed into the blood, and thus arrest its local deposition, presumed to depend on mechanical retention in the extreme vessels of the pulmonary artery. Thirdly, to place and retain the patient under such circumstances, in reference to his medical, dietetic, and general treatment, as seem on rational principles best calculated to meet those evils which result from the existing state of his respiratory organs; thus affording time for removal by the natural process of softening of such tubercles as already actually exist." (p. 225.)

These indications are clearly stated: in the second only of them is there any character of novelty, and to the details referring to it our observations shall be almost altogether confined. First, however, we must take the trouble of pointing out an error of no mean importance connected with the subject of diathesis—an error which, like most of those committed by Dr. Campbell, originates in his unfortunate proneness to submit the investigation of matters of fact to speculation, and not to observation. The phthisical diathesis is the subject; by which Dr. Campbell means the constitutional state of subjects destined to become consumptive; and *more solito* the outward aspect of these individuals is portrayed. Here we have the delicate and clear skin, the light hair, the elegant contour of the face, body, and limbs, the tumid upper lip, the large and lustrous eyes, the pearly sclerotic, with the sound and regular teeth, the "blush suffusing the cheek of beauty," &c., which it has been the fashion to describe in more or less glowing periods as the outward and visible signs of tuberculous constitutions. But though these pictures may be pretty, it does not exactly follow that they are correct;

our own experience (and we could give this numerically,) cries loudly against their accuracy; but we prefer referring to results already before the public. We read in the elaborate and, in many respects, admirable work of M. Fournet: "I have seen *very few* phthisical patients with light hair; in *almost all* of them the pilous system was well developed and brown. I have met with several whose hair was ebony black. The colour of the iris agreed very accurately with that of the hair. Hazel or brown eyes were most frequent among my patients." Simply, no doubt, because the hazel is the most common French eye. Again, "the occurrence in phthisis in subjects endowed with a strong, muscular constitution, having a well-developed and well-formed skeleton and sanguineous temperament is more frequent, on the evidence of my cases, than is commonly supposed. This class of subjects forms nearly *one third* of the total number of phthisical patients." Now is it probable—is it even remotely probable—that, taking the entire mass of individuals entering the Parisian hospitals, and afflicted with all varieties of disease, we should find more than one third possessed of muscular forms and the sanguineous temperament? On the contrary, it is certainly improbable, to say the least, that that third would be found; but we shall not on this account infer that the temperament in question forms *the* or even *a* predisposition to phthisis. No; all that the facts authorize us in affirming is, that subjects of every constitution in existence are liable to the inroads of phthisis to the same or nearly the same amount. The error which Dr. Campbell has thought fit to perpetuate in emphatic terms is one not merely important as a point of pathological doctrine, but one likely to entail practical mischief; had this been otherwise, we should not have considered it deserving of so lengthened a notice.

Dr. Campbell's second indication, we have seen, is to "accomplish a solution of tuberculous matter after it has passed into the blood;" no trifling undertaking, it would appear, on first thought. Our author was not, however, discouraged by the apparent difficulty of the task. He examined experimentally the degrees of "solvent power exerted towards the matter of tubercle by various substances." The pure alkalis were in these experiments, as has appeared in another part of this notice, discovered to be the most effective agents of solution. Many other circumstances seemed to Dr. Campbell to point out these chemical agents as deserving of a more persevering trial in the treatment of phthisis than they had yet obtained. Among these circumstances the author mentions their "universally-acknowledged efficacy in the analogous affection of external scrofula, and the general impression which prevailed in the humoral schools of pathology, that they constituted the most effectual of those substances which were comprised in the class of attenuating medicines." The alkalis have besides, as Dr. Campbell correctly states, been mentioned by almost every writer on the medicinal treatment of consumption as worthy of more or less confidence. Now, if we consider the grounds upon which the use of alkalis is here advocated, we shall not find, we apprehend, very strong motives for estimating the practice highly. The very notion that tuberculous matter exists in the blood—the fundamental notion or basis on which the superstructure is raised—is, at the best, a possibility, in reality a mere hypothesis. But, admitting that it does so exist, and admitting the equally-necessary postulate, that

tuberculous matter is readily dissolved out of the body by the fixed alkalis, does it follow, that when within the body and contained in the blood-vessels, the ingestion of alkalis into the stomach will similarly affect it? Not in the very remotest degree. But this is not all. Let us again admit that it will actually so disintegrate and dissolve tuberculous matter accumulated into small masses in the lungs: have we any reason to anticipate benefit from such disintegration and solution? What we should really do in this case would be to spread through the whole system in a semi-liquid state, and by means of the blood, what was previously aggregated in one or more limited spots. Would the diffusion of pus or of the matter of cancer through the frame of persons suffering from abscess or carcinoma—granting we had the power to effect it—be likely to improve their condition? These points appear to us to prove most clearly that, considered upon *à priori* grounds, the alkaline treatment possesses no shadow of a claim to the attention of solid men.

But Dr. Campbell's advocacy assumes a very different tone of importance when he goes on to inform us that a "very ample experience, extending through a period of nearly ten years, and embracing above 400 cases of well-marked phthisis, confirmed the soundness of the views which originally led to its adoption." Yet even here there is a palpable *non sequitur*. It does not in truth follow, supposing we admit that these 400 cases were all of them really cases of well-marked phthisis, that they were all treated with alkalis, and that every single consumptive individual was distinctly and materially benefited by their use—it does not follow, we say, even in the most distant degree, that Dr. Campbell's views, or any other views which might have led to the adoption of the alkaline treatment, were sound; it simply and alone follows, that phthisical subjects derive benefit from the use of alkaline medicines. And here is, in truth, the important and really interesting question; is improvement actually obtainable and capable of being sustained under the constitutional influence of these medicines? The reader will perceive that we speak not of "cure;" we put the question in the most favorable manner possible for Dr. Campbell; we ask simply, has *sure*, or even tolerably sure, and *continued* amelioration of consumptive subjects arisen under this treatment? not such amelioration as will, we all know, frequently follow, *pro tempore*, in the case of dispensary patients, an altered system of diet and the use of ipecacuana or any other simple medicine. Dr. C. refers us to some cases at the end of his volume in proof of even much more—of complete suspension, at the least, of the disease having followed the exhibition of alkalis; and to these cases we accordingly turn.

Dr. Campbell observes that, to render reported cases of any value, two things are required: honesty on the part of the reporter, and sufficient evidence that he has not been himself deceived. Of the first of these Dr. Campbell is, we are persuaded, possessed. As respects the latter, it is not sufficient that the *terms* of evidence should exist in the report of any case, the reader also requires (unless the fulness and manner of the description be such as to make this unnecessary, which is the rarest of merits in the works of authors) to know from other sources whether the writer be really capable of accurately appreciating the conditions which supply those terms. On carefully considering Dr. Campbell's efficiency in this point of view, on the grounds furnished for the formation of an opinion in some introductory remarks on diagnosis in the chapter con-

taining his reported cases, our inference is that his acquaintance with the physical signs of the disease is correct generally, so far as it goes, but far from minute;* but it will be perceived that, in one point of view, the want of either the power or the will to discriminate very minute changes in the physical state of the lungs would militate *against* Dr. Campbell's establishment of his doctrine; it would prevent him from detecting the very earliest deposition of foreign matter in the lung, the condition most likely to be benefited by the treatment lauded. That he has scarcely attempted such detection, is manifest from the simple fact of his considering percussion a more valuable instrument in the early discovery of tubercle than auscultation.

The cases related, sixteen in number, are arranged under these heads: under the first appear "ten examples of phthisis in its early stage, which appeared to be made out in a manner tolerably satisfactory." Hence the treatment is alleged to have "conduced to the quiescence of existing pulmonary tubercles, and arrested their tendency to increase." Our space will not admit of more than a very brief commentary upon some of these cases. *No.* 1. Here the existence of tubercles is not, by any means, positively established; the patient took the alkali for two months, and passed subsequently two months without any cough, to which she had previously been subject. Dr. Campbell saw her eighteen months after she had been treated, but, strange to say, neglected to examine the chest, or at least says nothing of his having done so. *Case* 2. Alkali continued for fifteen months; signs of consolidation in both lungs, which signs had diminished on one side, not on the other, at the end of the treatment. Seven months later, Dr. Campbell saw this woman, but tells us nothing of the physical state of her chest. *Case* 3. The medicine used for two months; signs of consolidation on both sides, unchanged at the end of that period, except that the "bronchial respiration had ceased to be harsh." The meaning of this does not appear very clear. Dr. Campbell saw this woman upwards of a year afterwards, but, as usual, leaves us in ignorance of the physical state of her chest. *Case* 4. "The upper lobes of both lungs, especially the right, were extensively solidified." In two months, this man "was dismissed *well*"! He had abdominal symptoms when first seen; upon the removal of these, we presume Dr. Campbell founds the use of the adjective *well*. That the case proves, as it is alleged, the effect of the alkali in preventing a threatened fresh deposition of tubercles, would probably never have struck any one but the author himself, at least upon the evidence he has given to the public. *Case* 5. This man appears to have been temporarily better under the alkali upon two occasions, when an aggravation of his ordinary symptoms occurred. *Case* 6. Alleged signs of solidification at both summits; in six weeks the patient was "very nearly free from cough and much increased in flesh." Alkalis with anodynes. *Case* 7. Alkali for twelve months; at the end of that time no change in the physical signs. Four years after, the state of chest not enquired into, though the patient was then seen. The remaining three cases are more or less closely similar to these: in all benefit, in respect of the local and general symptoms, appears to have been effected. But we see no particle of evidence to show that such benefit depended on anything but the disappearance of local

* Dr. Campbell appears to be wholly ignorant of the diagnostic value of the expiratory murmur.

congestion; nor do we feel disposed to admit that any practitioner, who has treated phthisis extensively, could not produce many cases of quite as satisfactory character—cases which had been submitted to all varieties of treatment. It will be observed that, so far as they are mentioned, the physical signs appear to have undergone none of that modification which, were Dr. Campbell's theory of solution correct, should have been manifested along with the amelioration in respect of symptoms.

Next follow three cases, "tending to show the possibility of tubercular absorption." These cases are worth study of an attentive kind; we, therefore, refer the reader to the original reports. Lastly, appear three "cases of phthisis, in its very advanced stage, benefited by the alkali."

On the whole, we are of opinion that sufficient evidence has been here brought forward to show that alleviation of symptoms, to a marked amount, will sometimes take place under the use of alkalis, and that these medicines are consequently deserving of the attention of persons engaged much in the treatment of consumptive subjects; but we, as before intimated, can discover no distinct proof of their *extraordinary superiority* as remedial agents in the management of this malady.

The three points upon which Dr. Campbell insists, regarding the mode of administration of the alkali, are, that it be administered in the *pure state* (the liquor potassæ of the Pharmacopœia); that no medicine, which might convert it into a salt, be prescribed simultaneously; and that its employment be persevered in for a long time. The dose of the medicine, as employed by Dr. Campbell, is from ʒjs. to ʒj. three or four times daily; and the ordinary vehicle is milk or water. If acidity is present, gr. x-xx. of the carbonate are added.

In various parts of the work, with which we now part, close practical familiarity with the management of consumptive disease is distinctly exhibited; and, estimating him by this volume, through which alone we know him, we should be inclined to regard very highly Dr. Campbell's ability as a therapeutical physician.

The literary execution of the work is extremely careless. Dr. Campbell's versions of authors' names are remarkably curious: we have *Loui* for Louis; *Cattereau* for Cottereau; *Crouvillier* for Cruveilhier; *Therard* for Thénard; *Tod* for Todd. The names of medicines are scarcely less scurvily treated: hydrargerum and hyociamus startle our orthographic sense on more than one occasion. Besides these appear such words (many of them *passim*) as d*i*spnœa, d*i*spepsia, anal*a*gous, cahexia, con*j*estion, hæmoptosis, *laying* in the sense of lying, shre*a*ds mucilag*e*nous, putr*i*faction, sept*œ*, &c. Nor can Dr. Campbell be considered always fortunate in the notions he appears to entertain of the meaning of words, or particularly elegant in their selection. In the preface, he speaks of the "*innate* conviction" he entertains of the importance of his volume; elsewhere, talks of a "*drummy* abdomen;" "contents himself with *preceding* the detail of cases with a brief announcement," &c. At page 5, he laments that "we are frequently compelled to *minister to diseases* in an empirical manner." Shakspeare, who probably suggested the phrase here used, is not responsible for its erroneous meaning, as it stands in Dr. Campbell's pages: Macbeth asks his doctor if he can "*minister to a mind* diseased?"—not if he can minister to a disease of the mind,—which latter query would simply mean, Canst thou add fuel to flame, and make the malady worse than it is?

Art. XVII.

The Gulstonian Lectures for 1842, *on the Mutual Relation between Anatomy, Physiology, Pathology and Therapeutics, and the Practice of Medicine.* By Marshall Hall, m.d. f.r.s. &c.—*London,* 1842. 8vo, pp. 86. With Three Plates.

That the subject of these Lectures was well chosen, no one can hesitate to admit; and that few of the present generation have surpassed the lecturer in the successful application of physiological truth to the explanation of pathological phenomena, will also, we think, be now generally conceded. Yet we must take the liberty of expressing some degree of disappointment in regard to the mode in which he has treated his subject; for the greater part of the lectures consist of familiar illustrations of well-known principles, such as appear to us better adapted for the instruction of students than for the edification of the learned body to whom they were addressed. We doubt not, however, that Dr. Hall was guided, in his selection of topics, by his estimate of the previous education possessed by his auditors, and of their general habits of thought; and of this we find an obvious indication in the introductory remarks, which contain what we should have scarcely thought necessary in *these* days, a defence of the general principle, that no one can be a sound practitioner, whether of physic or surgery, who is not also conversant with the leading facts of anatomy and physiology.

Nevertheless, like all that Dr. Hall writes, these lectures contain many acute ideas and interesting suggestions; but these are not unmingled with what we deem grave errors. We shall briefly notice the chief of each class.

The first lecture is devoted to Physiology; the second to Pathology; and the third to Therapeutics. The first topic considered is the relative importance of the functions of ingestion and egestion; and Dr. Hall arrives at what we deem the very just conclusion, that, startling as it may *primâ facie* appear, the functions of egestion are more immediately necessary to the maintenance of life than those of ingestion. An animal may live for some time without food, and even without oxygen; but it is speedily killed if the excretion of carbonic acid by the lungs, or of urea by the kidney, is prevented. The interesting enquiry is suggested, whether the law of diffusion of gases (by which the interchange of oxygen and carbonic acid in the air-cells of the lungs is regulated,) will enable us to determine by what addition of carbonic acid to the atmospheric air, the exhalation of this gas from the blood may be entirely prevented. That it is the accumulation of carbonic acid in a limited atmosphere, rather than the withdrawal of oxygen, which is injurious to an animal confined in it, has been shown by the experiments of Dr. D. B. Reid; who found that if provision be made for withdrawing the carbonic acid from the air as fast as it is generated, (by means, for example, of a large surface of lime-water or solution of caustic potass,) the animal appeared to suffer but little inconvenience until the oxygen was nearly exhausted. The next of the chief subjects treated of in the first lecture, is the Physiology of the Circulation, in which Dr. Hall repeats his well-known views as to the entire dependence of the capillary and venous circulation upon the action of the heart; upon this question, however,

we have too frequently stated the grounds of our entertaining a contrary opinion, for it to be requisite that we should now enter upon it. We shall only stop to remark, therefore, that Dr. Hall, in alluding to the supposed explanation of the circulation of *acardiac* fœtuses which was offered by Sir A. Cooper, does not seem to be aware that there is at least one case on record, in which, after the most careful examination by an accomplished anatomist, this explanation was found to be completely inapplicable. We may further point out that Müller, who is quoted by Dr. Hall as an important authority on his side, can scarcely be regarded as a consistent advocate of the doctrine; since whilst he states in one place that "the motion of the blood through the capillaries is effected solely by the action of the heart," he admits in several others, that the circulation may be greatly influenced by a local change in the part, *turgor vitalis* for instance; which is only another mode of saying that an increase of vital action in the part will accelerate the circulation through it, the very point upheld by Dr. Alison, Dr. Carpenter, and others who take the opposite side to Dr. Hall in this question. The coronary circulation is next dwelt upon in some detail; and it is shown that the union of the systemic and pulmonic hearts into one mass obviates the necessity, which would otherwise exist, for a special distribution of vessels to the right side. In fishes, in which the whole heart is pulmonic, and in which the trunk that issues from it is to be regarded as a pulmonary artery (conveying venous blood) rather than as an aorta, the coronary vessels arise, as we are informed by Dr. Grant, not from that trunk, but from the vessels that return arterial blood from the gills and send it to the system. The flow of blood into the coronary arteries is synchronous, not with the systole of the heart, but with its diastole: the flow is prevented by the contraction of the muscular structure, which is seen to render the heart pale so long as it lasts, and is occasioned by the subsequent reaction of the aorta. Dr. Hall thinks that the injection of blood into the tissue of the heart is a principal cause of its dilatation; and supposes that some variation in the arrangement of the arteries of the auricles and ventricles occasions their alternate action and regular rhythm. This idea, however, appears to us quite inconsistent with the well-known fact, that the hearts of cold-blooded animals will continue to act regularly long after they have been entirely drained of their blood. Dr. Hall subsequently makes some very just remarks upon the necessity of a due supply of arterial blood for the action of the nervous centres; but we think that he over-rates its importance in regard to the muscular system, since, as we have just seen, certain muscles will act for many hours without it.

In the second lecture, one of the subjects most dwelt upon is Asphyxia; and an interesting parallel is drawn between the gaspings and other violent abnormal efforts which are witnessed during suffocation, and the phenomena of death from loss of blood. Dr. Hall points out that, in both instances, the actions are probably not of a *reflex* character, as are those of ordinary respiration, but are *centric*, having their origin in the disturbance of the supply of blood to the medulla oblongata. He subsequently adverts to Secondary Asphyxia, or death from convulsion at a period more or less remote from that at which the cause of asphyxia was operating, as by no means an unfrequent result of accident and disease; and points out the necessity of continuing for some time our efforts to

restore the blood to its normal state. The influence of various forms of interrupted circulation in the production of congestions, extravasations of blood, dropsies, &c. is then ably pointed out; and the effect of a suspension of the coronary circulation in producing sudden death is much dwelt upon, perhaps rather too much. This last question Dr. Hall proposes to investigate by experiment; and until it shall have been determined, we think it better not to speculate upon it. Our own impression is, that a mere suspension of the coronary circulation will produce rather a gradual than a sudden failure of the heart's action. Several other interesting and important subjects are treated of in this lecture; but there are none which present any peculiar novelty.

In the third lecture we may especially direct attention to the remarks upon the Action of Poisons, (the investigation of which is very important to the pathologist from its connexion with that of ordinary medicinal agents,) as giving what we believe to be the true view of the subject. Dr. Hall considers that the view of those who advocate the doctrine of the transmission of all poisons by the vascular system, and of those who attribute their operation to the nervous system, are alike erroneous if *exclusively* adopted. The experiments of Mr. Blake and others have clearly proved that the local action of poisons on parts remote from those to which they are applied, is to be attributed *in many instances* to their absorption and actual transmission by the blood-vessels; but, as Dr. Hall justly remarks, there are many agents which indubitably act on the incident nerves, and which occasion phenomena analogous to those resulting from poisonous agents. He instances hydrophobia and tetanus as examples of the occurrence of two sets of parallel morbid phenomena, the former resulting from the reception of a poison into the blood, the latter from an irritation conveyed through an incident nerve.

We quite agree with Dr. Hall, (Preface, p. viii.) that "a treatise on the subject of these lectures, in which each principle should be set forth by a *fact*, an *observation*, or an *experiment*, would be of incalculable value to pupils." Such a treatise Dr. Hall has long projected; and we hope it will not be much longer ere he will be able to execute it.

ART. XVIII.

De Eventu Sectionis Cæsareæ, etc. Auctore C. KAYSER, Licentiato Medico.—*Havniæ*, 1841. 8vo, pp. 122.

On the Result of the Cæsarean Section. By C. KAYSER.—*Copenhagen.* 1841.

IT happens but rarely that we meet with inaugural dissertations of merit sufficient to claim a notice in these pages. For the most part they contain little that is either novel or important, but present hasty generalizations from a few cases, or consist of ill-arranged compilations made without any critical sagacity. It is but justice, however, to the University of Copenhagen to state, that a considerable proportion of the theses emanating from it are of a very different character, and of this number is that now before us. It is, indeed, a work which does honour at once to the author and to the university of which he is a member.

M. Kayser has applied himself with diligence to the investigation of a subject which required to be further elucidated than it has been hitherto,

and with reference to which there is in this country especial need of more extended knowledge. No space is occupied with curious though profitless antiquarian discussions as to the origin of the name by which the operation is usually distinguished, or as to its performance among the nations of antiquity, but the author at once proceeds to matters of practical importance. His object is to present a list as complete as possible of all well-authenticated cases in which the Cæsarean section has been performed, to display the results of the operation, and to examine the causes to which its success or failure may be attributed. It is true that this task is by no means new, but it has never been executed so well as by M. Kayser. All of the older writers on the subject have fallen into the error of reporting cases which were ill substantiated, a fault which resulted in great measure from their object being not so much to arrive at a knowledge of the actual results of the operation, as to prove that it might be performed with success. Thus, the work of Rousset, published in 1581, which was the first apology for the Cæsarian section, contains twenty cases, but not one of these was observed by Rousset himself, while some might be rejected without hesitation as being evidently fabulous. The same fault attaches more or less to all writings on the subject down to the year 1750, since which time more attention has been paid to collect unsuccessful as well as successful cases. It is therefore with great propriety that M. Kayser divides the history of the Cæsarean section into two periods, of which the former terminates with the appearance of Simon's essay in 1749; and the latter extends from the year 1750 to the present day.

It will be seen by referring to Baudelocque's essay on the Cæsarean Section, or to its translation by Dr. Hull, that the commonly-received estimate of the results of the operation is based in a great measure on cases reported to have occurred before the year 1750. The same observation holds good with reference to the elaborate collection of Michaelis; and several of the 258 cases which he has reported rest on very slender authority. M. Kayser's calculations are founded on the reports of 338 cases in which the operation was performed subsequently to the year 1750; and he rejects all instances of its performance before that time, either as being insufficiently authenticated, or at least too imperfectly reported to admit of any useful deductions being made from them.

Of these 338 cases, occurring between 1750 and 1839, 128 had a fortunate result, as far as the mother's life was concerned, while 210 terminated fatally. These figures give us a mortality of 62 per cent., which is 10 per cent. higher than that deduced from the investigations of Michaelis, or Levy, a recent Danish writer,* and 23 per cent. higher than the estimate formed by Dr. Hull. It appears, too, very probable that many cases in which the operation was performed have never been reported; and this was especially likely to be the case in former times, when medical journals were less numerous than they are at present. Unsuccessful cases were more likely to be left unreported than those in which the operation had a fortunate result, and it is therefore very probable that the real mortality is much higher than 62 or even than 67 per

* Dr. Churchill, in his elaborate work on Operative Midwifery, enumerates 316 operations between 1750 and 1841. In 149 cases the mother recovered, and 129 out of 182 children were saved: numbers from which results a mortality of 52·8 per cent. for the mothers, and of 29·8 per cent. for the children. Dr. Churchill's conclusions, therefore, tally exactly with those of Michaelis and Levy.

cent., as estimated by Velpeau. Wilde, indeed in his work entitled *Das weibliche Gebärunvermögen*, calculated the mortality at 90 per cent., and M. Kayser hesitates to assert positively that that estimate is too high.

Leaving these suppositions, however, and adopting the data with which M. Kayser has furnished us, we arrive at one very encouraging result; namely, that the fatality from the operation has been progressively diminishing since the year 1750.

	Successful cases.	Fatal cases.	Rate of mortality.
1750 to 1800	37	80	68 per cent.
1801 1832	54	94	63
1833 1839	37	36	49
	128	210	(p. 99.)

The event, as regards the life of the child, has not been noted in all cases, but it does not seem that the mortality has undergone any important diminution, but has continued with occasional slight fluctuations at about 31 per cent.

After ascertaining the absolute mortality of mother and child from the Cæsarean section, M. Kayser next passes to the enquiry, (p. 109,) whether there exists any connexion between the recovery of the mother and the survival of the child. Michaelis denied the existence of any such connexion. It results, however, from our author's investigations, that only 27 per cent. of the infants were still-born in cases where the mothers did well, while their mortality amounted to 32 per cent. in those instances in which the mother lost her life.

The chief points which might be expected to influence the event of the operation both to mother and child, are the duration of the labour, the length of the interval between the rupture of the membranes and the performance of the Cæsarean section, and the circumstance of attempts having or not having been previously made to deliver the patient with the forceps. In cases where labour had lasted more than seventy-two hours, the mortality was 72 per cent. (p. 111;) in those where it had continued for a shorter time, only 61 per cent. The prospect of a favorable result for the mother is likewise increased by the performance of the operation before the rupture of the membranes, while the probabilities of an opposite result are greater when the liquor amnii has escaped long before the commencement of the operation. The influence of the duration of labour upon the infant's life, however, is much more striking. Sixty per cent. died when labour had lasted more than seventy-two hours, 33 per cent. when it had lasted between twenty-five and seventy-two hours, and only 28 per cent. when the Cæsarean section was performed within twenty-four after labour had begun. The influence of the escape of the liquor amnii upon the mortality of the infants is precisely such as we should anticipate. The numbers, 14, 22, and 49 per cent., representing the rate of their mortality according as the operation was performed within six or twenty-four hours after the rupture of the membranes, or after the lapse of a still longer time. The previous employment of instruments does not appear to have much influence on the fate of the mother, though it greatly diminishes the chances of survival for the child.

The supervention of inflammation is the most to be dreaded of all the consequences of the Cæsarean operation.

"In 123 cases the cause of death was stated with more or less accuracy; and it appears that 77 women died from inflammation, or its consequences, and 29 from the shock to the nervous system. Internal hemorrhage occurred in 10, in whom coagula of blood were found in the abdomen; 2 died from external hemorrhage; 2 from pneumonia; 1 from rupture of the uterus, and consequent hemorrhage, on the seventh day after delivery; 1 died from osteomalacia; and 1 from the immediate effects of the operation, only twenty-four hours after its completion." (p. 129.)

Inflammation, then, was the cause of death, in 63 per cent. of the cases, while only 24 per cent. died from the shock to the nervous system.

The date of the death of these persons was as follows: 1 died, as above noticed, immediately after the operation, 9 within six hours, 16 between six and twenty-four hours, and 108 within the first week, 16 died between the first and third week, 1 on the thirtieth, and 1 on the thirty-sixth day.

The above are only some of the principal points connected with the Cæsarean section which M. Kayser's diligence has elucidated. The influence of many other circumstances on the results of the operation is displayed; but we have reached the limits available for the present notice, and must refer our readers to the dissertation itself, of which we can honestly say, that the library of no teacher of obstetric medicine will be complete without it.

Art. XIX.

1. *Analekten über Kinderkrankheiten.* Band III. und IV.—*Stuttgart*, 1837.

Selected Essays on Children's Diseases. Vols. III. and IV.—*Stuttgard*, 1837.

2. *Von Mezler, Sammlung auserlesener Abhandlungen über Kinderkrankheiten.* IX. Hefte—*Prag.* 1836-40.

Von Mezler's Collection of the most important Essays on the Diseases of Children. Nine Parts.—*Prague*, 1836-40. 8vo.

The first two volumes of the Stuttgard Selection on Children's Diseases were noticed with well-merited commendation in this Review for October, 1836. Since that time not only has the work been brought to a termination in a manner highly creditable to its editor, but a second series of essays, arranged on a similar plan, has been published at Prague under the superintendence of Dr. v. Mezler.

The nature of these compilations was sufficiently explained in the notice of the first two volumes of the Analekten. Without endeavouring, therefore, to enumerate all the subjects treated of in the volumes now before us, we shall seek to cull for our readers whatever may appear to be most novel or important.

Dropsy. The first paper on which we light is on a peculiar form of dropsy, (Analekten, Heft ix. p. 42,) by Dr. Wolff, of Bonn, a physician deservedly eminent in the Rhine provinces for his skill in the management of infantile diseases. This affection is not very unusual, Dr. Wolff having

met with above a hundred cases in his own practice during six years. He has never observed it after the age of puberty, but the children attacked by it were in most instances between their second and fifth year. It is usually ushered in by general indisposition, with loss of appetite, and an irregular state of the bowels. The tongue is slightly furred, and the patient suffers from occasional pain in the abdomen, with some degree of fever and acceleration of pulse. These symptoms are frequently so slight as to be overlooked by the parents, but at other times attention is excited by the severity of the abdominal pain and the frequent occurrence of vomiting. After the lapse of from five to fourteen days, during which it often happens that drastic purgatives are given to the child, under the supposition that it is suffering from worms, the pains become constant, the febrile symptoms more strongly marked, the loss of appetite is complete, and the abdomen grows tumid, and on a careful examination yields a distinct sense of fluctuation. Among the symptoms of the disease, Dr. Wolff attaches considerable importance to a peculiar tumidity about the root of the nose, and the value of this sign is confirmed by Professor Nasse.

This form of abdominal dropsy does not reach so great a degree as is often attained by ascites in the adult, and it is never associated with œdema of the extremities. Hence its real nature may be overlooked. If, however, the disease be left to itself, the extremities of the child grow emaciated by degrees, till at length the skin hangs around them in folds; fluctuation becomes gradually more obscure without any diminution occurring in the size of the abdomen, the patient's strength fails, universal emaciation takes place, the bowels are now purged, now constipated, irregular accessions of fever come on, and the little sufferer pines away into its grave.

This fatal event, however, is unusual, for the disease appears very accessible to the influence of proper treatment, and hence Dr. Wolff has never had the opportunity of making a post-mortem examination. He conceives that the affection consists in a chronic form of peritonitis, and accordingly employs in its earlier stages leeches to the abdomen, together with small doses of calomel. In some instances digitalis was combined with the calomel, and in the more advanced stages of the disease great benefit appeared to result from a combination of small doses of digitalis and cream of tartar. Whenever the disease has reached its second stage it becomes especially important to make a correct diagnosis, lest the practitioner should be betrayed into the employment of stimulating remedies, which invariably produce an injurious effect.

Dr. Wolff expresses his conviction that many of the cases of atrophia infantum, in which no scrofulous disease exists, are produced by the injudicious management of this form of chronic peritonitis.

Stomacace. Passing over two essays on mesenteric disease, the one by Dr. Wendt of Breslaw, the other by Guersent of Paris, and extracted from the Dictionnaire de Médecine, we come to a paper on stomacace, by Jörg of Leipsic, and to another on gangræna infantilis, by Richter. The latter paper is judiciously compiled by the editor from various memoirs by Richter, but its subject is more familiar to the medical reader than that peculiar affection to which Jörg gives the name of stomacace. It occurs in children between the periods of dentition and puberty, and

consists in the eruption of several small vesicles containing a transparent fluid, and surrounded by an inflamed area, on the tongue, the gums,and inner surface of the lips. In the course of a few days these vesicles become converted into small excavated ulcers, which are not coated by lymph, and furnish only a very scanty secretion of pus. The whole lining membrane of the mouth is at the same time more or less inflamed, the secretion from the salivary glands is increased in quantity as well as altered in character, giving out a very offensive smell, and the ulcers are so painful that all movements of the mouth, as in mastication or speaking, are avoided by the children as much as possible. Loss of appetite, slight fever, and a constipated state of the bowels, are associated with the local affection; but under judicious management, the disease usually runs its course in from eight to twelve days. Purgative medicines and appropriate diet, and washing the mouth frequently with aromatic gargles are the means requisite for its cure. Professor Jörg states that he has never known a case in which this affection terminated unfavorably, and we refer to it now partly from having seen it occasion considerable alarm to persons who did not sufficiently understand its differences from the far more formidable disease, *cancrum oris.*

Malignant scarlatina. In the eleventh number, besides many other very interesting papers, we meet with the account, by Professor von Ammon of Dresden, of an epidemic of malignant scarlet fever which prevailed in that city during six months, at the close of 1831 and beginning of 1832. In October 1831, several cases of *scarlatina simplex* appeared in one of the suburbs of Dresden, and ran a favorable course, but in other parts of the city many children died from a cerebral affection which was unattended with any trace of an eruption, but proved very rapidly fatal. In the middle of December cases of scarlet fever became much more numerous, and in January 1832, the disease was prevailing epidemically in all parts of the city. It attacked both sexes, and persons of all ages, and medicine did not seem to exert much influence on its course, death or recovery frequently occurring when least anticipated.

The onset of the disease was sometimes announced by the usual premonitory symptoms; but often persons were attacked by pneumonia, and expectorated blood, till with the appearance of the rash of scarlet fever all signs of disease of the respiratory organs disappeared. Children were sometimes seized while at play with violent headach; they fell into a sleep succeeded by coma or convulsions, and died within twenty-four hours. In others the eruption suddenly broke out while they were apparently in perfect health, and the fever ran its course without the supervention of a single bad symptom. The exanthema presented very various appearances, and its eruption was frequently accompanied with an extremely dangerous affection of the nares, throat, and respiratory organs, from which an ichorous secretion was poured out. The heat of the skin was great and pungent, the pulse was extremely frequent, and, though large, extraordinarily feeble. In those who died during the fever, head affections were the most frequent cause of the fatal result, but sequelæ of all kinds were very numerous and severe, and the dropsy which followed desquamation was especially to be dreaded.

Great turgescence of the vessels of the brain and congestion of the dependent parts of the lungs were generally found after death. In the

body of one patient the whole substance of the heart showed marks of inflammation, which had terminated in gangrene. Several bullæ existed on the surface of the liver, and there were large ecchymoses on various parts of the intestines. The whole of the corpse gave out an insupportably gangrenous odour. These appearances were met with only in one case; but in all, the right cavities of the heart had a peculiar red colour, and the heart as well as the large arterial trunks contained large, firm, fibrinous coagula.

The treatment of the epidemic presents nothing of particular moment. Depletion was never found to be useful; but in many of the worst cases great benefit resulted from the administration of large doses of the carbonate of ammonia, as six or eight grains every hour.

Dropsy occurred in a great proportion of the cases. It was frequently accompanied with violent palpitation and other signs of affection of the heart, requiring the free use of local depletion. Antiphlogistic measures and mild diuretics led to the recovery of the patient in many instances; but not unfrequently, after the dropsy had been got rid of, and many days or even weeks of tolerable health had elapsed, inflammation was again set up in some internal organ, and purulent effusions were found after death in the pleura or pericardium or between the muscles.

If the inflammatory affection of the heart was overlooked at the outset, it often happened that the dropsy disappeared; but organic disease of the heart remained behind, under which the patients sank after some severe and protracted suffering.

The twelfth number occupies the whole of the fourth volume, which contains nearly five hundred pages. It opens with two essays on the Diagnosis of Infantile Diseases, by Valleix and Professor Naumann. Next follow Dr. Gregory's article on Smallpox, from the Cyclopædia of Practical Medicine, and Professor Wendt's essay on Scarlet Fever, somewhat abridged from his Manual of Children's Diseases. There are besides many other papers in this volume of which we would gladly present our readers with an abstract, but we must leave space for some notice of V. Mezler's judicious collection of essays.

Dr. v. Mezler has followed a plan similar to that adopted by the editor of the Stuttgard *Analekten*, but has wisely avoided, except in a very few cases, extracting the same papers. General observations on the diagnosis and therapeutics of children's diseases, by various writers, with an account of Goelis's practice at the Vienna Dispensary for Children, occupy the first number. The second number contains some general remarks on the diseases of children and their remedies, and two essays, one by Formey, on the encephalitis of children, the other by Hinze, on the greater frequency now than formerly of cerebral affections in early life.

Hydrocephalus. This is not the place for an examination into the correctness of Formey's theoretical views; of the value of his practical observations there can be no doubt. The points to which he attaches most importance, as aiding diagnosis in the earliest stages of acute hydrocephalus, are an eruption of a strophuloid character on the outer side of the upper arm, sometimes on the cheeks and lips; the appearance of peculiar, glistening, micaceous particles in the urine, of which Coindet speaks; the changeableness of the child's temper and peculi-

arity of its cry; staggering in walking, and frequent falling; inclination to sickness and actual vomiting, drowsiness without sleep; and the little impression produced by medicine, especially by purgatives. Such are the symptoms by which the disease is characterized during that stage when we may hope to cure it. The occurrence of effusion and the consequent great increase of danger are marked by the restlessness of the child giving place to a state of apathy interrupted by occasional cries, but from which it cannot be roused except by raising its head from the pillow. The eye now loses its sensibility to light, the pupil becomes preternaturally dilated, and the children lie in a condition resembling sleep with their eyelids half open. The vomiting occurs much more seldom, or ceases altogether, and the children devour in a hasty manner anything that is put into their hand. The pulse becomes slow and irregular, the bowels are confined, and the fæces solid and dark.

These symptoms are all of great value, though one or two of them we think are not of such universal application as they would appear to be from the above statement. The dilatation of the pupil is, we are convinced, the sign least of all to be relied on; for it not only varies as to the time of its occurrence, but it is not even observed in all cases. The slowness of the pulse, too, is sometimes wanting; its occasional irregularity we conceive to be of considerably greater importance, though we do not regard it as a symptom peculiar to the stage of effusion.

Formey is a great advocate of cold affusion on the head, which he directs to be repeated every hour or every two hours; and states that its continuance during several days may be requisite. The evidence of such men as Formey and Heim in favour of this practice would carry great weight, even though its good effects were not attested by many subsequent writers. The practitioner, however, should well count the cost before he begins to employ this remedy, and should above all make sure that the parents will second him in carrying out a plan which has the appearance of being needlessly cruel. With regard to the best time for its employment, we refer our readers to some very sensible observations by Dr. Münchmeyer, in the Hannoversche Annalen, of which an abstract was given in the October Number of this Journal. (Vol. XII. 559.)

The sixth number contains a paper by Professor Nasse, of Bonn, on the same subject as that by Hinze, and like everything written by that distinguished pupil of the famous Reil, it bears the impress of much research and thought. It does not, however, admit of condensation.

The eighth number contains several essays on measles and scarlet fever, from which we select one by the late Dr. Heim, of Berlin, on the differential diagnosis of scarlet fever, rubeolæ, (the Rötheln of German writers,) and measles.

Rubeolæ, (*Rötheln.*) Dr. Paterson, of Edinburgh, recently drew the attention of the profession in Great Britain to this variety of the exanthemata, in a paper published in the Edinburgh Journal for October, 1840. The subject, however, is of importance sufficient to claim a notice here, especially as no one has detailed the characteristics of rubeolæ with such minute accuracy as Heim. Heim is disposed to regard the disease as a variety of scarlatina: Dr. Wagner of Schlieben, however, states, in a paper originally published in Hecker's Annalen, that neither measles nor scarlet fever exercise any preservative influence in protecting a person from

rötheln. Of the two diseases, scarlatina certainly bears a much closer resemblance to rötheln than is presented by measles. Both scarlet fever and rubeolæ are characterized by great rapidity of pulse and by febrile symptoms, which often run very high. In both, the eruption usually appears within twenty-four hours after the invasion of the premonitory symptoms, and in both, sore throat is of frequent occurrence. Scarlatina, however, is sometimes unattended by sore throat, and at other times the affection of the throat is unaccompanied with any eruption; but in *rötheln* neither the angina nor the exanthema are ever absent. The character of the eruption of rötheln affords the chief means of discriminating between these diseases. Like the eruption of scarlet fever, it usually appears simultaneously upon the whole body; and, like it, is not accompanied with any sensible elevation of the skin, such as distinguishes the eruption of measles. But its colour is somewhat darker than that of the eruption of scarlet fever, and it is not subject to that sudden disappearance which occasionally occurs in the course of that disease. The characters which it puts on are twofold; consisting either of innumerable little dots, one line or a line and a half in diameter, of a somewhat irregular form, and presenting a well-defined outline; or the patches are destitute of a distinct boundary, and, if the disease is severe, coalesce so as to bear some resemblance to the rash of scarlet fever. The rash of rubeolæ, like that of scarlatina, disappears under pressure, but on the pressure being remitted the distinct spots of rubeolæ become evident, and from them the general redness extends. No such spots, however, appear if the finger is removed from the skin of a scarlet fever patient, but the redness returns without any such central point being observed.

The subject of rubeolæ requires still further investigation than has ever yet been bestowed upon it. There are many important differences between the statements of Heim and those of Dr. Paterson with reference to the diagnosis of the disease. Authors, too, are by no means agreed as to whether *rötheln* is a variety of measles or of scarlet fever, or whether it is, as many suppose, a really independent disease.

Our stock of essays is not nearly exhausted, but our space is; and we have room only most heartily to commend both the Stuttgard and Prague *Analekten* to all our readers, as forming by far the most valuable collection of essays on children's diseases with which we are acquainted.

Art. XX.

Die Lehre von den Zeichen, Ersheinungen und der Dauer der menschlichen Schwangerschaft, so wie von den Phänomenen einer überstandenen Geburt. Von W. F. Montgomery, a.m. m.d. m.r.i.a. *Bonn.*

An Exposition of the Signs and Symptoms of Pregnancy, the Period of Human Gestation, and the Signs of Delivery. By W. F. Montgomery, a.m. m.d. m.r.i.a. Translated by Dr. Schwann. Introduction by Dr. Kilian, Professor of Midwifery at Bonn.

In a former Number of this Journal*, we reviewed the original work from which this translation has been made, and expressed very fully our favorable opinion of its merits; since that period, four years have

* No. VIII. p. 167, Oct. 1837.

passed away, and public opinion, both in this country, in America,* and on the continent, has fully ratified our verdict.

Were anything wanting to add to the high opinion of this excellent work which we have always entertained, we should find it in the circumstance of its introduction to the German public under the auspices of such a judge as Kilian, one highly qualified by his own continued studies and practical experience to guide the public judgment aright in this branch of medicine.

The translation appears to us to be correct, and as literal as the spirit of the two languages would permit of, doing considerable credit to Dr. Schwann's literary powers.

It is to be regretted that those beautiful plates of the areola surrounding the nipple in the successive months of pregnancy, which adorn Dr. Montgomery's work, have not been copied into the translation; by omitting them, the translator has acted in a certain degree illiberally towards the author, because it is impossible for any description, howsoever lucid, to convey to the mind the impression at once transferred to it by those beautiful drawings. We were also surprised to find that the plate illustrative of the decidual cotyledons, a structure first described by Montgomery, has also been omitted; the same remark applies to this, as to the former omission, and we think this plate, containing one of the most original points in Montgomery's work, ought to have been inserted.

In our former review of the English work, we stated that we considered Dr. Montgomery's observations on the "application of auscultation to be good, but rather meager," his evident object being to render it the confirmatory or conclusive evidence where other signs were present, although in some occasional instances it alone would be sufficient to detect the existence of pregnancy. Dr. Kilian, in his introduction, has followed up this matter, and has entered fully into the signs given in pregnancy by the stethoscope, we fear, with rather too much accuracy; yet, having done so, he feels it requisite to state his doubts and difficulties. Amongst the first of these, he thinks that it is not every accoucheur who possesses that sharp and clear sense of hearing and of appreciating sounds necessary for the discriminating of difficult cases. It would be as rational to expect that every one should have equally good sight; "and we will here subscribe to the opinion of Henne, and we have had a sufficiently extensive experience to prove, that there is not more success to be obtained in auscultating the uterus than the chest." Such are the words of Kilian, a good and well known stethoscopist. We find Dr. Montgomery quoting Laennec to the same effect, with this difference, that that accurate observer was of opinion that, to form a correct judgment from the sounds given by the uterus required more care, and was beset with more difficulties than all those found in investigating the diseases of the chest. If the stethoscopic symptoms alone were to determine pregnancy in every case, in what inextricable confusion would the practitioner be involved, who should be consulted in such a case as that recorded at page 123 of Dr. Montgomery's work, where a large abdominal tumour gave rise to suspicion of pregnancy, of which several symptoms existed, especially a very distinct *placentary murmur*. Here, too strong credence in the stethoscope would have engendered error, and we should not envy

* Two editions have since been printed in America.

the practitioner who pinned his faith on the instrument if, when nine and ten months had passed by, and no appearance indicated approaching labour, he should begin to suspect that he had allowed his quickness of ear to deceive him, and that he had foolishly made use of but one sense, in place of employing all in the service of his patient and of his own reputation.

Dr. Kilian animadverts in very animated terms on the confidence felt by some persons in their powers of hearing in determining the death of the fœtus in utero, and expresses his opinion that the spirit of imitation may lead young and inexperienced persons to be too ready in the employment of the forceps and perforator, a privilege which should alone be enjoyed by the masters in the art. Pursuing the subject, he refers to Dr. Collins of Dublin, to whom he gives the title of the *most daring perforator of any period:* he (says Dr. Kilian) only perforates when he can no longer hear the beat of the fœtal heart; who might not copy him when Deisch, one of the correctest and greatest amongst modern authorities, is involved in the very same accusation, and rest content, like Collins and all enthusiastic friends of the stethoscope, of being perfectly satisfied with its evidence alone? From these and other passages, we find that Kilian agrees with Montgomery in the useful, but guarded and cautious, employment of this instrument, fully feeling its value, but not permitting it to usurp the place of the other physical signs and symptoms.

The speculum, in the opinion of Kilian, is of far less value than the stethoscope in discovering the different states of pregnancy, although its utility is unquestionable, and its employment indispensable, in investigating the various diseases and alterations which occur in the os tincæ and vaginal parietes. He adduces a curious fact, that this instrument, which has been but a few years introduced into general practice, was known and employed some centuries ago; and reference is made to the work of Paulus Ægìnetus, (Ed. Basil, 1538,) where the instrument is described under the name of Δίοπτρον, and its employment is termed Διοπτρισμὸς; and Dr. Kilian recommends that the words Dioptrum and Dioptrismus, as analogous to Catheter and Catheterismus, should be adopted into our medical nomenclature, as far more elegant than the indelicate term, speculum vaginæ. We warmly join in this recommendation.

One of the signs of pregnancy laid down by Jacquemin, and attested by Ricord, Kluge, Parent-Duchatelet, Lauer, and others who have had very extensive experience, viz. a bluish or livid colour of the vagina from the os externum to the os uteri, is spoken of by Kilian as one of the most constant signs of pregnancy; and the only thing wanting to make it one of the most valuable signs is to know if it be found in this state of the uterus only. Dr. Montgomery, after quoting the opinions of Kluge, Jacquemin, &c., states that nothing within his observation contradicts the accuracy of the sign under consideration. Duchatelet mentions that M. Jacquemin's accuracy in discovering pregnancy by this test was fully proved by a trial made for this purpose on no less a number than 4500 prostitutes.

Still Dr. Montgomery entertains doubts whether this sign, so constant in pregnancy, may not exist under other circumstances producing increased vascular determination or congestion of the genital organs; he says: "In a case lately seen, it became necessary to examine the vagina

at the time of menstruation, and this purple hue of the mucous membrane was distinctly perceived.....It is also known that a common mode, long in use, of ascertaining whether certain of the lower animals are in a state fit for intercourse with the male, or in heat as it is called, is to examine the orifice and internal surface of the vagina, which, under such circumstances, is found almost inky dark." Mr. Cruikshank, in the Philosophical Transactions for 1797, p. 199, states that the method pursued by the feeders of rabbits for knowing when the female is in fit state for impregnation, is to turn up the tail and invert part of the vagina, when, if the animal be, as they term it, "in heat," the orifice and internal surface will then *be black as ink from the great derivation of blood to these parts.* "These considerations," says Dr. Montgomery, "must considerably modify the value of this test; but, nevertheless, should subsequent observations prove that healthy pregnancy is invariably accompanied by such an appearance becoming visible within the first or second month, the fact would certainly be one of the most important additions ever made to our means of making a correct diagnosis in cases of early pregnancy, and the more especially as it would be applicable to a period at which we have no other satisfactory means of discovering the existence of that condition, and might occasionally, under peculiar circumstances, be resorted to with propriety and advantage."

The ninth chapter, in which an examination of substances expelled from the uterus—early ova, moles, hydatids, membranes formed in dysmenorrhœa, and in other conditions of uterine derangement,—is given, has been peculiarly well rendered by the translator. The entire chapter is full of interest. Our author does not take upon himself to decide the question as to whether moles are to be considered as a product of conception or not, although it is evident he leans to the former opinion; yet his cautious advice in giving evidence in a medico-legal point cannot be too much admired. Having cited a number of conflicting authorities on this subject, he says "he is not prepared to undertake to reconcile them, but it appears to him almost certain that much of the discordance has arisen from substances of very different characters having been indiscriminately classed together under the general term of moles, some of which were undoubtedly neither more nor less than diseased ova, or remnants of such, while others were as certainly either merely condensed coagula, or perhaps uterine polypi. Hence Mahon* appears perfectly justified in making the following remarks: "The existence of moles properly so called, is extremely doubtful, since they may all be referred to some one or other of the substances of which we have spoken; viz. a placenta which had continued its growth, the fœtus having perished; the degenerated remains of the afterbirth: coagulated blood; sarcomatous tumours or polypi of the uterus. The first two cannot exist except after sexual intercourse; the other three may be found independently of it. This is the distinction, which it is of the greatest importance to make in questions of legal medicine, that we may not without cause compromise the reputation of the unmarried girl, or the widow of irreproachable life and conduct." In this view the writer entirely coincides, and thinks the medical jurist would not be justifiable in pronouncing any such mass expelled from the uterus as proof of pregnancy, except he can detect in it either the fœtus or a part of it, or some other of the component structures of the

* Médecine Légale, tom. i. p. 274.

ovum; and even then, without further proof, "we must not," to use the words of Morgagni,* "immediately doubt the woman's chastity, since, as has been said above, the placentula might have remained in the uterus formerly, in an abortion that had not been much taken notice of;" which remark he makes in reference to cases in which portions of placenta appeared to have been a long time retained in utero, from which they were afterwards expelled in the form of moles when the women were advanced in life and many years widows,† as happened in the case related by Ambrose Paré,‡ in which a mole was retained seventeen years.

Dr. Montgomery states his opinion unequivocally, that uterine hydatids do not occur except after sexual intercourse, and as a consequence of impregnation; he never having met or heard of a case in which their presence was not accompanied or preceded by the usual symptoms of pregnancy. "It may not be amiss to notice here an argument from analogy, which has been brought forward against this view of the question, namely, that hydatids being formed in other situations, as the brain, &c., why may they not occur in the uterus also independently of any other circumstance, as intercourse or conception? To this I would reply, first, that the hydatids produced in the situations alluded to differ *toto cœlo* in their characters from those of the uterus; and secondly, that whenever hydatids are formed it is always in connexion with serous membranes which do not exist in the uterus until the ovum is deposited there, whose membranes are essentially serous." This reasoning is ingenious: however, in a medico-legal point of view, Dr. Montgomery thinks "it would be presumptuous and absurd to maintain that, because we had always found hydatids in connexion with one particular cause, there might not be some other also capable of producing them; and as there may be a doubt, we must let the accused have the benefit of that doubt.

In writing this notice of Dr. Schwann's translation, we feel we have not done full justice to Dr. Montgomery's work; but we did not like to touch on any of the subjects which had entered into our former review, in which we naturally selected those which we thought of the highest interest. The sound and correct practical remarks, which abound throughout the volume, must be of extreme value to the physician in every country, and we perfectly accord with Dr. Kilian in the sentiment expressed in his preface, "that in advising his friend Dr. Schwann to undertake its translation, he was conferring a boon on his native land; that the transplanting of this work to a German soil would afford additional means to the German physician for contending against error, confirming truth, exciting him to new discoveries, and bringing his information on those subjects nearer and nearer to perfection."

ART. XXI.

Principles of Surgery. By JAMES SYME, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh, and Surgeon to the Queen. Third Edition.—*London*, 1842. 8vo, pp. 505, with Plates.

UNDER the above title, Mr. Syme has published a work which has extended to three editions, a pretty conclusive proof that it is highly esti-

* Epist. xlviii. art. 13.

† See Mém. de l'Acad. Roy. des Sc. for 1735: Vallisneri, tom. ii. cit. p. 2, cap. ult.

‡ Lib. xxiv. chap. xl. xliii. p. 718.

mated. A work may have a certain value because it may be the only one on the subject, and thus, though intrinsically of no great excellence, it may occupy a prominent place; but this is not the case with the work before us: it has several competitors, some more recently written, and with one exception its rivals are all by Mr. Syme's countrymen. We may be accused by our northern brethren of partiality, but we must say that although each may have its merit, we prefer to any of them the humble "First Lines of Surgery" of Mr. Samuel Cooper. When Mr. Syme states that his work is intended to give a comprehensive and systematic view of the facts and opinions which constitute the science of modern surgery, all we can say is that we are astonished at the paucity of those facts and opinions. And when he states that the present edition has been carefully revised and corrected in every point where the improvements of others or his own reflection and experience suggested alteration, we merely say that he has passed a very harsh judgment upon many meritorious discoveries, if he be aware of them, and that if he be not he ought to be.

If we look to the subject of Inflammation, nothing is stated of the observations of Hastings, of Gendrin, of Kaltenbrunner, Macartney, and many others. If we turn to the subject of Suppuration, we have nothing of the curious researches of Henle, Gendrin, Guterböck, Mandl, Müller, Gulliver, and others on pus. In speaking of diseases of blood-vessels, no mention is made of dissecting and some other varieties of aneurism. And we should have thought it would have been proper to say something more of the ligature of the subclavian at its inner third, than this: "the subclavian artery may be tied also on the inner or sternal side of the scalenus, but the numerous branches that spring from the artery here, together with the close neighbourhood of the pleura, vein, and on the left side the thoracic duct, render this operation extremely difficult and dangerous." Would it not have been better to say how many times it had been done, and that it had never succeeded? There are many operations both difficult and dangerous which are performed every week—but is this one that should ever be performed?

These are the kind of drawbacks which are presented in almost every page of his work; and, considering that it is a class-book in the northern capital, and that it is most desirable that the quality of information communicated to the student should be improved, and the standard of education should be raised, we could have wished that more pains had been taken to indicate the sources to which the student should resort for important matter connected with many questions, at present involved in more or less of obscurity.

Mr. Syme is unquestionably an able surgeon, and understands surgery well; but the work before us leaves much to be desired as a text-book for the student. As the work has reached a third edition, it may probably reach a fourth; in that case we would suggest that, if the author's own time is too fully occupied with more important avocations, he should call to his assistance some well-informed young surgeon, who under the author's eye could render very important services in the preparation of the succeeding edition.

PART SECOND.

Bibliographical Notices.

Art. I.—*A Therapeutical Arrangement of the Materia Medica, or the Materia Medica arranged upon Physiological Principles, and in the order of the General Practical Value which remedial agents hold under their several denominations, and in conformity with the physiological doctrines set forth in the Medical and Physiological Commentaries.* By Martyn Paine, m.d. a.m., Author of the Commentaries, &c.—*New York*, 1842. Sm. 8vo, pp. 271.

Notwithstanding the obligations we lie under to the author of the small volume whose very characteristic title we have just transcribed, we are compelled, by a sense of duty, to state that it is not a very superior production. We cannot but regret the necessity of making this avowal, seeing that Dr. Paine hath honoured us of late, and we have bought golden opinions from all sorts of people, through his instrumentality. By means of various communications in the American journals, and sundry separate publications, having reference to this Review, its editor and contributors, which have been carefully transmitted by post to all the most eminent men in the profession in Great Britain, and even on the continent; and, lastly, by the kind dedication of the present volume "to the forbearing consideration of Dr. John Forbes," and to his "justice honour," he has not merely carried our fame into new regions, given our names a fresh impulse—

Virum volitare per ora,

but has actually ministered to our gratification on a point generally considered as touching all men most nearly, viz. the improvement of our exchequer. It is a fact which Dr. Paine can any day verify, by a reference to our New York publishers, that in the quarter immediately succeeding his great advertisement of this journal, the demand for it in America was increased one twelfth! But we are sure that Dr. Paine, with that liberality of sentiment, cool judgment, and ready credence of the assertions of men of honour, so characteristic of him, will not only forgive us for remaining unseduced by his moving blandishments, but will commend us (may we hope in another pamphlet?) once more to the medical world as the greatest of journalists, and the most impartial of critics.

After this long and exculpatory preface, it may be expected that we are going to be very severe upon Dr. Paine's volume. In one respect, this is the case, as *we* feel it to be a severe judgment to be obliged to say of so good a friend that he has written a book that is not first-rate.

The title-page, which we have copied nearly at full length, will give the reader a general idea of the nature of this volume. The "arrangement" adopted by the author is somewhat different from that of preceding authors, but not so much so as to be very noticeable by his readers. The following are the names of his "Classes," and of the "Orders" contained in the first class:

"Classes. i. Antiphlogistics; ii. Permanent Tonics; iii. Diffusible Sti-

mulants; IV. Cerebro-Spinants, or Nervous Agents; V. Astringents; VI. Uterine Agents; VII. Urinary Agents; VIII. Anthelmintics; IX. Errhines; X. Chemical Agents; XI. Diet and Regimen, in a general sense.

Orders. Class I.—*Antiphlogistics.* 1, Bloodletting; 2, Cathartics; 3, Emetics; 4, Alteratives; 5, Expectorants; 6, Direct Sedatives; 7, Diuretics; 8, Cutaneous and other applications; 9, Low diet and rest. (*Negative.*)

One feature of the present volume leads us to entertain a very exalted opinion of the intelligence and industry of the medical pupils in America. Dr. Paine, of course, knows their capacity and temper, and has too much sense to draw up for their especial use a book which they could not, or would not, understand. It is, however, certain that, in the particular referred to, Dr. Paine has gone much beyond the reach, if not of the powers, certainly of the will of the medical pupils in this country. We can truly add, that he has gone beyond *our* reach also; but we wish to lay little stress on this circumstance, as, on other occasions, we have acknowledged our feeble powers to be inadequate to follow the vigorous wing of Dr. Paine. Perhaps we are mistaken in thinking that the author's good nature has led him to place too much confidence in the zeal and industry of his readers, in believing that they will patiently work out from all quarters of his book the substantial materials of the numerous formulæ, of which he gives the mere symbols under the head of the individual articles of the materia medica. Of the accuracy of this opinion of ours—and we cannot but have some mistrust of any opinion at variance with that of Dr. Paine—we leave our readers to judge from the following extracts, which we take from the beginning of the volume, and which are fair specimens of the work. In order that we may place our readers in the same position as ourselves, in regard to the understanding of the extracts, we premise his "Instructions" as to the manner in which the symbolic formulæ are to be understood and elaborated. We hope none of our readers will be ill-natured enough to exclaim, with Dangle, in the Critic, "Egad! I think the interpreter is the hardest to be understood of the two!"

"For the sake of brevity, and to impress the memory, characters or symbols are frequently employed to indicate the combinations of remedies. The initial letters following the usual ℞ stand for the article with which the formula is connected. If a figure follow next, it stands for the article so numbered in that group. If an article be derived from another class, the word *class* is introduced, and the number following indicates which class. Then follows a number referring to the intended article in that class; or, if the class have orders or subdivisions, the word *order* or *sub.* with the number or letter indicative of which *order* or *subdivision*, precedes the number which stands for the remedy. If several numbers succeed each other uninterruptedly, after the announcement of the class, etc., they all refer to remedies under the same group."

"Jalap. The best formulæ are the following:—℞. J. 1. ℞. J. 1 and order 3, 1. ℞. J. 1, and order 4 A. 18 or 19. ℞. J. 11, ℞. J. 11, 12. ℞. J. 1, 12. ℞ J. order 4 A. 18 or 19, class 3. 12.[a] ℞. J. 23. ℞. J. infus. 6.[a] ℞. J. infus. 12.[c] ℞. J. infus. class 8. 2."

"Aloes. See Calomel and Blue Pill for several formulæ; also, ℞. a, 1. order 4. A. 18, class 4, order 1, 13, 16. ℞. a, 12, 52. ℞. a. 16,[b] 17. ℞. a. 16,[b] 17, 18, 31, order 4. A. 13. ℞. a. 1 or 2, class 4, order 1, 16. ℞. a. 1, 16,[b] 17, class 2, 15. ℞. a. 17, 12.[c] class 3, 14.[b] ℞. a. 12, 21, class 2, 9. ℞. a. order 4. A. 1, 13, Sub. 5, 1. ℞. a. 18, class 2, 43. ℞. a. 16.[a] 3,[b] 31, 17. class 4, order 1, 16."

"Rhubarb. ℞. R, 14. ℞. R. 1. ℞. R. 1. 3. ℞. R. 7. ℞. R. 6. 52. ℞ R. 6, 17. ℞. R. 22, class 3, 9.[c] ℞. R. 7. 14. ℞. R. 14, class 3, 11.[c] ℞. R. class 3, 12,[c] class 4, order 1, 11. class 10, 1."

ART. II.—*A Brief Commentary on Functional Derangement of the Stomach in Indigestion.* By WILLIAM WYNTER, M.D. Resident-Physician at Liverpool.—*Liverpool*, 1842. 24mo, pp. 25.

THIS is an incredible book: would to Heaven, as Dr. Johnson said of the piece of music, it had been an impossible one also! We have taken the trouble (God help us, poor critics!) actually to count the number of words in it, and they almost coincide with the number contained in FOUR of the pages of this Journal! What *can* be the meaning of such a publication as this? The only one conceivable to us, with our present lights, is one so discreditable to any member of an honorable profession, that we will not adopt it without further scrutiny: it is, that this miserable atomy—shadow—imago—eidolon of a book has been printed merely to furnish a substratum to an advertisement in the newspapers, to announce to the world around the Mersey the portentous intelligence, that

Now is the WINTER of our discontent

no longer to be found in "the county and borough of Brecknock," but is verily "Resident-Physician at Liverpool." But why *resident*-physician? Are the other physicians practising there, and practising so honorably and well, not resident also? Have our excellent friends, Drs. Jeffrey and Scott, and Macrorie and Baird become denizens of the steamboats that crowd the Mersey, or of the steam-carriages that shoot along the Birmingham Railway?—But we have already wasted too much space on a production which is in every respect inconceivably paltry and despicable, and of which it is difficult to say whether the language is more inaccurate or the matter more absurd.

ART. III.—*Discourse on the Enlarged and Pendulous Abdomen, &c. Augmented by a Dissertation on Gout, suggesting New Physiological Views of its Cause, &c.* By RICHARD FRANKUM, Esq. Surgeon.—*London*, 1842. 12mo, pp. 121.

A WORK of idler and shallower gossip, of triter commonplaces, and containing more slight and more ridiculous pathology, than that of Mr. Frankum, we have seldom set our eyes on. The singular originality of some parts has struck us. Thus (pp. 27-8), "The digestive power of the stomach is, in general, proportionate to the quantity of food given to it. . . . The natural powers, then, of the stomach, are in proportion to the quantity of food it has to feed on, as the size of a blacksmith's arm increases according to the strength demanded from it." An agreeable piece of information this, for those unruly patients, who, with weak stomachs, have unmanageable appetites, and who have only to become gourmands in order to rid themselves of their atonic dyspepsia.

The humanity and tenderness as well as the ingenuity with which Mr. Frankum argues the cause of the unfortunate owners of pendulous bellies, are creditable alike to his head and heart.

"Before," he says, "we proceed to the treatment of this disorder, we shall say one word of the attitude of those who have a protuberant or pendulous belly, in justice to those, and many are they, whose feelings are anything but ostentatious, as their gait implies. It is certainly somewhat odd that presumed dignity of personal appearance should be derived from unnatural causes, and

that the erect position of the body is conceived to be the *most dignified and imposing when corporeal substance and a projecting form are given to it.* All this is not less curious than true; and the strut of a man thus circumstanced is attributed to any but the right cause, viz., the infirmity of his condition. None but those who feel and suffer from this infirmity can understand the pain which modest minds are subject to endure from the construction which the world is too apt to put upon it. . . . Under all circumstances, it is a case that requires the kindliest instead of the harshest construction, and the silence of commiseration rather than the finger of remark." (pp. 41, 43.)

What Gibbon calls the "chivalry" of Burke's defence of the church and the clergy in his reflections on the French revolution, was nothing to this!

"Gout," Mr. Frankum tells us, p. 121, "is a disease essentially dependent on an inflammatory affection of the mucous membrane of the stomach, and its degrees of severity are also dependent upon the extent to which that organ is affected." This, we presume, is the "new physiological (?) view" announced in the title-page. But from other parts of Mr. Frankum's volume, we should scarcely have expected that he should have so specially and formally set himself forth as a supporter of the theory of an intimate relation between stomach disease and gout: for what do we find him saying at p. 27?—"There is no mistake more common than that a large eater has always a full and foul stomach; the interference with health seldom arises from an affection of this most vital of our organs, supposing it to be in a healthful condition!" And, in the second page following, he speaks "of the insusceptibility of the stomach to take on diseased action." We are informed at p. 114 that "purging is one of the best and safest diaphoretics" in gout.

In the foot-note references to this volume non-professional authors are more frequently quoted than professional. Schoolboy citations from Shakspeare occur repeatedly. Byron's Narrative, Childe Harold, Boswell's Life of Johnson, Bruce's Travels, Bacon's Essays, Middleton's Life of Cicero, are dragged on the arena, on occasions when assuredly the *nodus non vindicibus talibus dignus est.* We regret the appearance of such a work, and trust that our present notice of it will make apparent to Mr. Frankum the error he has committed in giving a production so totally unprofessional and so trashy to the public.

Art. IV.—*The First and Second Reports of the Medical Missionary Society in China; with Minutes of Proceedings, Hospital Reports, &c.—Macao*, 1841. 8vo, pp. 68.

These Reports form a complete account of the Society's operations from the commencement, which are extremely creditable to all concerned. They show the activity with which the society carries on its operations, and strikingly indicate not merely the benefits likely to be derived from it eventually, as a means of civilization, but even the vast present and immediate good in healing the diseases of the people. No less than 6300 cases were treated by the medical officers, Mr. Hodson, Dr. Parker, Mr. Lockhart, and Mr. Hobson during the first three years. A good deal of curious information is given respecting the diseases of the Chinese, which we regret being unable to lay before our readers; but we earnestly recommend this excellent society to their attention and patronage.

ART. V.—*Dr. Hooper's Physician's Vade Mecum; or Manual of the Principles and Practice of Physic. New Edition, considerably Enlarged and Improved, with an Outline of General Pathology and Therapeutics.* By W. A. GUY, M.B. Cantab., Professor of Forensic Medicine, King's College, London; and Physician to the King's College Hospital.—*London*, 1842. Sm. 8vo, pp. 493.

IT is often useful for the practitioner as well as the student to have a concise and correct epitome of the history and treatment of diseases to refer to, either as an awakener of knowledge laid by in the mind but perhaps forgotten for the time, or as a suggester of new views and plans for further investigation. Dr. Hooper's well-known Vade Mecum has long served this purpose; and to enable it to continue to do so, it was necessary to alter what was antiquated and obsolete, in its pages in conformity with recent improvements and newer doctrines. This task has been excellently well executed by Dr. Guy; and the student and general practitioner may confidently turn to the present edition for the information which it is the scope of the work to supply. We rather think, however, judging from the character of the alterations made in the work by Dr. Guy, that a less philosophical editor might have produced a work more suited to the wants of those likely to consult it. We think that the admirable FIRST PART, extending to more than one third of the whole book, entirely contributed by Dr. Guy, and devoted to the subjects of medical study, phisiology, general pathology, and therapeutics, is rather out of place in a work of this kind, which, in our opinion, should be confined to a concise exposition of the history and treatment of special diseases. By omitting this first or general Part, and devoting the whole attention to correct, amend, and especially to condense the SECOND PART, the editor might have presented us with a work of one half the size and price, and no less calculated than the present to fulfil what we conceive to be the true scope of a vade mecum. If, by such a plan, however, we had been altogether deprived of the matter contained in the first part of the present volume, we should have regretted its adoption. But we do not consider this as at all a necessary consequence. On the contrary, we think Dr. Guy instead of giving us one book should have given us two, namely, a treatise on general pathology and therapeutics, and an epitome of practical medicine; and when a new edition of the present work is called for, we strongly advise this course to be pursued; the vade mecum being condensed to a five-shilling duodecimo, and the new treatise expanded, if such is the author's pleasure, into a ten-shilling octavo.

ART. VI.—*Observations on the Admission of Medical Pupils to the Wards of Bethlem Hospital, for the purpose of studying Mental Diseases.* By JOHN WEBSTER, M.D. Second Edition.—*London*, 1842. 8vo, pp. 32.

THIS is a most seasonable appeal to the common sense of the Governors of Bethlem Hospital, and cannot fail to carry conviction to their minds, if they indeed possess the quality we have assumed. We think the whole profession and the public are under great obligations to Dr. Webster for his exertions in a cause incalculably important to both. No

medical man, after reading the statements and arguments of the author, can fail to be astonished that such a glaring anomaly should still exist as the exclusion from the course of study prescribed for students, of one of the most important diseases in the whole range of nosology, and one which every practitioner must sooner or later meet with in his every-day business; and we think that no well-informed layman can rise from Dr. Webster's pages and fail to perceive the absolute necessity of doing away with this anomaly. Dr. Webster shows in the most satisfactory manner that so far from the visits of pupils to lunatic asylums being injurious to the inmates, they are positively beneficial to them; a fact which would seem to remove the sole objection that has any show of force against the proposition advocated by him. In the latter part of his pamphlet, Dr. Webster notices the fact of Dr. Conolly being now actually engaged at Hanwell in giving a course of gratuitous clinical lectures on insanity to pupils from the different metropolitan medical schools; and we can add, from our own knowledge, that these lectures have been already productive of results of the most beneficial kind. In the case of Dr. Conolly, we see a memorable instance of the importance it may be to society that public offices should be well filled. By acting, at last, on the "Detur Optimo" principle, the governors of Hanwell have indirectly the glory, not merely of leading the way, in this country, to remove from the medical profession the astounding imputation of ignorance of the nature and treatment of insanity, but of having taught the whole world, through the personal and literary labours of their physician, that the time has at length come when no one shall henceforth dare to treat the unhappy lunatic, of high or low degree, as a criminal, to be punished; but as a patient, to be commiserated and gently dealt with. If Dr. Webster, as we hope and believe, shall be the means of ultimately converting our metropolitan asylums into clinical schools, like that now open at Hanwell, he will have just reason to pride himself on the accomplishment of a feat which would do honour to any name in the profession. With the view of assisting him in his endeavours, we earnestly entreat all our readers not merely to peruse his pamphlet but to recommend it to the notice of all their influential friends out of the profession. Its matter cannot be better; its style may be not a little improved by the most ordinary degree of attention to the common rules of writing.

Art. VII.—*Interesting Facts connected with the Animal Kingdom; with some Remarks on the Unity of our Species.* By John C. Hall, m.d. —*London*, 1841. 8vo, pp. 301.

The author informs us in his preface that this work contains "the substance of a course of lectures on the animal kingdom, delivered at many of the scientific institutions of the metropolis." These lectures doubtless were well suited for their purpose, and proved both useful and instructive to the hearers. In their present form, however, they contain nothing novel; nor do we find in them old knowledge put in a new form. Nevertheless, such readers as are unacquainted with Dr. Prichard's great work, and who are desirous of obtaining general views of the animal kingdom, and a compendium of the natural and physical history of man, may peruse this little volume with both pleasure and profit.

Art. VIII.—*Spinal and Nervous Diseases, Rheumatism, and Paralysis; or Cases and Observations illustrating an Improved Treatment.* By John Hey Robertson, m.d.—*Glasgow*, 1842. 8vo, pp. 119.

Sometime since we noticed a previous work of Dr. Robertson, along with several other treatises on spinal affections. We then called attention to the efficacy of dry cupping in some of these cases, and approved of the mode of practising it which Dr. Robertson describes, (Br. & For. Med. Rev. No. XXIII. p. 183.) In the work before us, which the author informs us is a continuation of the former, the same practice is advocated, and a variety of cases illustrative of its efficacy are adduced. None of these which we have examined present any points of such novelty as to induce us to quote them at length. We are disposed to believe that many cases of rheumatism and the majority of cases of paralysis will be but very partially relieved by *dry* cupping alone; and that not a few of the former would be only aggravated by that treatment. Certain cases compounded of neuralgia and rheumatism, and cases of muscular rheumatism, may be relieved by this practice; but we apprehend that in cases of acute articular rheumatism, dry cupping would be worse than useless. In short, *dry* cupping is contra-indicated, wherever inflammatory action has prevailed, and where congestion, not simply passive but active, and accompanied with positive dilatation of the capillaries, has taken place. In such cases, if cupping-glasses are at all to be used, the armed ones must be selected. We observe, indeed, that in some cases, Dr. Robertson allows a few drops of blood to be drawn by the cups, after having first applied these in the dry mode.

Dr. Robertson has ventured, in the present volume, on the subject of "the theory of the remedial action of dry cupping," a task, which, considering how he has executed it, we think he might have declined with advantage to his own reputation. "The proper time," we are told (p. 89), "to keep a glass on is from 15 to 45 seconds. .... If permitted to remain on for 10 or 15 minutes, considerable pain and even swelling will be the result."

Without retracting our former partial approval* of Dr. Robertson's earlier treatise, we must express our conviction that the apparent success of the author in many alleged cases of rheumatism and paralysis, detailed in the present volume, must be accounted for from errors of diagnosis.

In a literary point of view, the work is extremely defective, and has too much the air of having been designed for the general rather than for the professional reader. As a volume of medical reports, it is also very deficient. Thus in case 24, we are told that along with cupping, "internal medicine was prescribed!" What are we to understand by a phrase so unbusiness-like as this? May we not legitimately conjecture that this anonymous "internal medicine" may have had as great a share in the cure as the dry cupping, or perhaps a greater? Dr. Robertson, at least, has no right to call us uncharitable for forming such a conjecture.

* We cannot refrain from here noticing a species of dishonesty which certain publishers practise and authors permit; that, namely, of making garbled extracts from notices in reviews. In our remarks on Mr. R.'s former work, we say—"it is not a bad book, but it is not needed:" of which observation Mr. R. or his publisher quotes the first clause and leaves out the last, and continues the quotation as if nothing had been omitted. This is indefensible, and it is not the only misquotation.

Art. IX.—*The Practice of Medicine; or a Treatise on Special Pathology and Therapeutics.* By Robley Dunglison, m.d., Professor of the Institutes of Medicine in Jefferson College.—*Philadelphia*, 1842. Two Vols. 8vo, pp. 572, 750.

The singular success that has attended Dr. Dunglison's various elementary works might, of itself, be sufficient excuse for his publishing in America a new treatise on practical medicine. He has, however, a still better reason in the fact so well stated in his preface, that "the improvements and modifications incessantly taking place in the departments of pathology and therapeutics, render it advisable, from time to time, to incorporate them, so as to furnish those, to whom the different general treatises, monographs, and journals are not accessible, with the means of appreciating their existing condition."

In the volumes before us Dr. Dunglison has proved that his acquaintance with the present facts and doctrines, wheresoever originating, is most extensive and intimate; and the judgment, skill, and impartiality with which the materials of the work have been collated, weighed, arranged, and exposed, are strikingly manifested in every chapter. The author may truly say that "he is not conscious of possessing any exclusive opinions; and has endeavoured to be essentially eclectic; neither is he aware of having any undue prejudices." The consequence of this is that the different parts of the work bear a more equable proportion to one another than is apt to be the case with treatises compiled by writers of great originality, real or imagined. Great care is everywhere taken to indicate the sources of information; and under the head of treatment formulæ of the most appropriate remedies are invariably introduced. In conclusion, we congratulate the students and junior practitioners of America, on possessing in the present volumes a work of standard merit to which they may confidently recur in their doubts and difficulties.

Art. X.—*The Anatomist's Vade Mecum; a System of Human Anatomy.* By Erasmus Wilson. Second Edition.—*London*, 1842. 8vo, pp. 595. With 167 Illustrations by Bagg.

On a former occasion (Br. & For. Med. Rev. vol. X. p. 547), we noticed with high praise, on its first publication, this singularly beautiful and excellent work. The new edition calls for the repetition of our encomiums, and with interest, inasmuch as all the old merits are enhanced by cognate novelties both of text and illustration. "In the present edition," says Mr. Wilson, "I have recorded the investigations of Mr. Bowman, on the minute anatomy of muscular fibre; of Mr. Nasmyth, on the development of the epithelium; of Mr. Curling, on the descent of the testis. I have also contributed some original researches which I have myself made on the minute structures of bone." Additions of importance are also made to the chapters on the ligaments, muscles, nervous system, organs of sense and viscera; and the number of woodcuts is increased from 150 to 167.

PART THIRD.

Selections from the British and Foreign Journals.

I. THE FOREIGN JOURNALS.

ANATOMY AND PHYSIOLOGY.

Physiological results of Extirpation of the Salivary Glands. By Dr. BUDGE.

LITMUS paper, introduced into the mouths of a considerable number of rabbits, was changed into a deep blue colour. The same was the result in the case of dogs and cats, without any exception. A cat was deprived of food during two days and half, and a dog during one day; yet in both cases was the above change in the litmus paper not the less marked. A rabbit and cat had both their nervi vagi cut across: in both animals, till the moment of death, an alkaline reaction of the saliva was manifested. The cat lived till the fourth day. A dog had both its parotid, both its submaxillary, and both its sublingual glands extirpated; yet, to the astonishment of the author, the reaction was still alkaline in as great a degree as before. The glands themselves, after being washed, so as to free them from all traces of blood, were cut into and tested: the reaction was alkaline, but not in so great a degree as when the paper was introduced into the mouth. The animal quite recovered, and during the four weeks which it was permitted to survive, litmus paper introduced into the mouth, was always tinged blue. It was killed, and on examination a small quantity of food was found in its stomach. One bit of litmus paper laid on the stomach remained unaltered in colour; another piece became slightly reddened. The acid reaction seemed less than usual; but the author adds, that in dogs whose glands had not been extirpated he has often noticed very faint traces of acid. The results in a cat, which survived the operation four weeks without the smallest apparent injury, were nearly identical.

From these experiments the author infers that the spleen is not the only gland which can be extirpated without destruction of life. Yet no one can suppose that the salivary glands are useless. We must therefore conjecture that certain glands supplement each other; and that in the case of the removal of the salivary glands the pancreas, perhaps, eliminates the fluid which these glands usually do. It is known for certain that even the urinary secretion cannot remain in the blood when the kidneys are extirpated, and that the stomach endeavours in this case to eliminate, and *does* eliminate, genuine urine, though not, of course, in quantity sufficient for the purposes of life.

Although, according to experiments and observations on the human subject, the saliva, after eating, manifests an alkaline reaction, yet section of the vagus in brutes produces no change. The sympathy between parts supplied by the vagus and those supplied by the trigemini cannot here be taken into consideration. It is possible that this sympathy between the stomach and salivary glands may take place through the spinal branches received by the parotid.

Medicinische Zeitung, No. xviii. *Mai* 4, 1842.

On the Nerves of the Hard Palate. By Professor Bochdalek, of Prague.

As soon as the branches of the posterior palatine nerve have passed through their foramen they separate. Three or four small branches go backwards and inwards to the glands of the soft palate; others somewhat larger pass outwards to the gum by the last teeth; and the largest fasciculus, composed of five or six branches, goes forwards, gradually diverging and forming a kind of network over the whole arch of the palate. Its outer margin lies in the gums of all the teeth, and the inner forms the most varied plexuses at the middle line, with corresponding branches from the nerve of the other side. The number of filaments into which these five or six branches divide is indeed surprising. They lie even in several layers one over another, and form close networks, so that the sum of the branches seems to surpass by very far that of the trunks. Some of their filaments pass between the teeth to the anterior part of the gum, and there unite with the terminal branches of the supra-maxillary nerve which penetrate the anterior part of the alveolar process. But innumerable branches are lost in the glands of the hard palate and in their acini.

The author suggests that the purposes served by these nerves are, to give the energy necessary to so much reproduction as is constantly going on in the hard palate, to give sensibility to the mucous membrane of the lower part of the nasal cavities, and perhaps to add filaments to those of the superior maxillary nerve in the substance of the jaw. But higher uses he thinks they have in rendering the hard palate a delicate organ of touch: and he urges it as evidence for Panizza's view of the glosso-pharyngeal nerve being the exclusive nerve of taste, that its filaments are sent to the soft palate in which there is a distinct sense of taste; while the hard palate, which has no such sensibility, is supplied by the fifth nerve alone.

Medicinische Jahrbücher des K. K. Oesterr. States. Jan. 1842.

On the Anterior Columns of the Spinal Cord. By Dr. Stilling, of Cassel.

In frogs, the division of the white substance of the anterior part of the spinal cord, if the gray central substance be not injured, does not prevent the influence of the will upon the muscles which are supplied with nerves from below the wounded part. For a time the frog remains dull and as if asphyxiated; but he soon begins to move, and at length succeeds in obtaining sufficient power over his hind legs to leap for some distance with them.

Van Deen had already shown that sensitive impressions are conveyed through the posterior gray matter; and these seem to prove that the influence of the will is conveyed through the anterior part of the same substance; for if the incision be carried through the latter, all power of voluntary motion is lost.

Oppenheim's Zeitschrift für die gesammte Medicin. Januar, 1842.

On the Fatty Substance of Milk. By M. de Romane.

The author, in a memoir read at the Institute on the 25th of April, asserts, that in the formation of butter by churning there is no chemical change in the fatty constituent of milk; but that by the constant agitation of the fine membranous cells in which the particles of the butter, composing the milk-globules, are contained are broken, and the particles themselves are thus allowed to come into contact, and adhere in a uniform mass. Butter-milk, he adds, owes its opalescent appearance to the particles of the envelopes of the globules, which are suspended in it in vast numbers. He believes that this construction of the globules is adapted to the nutrition of the young animal; the membranes serving to shield the particles of butter till they have arrived in the stomach.

Gazette Médicale. Avril 30, 1842.

Closure of the Nasal Fossæ without "Nasillement."

There is at present under M. Lisfranc a man, in whom, in consequence of injury producing necrosis of some of the bones of the face, the whole of the soft palate is firmly adherent to the back of the pharynx, so that there is on this side no posterior aperture of the nares. Yet he does not "speak through his nose." And when the aperture which remains from the pharynx into the nose is carefully closed with a plug, his voice still remains unaffected.

Bulletin Gén. de Thérapeutique. Mars, 1842.

On the Origin, Mode of Formation, and End of the Blood Globules. By M. Donne.

In this memoir the author maintains that the white globules of the blood are formed by the aggregation of chyle-globules, and are intermediate in development between the latter and the true blood-globules. He adds two novelties: 1st, that milk-globules injected into the blood undergo the same changes as those of the chyle, and are like them converted into blood-globules; and 2d, that the blood-globules, after a certain period of existence, are dissolved into the liquor sanguinis.

Gazette Médicale. Mars 12, 1842.

On the Cause of the first Respiration after Birth. By Dr. Julius Budge, of Bonn.

The experiments detailed in this paper hardly merit notice except for the purpose of expressing a thorough disgust at their brutality and uselessness. It has been long known that division of the vagi nerves renders the respiration slower: and it is concluded that through them the impressions are conveyed from the lungs to the medulla oblongata, which, being reflected, excite the respiratory movements. But Dr. M. Hall has made it probable that the first respiration is due to a reflection of the impression produced by the cold air upon the skin of the new-born animal; and it has been suggested that after birth also the skin is one of the recipients of the centripetal impressions on which respiration depends. To prove this Dr. Budge actually removed daily a considerable portion of the skin from a rabbit, two crows, and several frogs. The experiments on the frogs were all useless; nothing, he admits, could be deduced from them. In the crows and the rabbit—who survived till, by several instalments, they were almost completely flayed—the respiration was generally, but not always, slower than is natural; the flaying seemed to have an effect somewhat like that of dividing the vagi nerves.

And now, who thinks it more probable than it was before, that impressions on the skin excite the movements of respiration? Who but he would forget that the sensitive nervous filaments exist as well in the raw skin which his cruelty exposes, as in that which he removes?

The details of the experiments are to be found in

Casper's Wochenschrift. Marz 19th, 26th, 1842.

On Apoplexia Intermeningea. By Dr. Joseph Engel.

The author remarks, that the sanguineous extravasation in this case, although it becomes in the course of time a firm, membrane-like substance, of a rusty hue, never presents the slightest organization, in which it differs from the true exudation, although no proper physiological explanation of this difference has been given.

Oesterr. Medicinische Wochenschrift, No. ix. *Feb.* 26, 1842.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

On the Typhus which prevailed at Rheims in 1833 *and* 1840. By M. Landouzy, Professor at the Medical School at Rheims.

The object of M. Landouzy is to examine into the analogies between the typhoid fever of France and the typhus which prevails in England.

His observations were made in the prison at Rheims, where fever broke out on October 1st, 1839, and continued to prevail until June in the following year, during which time 138 persons were attacked by the disease, of whom 17 died. Of these persons 103 were prisoners, 35 were physicians or attendants on the sick, of whom 9 died; whence it appears, that the disease proved more fatal to those who, coming from without, contracted it by contagion than to such as were seized directly by it in prison. It is remarkable that the epidemic was entirely confined to the prisoners on the untried side, while the convicted criminals entirely escaped. The prison which is constructed to hold from 80 to 100, usually contains 130 or 140, and in August and September, 1839, there were in it as many as 180 prisoners. The unusual crowding, too, during the autumn months was entirely confined to the untried prisoners, to whom likewise the fever was limited.

The symptoms, which are carefully detailed, do not differ from those observed in the recent epidemic in London. Nine tenths of the patients presented some degree of stupor; perfect coma, however, existed only in one case out of twelve. Delirium occurred in every case, but it was of a noisy character only in one case out of ten, and M. Landouzy thinks that it is always milder and less boisterous than in the *fièvre typhöide*. A petechial eruption was observed in every case but one. It usually appeared from the fourth to the fifth day, and disappeared from the tenth to the eighteenth. These spots did not disappear under pressure, and were abundant and confluent in proportion to the severity of the disease. Besides the petechiæ, lenticular spots of a rosy red, the spots of typhoid fever, existed in many instances. This rosy exanthema was always incomparably less abundant than the petechial eruption and was exclusively confined to the chest or the upper third of the abdomen, while the latter extended over nearly the whole body. They differed likewise in the date of their appearance, the petechiæ often showing themselves from the very onset of the disease, while the red spots were never visible before the tenth or twelfth day. Diarrhœa was present only in four cases, but sibilus was heard throughout the lungs in almost every instance.

The contagious nature of the disease was placed beyond all doubt by the circumstance that 35 of the attendants were attacked by the disease.

Of the 17 patients who died, the bodies of 6 only were examined; but in all the lesions found in the intestines were precisely those which are characteristic of the *fièvre typhöide*. Nothing particular was observed in any of the other organs, nor were any of those superficial erosions of the brain noticed which M. Piednagel and M. Louis mention, and which the former says are always met with when well marked cerebral symptoms have existed during the course of the fever.

The writer next examines the question of the identity or non-identity of the typhus of Rheims with the typhoid fever, and concludes—

That if, in all future epidemics of camp, jail, or hospital typhus, there should be found, as in the fever at Rheims, a complete absence of lesion of the spleen, as well as great difference among the functional disturbances which constitute the symptomatological character of typhoid fever, we should not be warranted in regarding typhus and dothenteritis more than as analogous diseases. If, on the other hand, different symptoms should be wanting in different epidemics, the conclusion would be warranted, that the two diseases are identical, and that their differences are owing to peculiarities in the epidemic constitution under which they arose.

In the present state of the question, M. Landouzy concludes that the jail typhus of Rheims and the typhoid fever present too close a resemblance not to be regarded as analogous, while their differences are too numerous to warrant the opinion of their being identical.

Archives Générales de Médecine. Jan. et Mars, 1842.

Statistical Researches into the Etiology of Pulmonary Phthisis.
By Dr. BRIQUET, of the Hôpital Cochin.

THIS paper is founded on an investigation into various particulars connected with the history of 109 phthisical patients in whom the disease was far advanced, and likewise on data furnished by all the deaths from phthisis in the hospital between January 1st, 1838, and January 1st, 1841, being 182 in number.

The conclusions (for which only we have space,) at which M. Briquet arrives are:

1. That during the past three years one tenth more of men than of women have been received into the Hôpital Cochin affected with phthisis: a result directly contrary to those obtained by MM. Lombard and Louis.
2. In at least a third of the patients phthisis was distinctly hereditary, and predisposition to the disease seemed more frequently to come from the father than the mother.
3. No immunity from the disease is afforded by the circumstance of being born of parents who are natives of the country, or by being brought up in the country.
4. Tall stature, a slender frame, an ill formed chest, and convexity from the root to the point of the nails are the only external characteristics of phthisical diathesis.
5. It occurred very seldom that the circumference of the upper part of the chest was less than that of the lower part: a fact directly contrary to the assertion of M. Hertz.
6. Those callings in the pursuit of which there is discomfort, want of exercise and of pure air present a greater number of phthisical persons than is to be found among those who pursue different occupations.
7. A third of these patients were more subject to catarrh than other persons, and were more sensible of cold.
8. In three fifths of the patients phthisis developed itself between twenty and thirty years of age, but more than two thirds of those whose parents had suffered from consumption became phthisical before their thirtieth year; while, of those whose parents had not been healthy, half did not show symptoms of phthisis till after thirty.
9. In four fifths of the patients there existed predisposition to phthisis, and in three fifths this predisposition was acquired.
10. Cold is the most powerful cause of the acquired predisposition; next to which are misery, privation, and distress of mind.
11. Phthisis is most frequent in cold seasons, and when there are many variations in the atmosphere.
12. Four tenths of the patients had not been exposed to the influence of any occasional cause of phthisis, but in most there existed a strong predisposition to the disease.

Five tenths had been exposed to and suffered greatly from some exciting cause, and this cause was in almost every instance cold and damp.

Revue Médicale. Feb. 1842.

On a peculiar Alteration of the Cerebral Substance.
By M. MAX. DURAND FARDEL.

M. FARDEL has occasionally observed in the white substance of the hemispheres of the brain vascular canals, which present, on a section being made, an appearance precisely similar to that with which anatomists are familiar at

those parts which are called the perforated plates of the brain. This condition M. Fardel regards as the result of a pathological process, and purposes to distinguish it by the name of "sieve-like condition of the brain,"—*état criblé du cerveau.*

These little holes are usually surrounded by perfectly healthy cerebral substance; they are circular, with well-defined edges scattered irregularly through the brain, varying in diameter, and unchanged in form by a stream of water. If water is allowed to flow on them for any time the cerebral substance is gradually washed away, and the little holes are seen to have been the artificial openings of canals, each of which contained a vessel.

The alteration is regarded by M. Fardel as the result of a general dilatation of the vessels of the brain produced by frequent congestion. Dilatation of the blood-vessels is by no means a rare appearance in the brains of old persons. It is in general especially-well marked in the corpora striata, where small canals are frequently seen, each of which contains a vessel that, being empty of blood, appears disproportionately small in comparison with the canal wherein it runs. This state is not in general associated with any alteration of the cerebral substance, other than an appearance of dilatation of the vessels of the hemispheres: congestion of the brain, of necessity, causes a temporary dilatation of the vessels of the organ; its frequent recurrence must compress the brain around each vessel and form in its substance those canals which are so evident when the vessels are empty and collapsed after death. The sieve-like condition of the brain is sometimes found unassociated with any other lesion; at other times, however, it coexists with the various forms of softening of the brain, and especially with that general *ramollissement* of the cortical layer of the convolutions which M. Calmeil has described as peculiar to the general paralysis of the insane, with induration of the brain, &c. Evidence exists to prove that the connexion between the *état criblé* of the brain and other lesions with which it is associated is not merely accidental; thus, in one instance in which idiocy, attended with occasional attacks of mania, followed a blow on the head, induration of the brain and the *sieve-like condition* were found confined to that hemisphere of the cerebrum on which the injury had been inflicted. It is not unusual in cases of chronic softening of the brain to meet with this sieve-like appearance for some distance around the softened part, betokening the previous existence of dilatation of the vessels. In the same brain, too, M. Fardel has met with recent *ramollissement* and injection of the vessels and old *ramollissement* with the *état criblé*.

From his observations, many of which are detailed at length, the writer concludes that,

The *état criblé* of the brain is produced by the presence of a great number of small canals, perforated in the cerebral tissue, each containing a little vessel, to the dilatation of which their formation is doubtless owing. These canals, the existence of which is normal in some parts of the periphery of the brain, appear in a rudimentary state without being necessarily morbid in some persons in advanced life.

Their usual seat is in the cerebral hemispheres, especially beneath the convolutions; but they are likewise met with in the cerebral protuberance, and in the medulla oblongata.

Though somewhat different in appearance, the little cavities so often seen in the corpora striata are probably of a similar nature.

The general or partial dilatation of a great number of the vessels of the brain appears to be owing to chronic sanguineous congestion, or to the frequent recurrence of congestion of the organ.

This opinion seems sufficiently warranted by the phenomena observed during life, as well as by the alterations which are found after death to coexist with the *état criblé* of the brain.

Twice this condition existed uncombined with any other appreciable lesion of the brain. In one of these cases there was simple dementia, in the other, dementia with general paralysis.

It is found associated with superficial *ramollissement* of the convolutions in insane persons affected with general paralysis, with general or partial induration of the brain, and in the centre of or around portions of softened brain.

This state probably existed but escaped notice in cases where grave cerebral symptoms were not found to have given rise to any lesion appreciable after death.

Gazette Médicale. Jan. 8, 15, 1842.

Peculiar Matter secreted on the Surface of the Hands of a Gouty Person after severe attacks of Gout. By Dr. Petit.

The patient who furnished this secretion was fifty-six years old, of a strong constitution, and addicted to good living. He had been subject to gout ever since his twenty-fourth year, and during the attacks of gout his urine frequently deposited a red sediment. After severe attacks he had observed a tenacious white matter form on his hands. Four grains and half of this substance, examined under the microscope, presented a number of transparent crystals. By chemical analysis it was ascertained to be composed of albumen in large quantity—about four fifths, of lactic and phosphoric acid, chloride of soda and phosphate of lime, and evident traces of urate of soda.

Gazette des Hôpitaux. Feb. 22, 1842. (*From the Journal de Pharmacie.*)

Singular case of Suppuration of the Arachnoid. By Dr. Wagner, of Vienna.

An infantry soldier in the Austrian service, of excellent conduct, and who had been the subject of no disease during the seven years of his military life, complained, on the 17th of November, 1839, of an affection resembling gastritis, for which he took on the following day a saline mixture in large doses, and derived some benefit therefrom; yet betook himself to bed at mid-day, (November 18,) and after sleeping for one hour, awoke with signs of disturbed intelligence, which gradually passed into profound unconsciousness, accompanied with strong convulsions. General and local bloodletting was fruitlessly had recourse to, the patient dying at half-past four in the morning of the 19th.

The principal morbid appearances internally were as follows: The dura mater had a parchment feel, and was not much injected; in the sinus of the falciform process there was a small quantity of blood: after removal of the dura mater, the whole surface of the cerebrum was seen to be covered with a yellow, thin pus, of such a consistence that it fell in drops from the brain, and could be easily wiped off with the back of the scalpel; there was slight depression of the brain over the ophthalmic ventricle; the arachnoid membrane was so entirely disorganized that no trace of it could be detected; the vessels of the pia mater were distended with blood, even in their minutest ramifications, and between the cerebral convolutions there was no inconsiderable quantity of pus; the cerebral substance was pappy, and studded with bloody points; the lateral ventricles were empty, but their walls completely softened; the whole lower surface of the cerebrum and the whole cerebellum were without a trace of arachnoid membrane, but covered with pus and much softened; the air-passages were slightly reddened; the *blood-vessels of the throat were greatly distended.* It may be added generally, that there were signs of disease in all or most of the other organs of the body, as the lungs, heart, liver, &c. As regards the spinal cord, the blood-vessels of its dura mater were full of blood; those of its pia mater, especially the veins, distended; the veins of the cauda equina, varicose; the spinal cord in its whole extent, especially towards the cauda equina, covered with pus, and totally without vestige of arachnoid membrane; the substance of the cord itself reduced to thin pap.

The author appends to his narration of the above curious and important case some useful practical reflections. He points out the striking absence of objective symptoms of sufficient gravity or severity to account for either the alarming physical or psychical morbid phenomena. He suggests how extremely difficult

it might have been, supposing the patient before or in the early stage of his malady to have received a slight fall, or a stroke on the head or spinal region, or had by accident taken or had administered to him any slightly deleterious substance, to determine before a legal tribunal to what cause or causes the subsequent death ought fairly to be ascribed; and it is easy to perceive, from a consideration of the case, that such an enquiry would, in the circumstances supposed, have been an exceedingly delicate one. The author recommends that in order to guide us in such emergencies, cases like the present should be most carefully investigated and kept in mind. In the above case the patient, although, according to official report, in good health up to the 17th and during the seven preceding years, was evidently in a condition of body predisposing in the highest degree to the most rapid and fatal disease. The rapid development of lesions in the other organs shows how speedily parts supplied by the peripheral nerves become disorganized when the central nervous masses themselves are attacked.

Osterreich. Medicinische Wochenschrift, No. IV. den 22 *Jänuar*, 1842.

On the employment of the Sulphate of Alum in the treatment of some forms of Angina Pharyngea. By M. Celestin Perrin.

It is by no means unusual for catarrhal affections, especially in damp situations, to leave behind them a sort of habitual chronic catarrh of the fauces. In these cases the mucous membrane is much injected, of a deep red, sometimes thickened, and the mucous follicles are very apparent and much developed. An adhesive mucus covers the parts and provokes a frequent and troublesome cough to effect its expectoration. The employment of alum gargles, of various strength, in these affections has for some years been often resorted to. M. Petréquin, of the Hôtel Dieu, has practised the insufflation of four parts of alum to one of sugar with great success; and M. Perrin has used the same means with similar results.

Encouraged by the good effects of the application in chronic cases, M. Perrin has had recourse to it in those which are acute. He mixes equal parts of alum and sugar, and blows them through a quill against the back of the pharynx. It is always necessary that the point of the quill should be even with the uvula, since otherwise the sudden descent of the velum palati may close the passage and scatter the powder on the back of the tongue, where it excites nausea and efforts at vomiting. Even in cases where the febrile symptoms run very high, the difficulty of swallowing is extreme, and the patients have on former occasions been depleted and subjected to very severe treatment, this application a few times repeated has seemed to effect a cure, and a great amelioration of the symptoms has followed its employment even once. Two cases are related in illustration, and the writer concludes by asking whether equally favorable results might be expected from this practice in cases occurring in dry and hot countries, or whether there is something peculiar in the anginas of damp and rainy climates, as Lyons, which renders them peculiarly amenable to this mode of treatment.

Bulletin Général de Thérapeutique. Mars, 1842.

On the Delirium following Acute Meningitis, and its Treatment by Cold Affusion. By M. Recamier.

In the Hôtel Dieu, under the care of M. Recamier, was a man who was the subject of chronic delirium, unaccompanied by any sign of disease of the brain or its membranes. He had already had two attacks of a similar kind, the second more severe than the first, and the third than the second, but they all yielded to depletion, which in the last instance required to be repeated several times. In the same ward was another patient in whom delirium remained after symptoms of inflammation of the brain, and depended, in M. Recamier's opinion, on some

modification in innervation produced by the disease, not on any continuance of the actual malady.

Two other similar cases are mentioned by him in which chronic delirium was removed by the employment of cold affusion. One was the case of a young lady in whom delirium existed unaccompanied by the signs of meningitis, but with irregular automatic movements of the hands, in which she crammed anything that was given her into her mouth. M. Recamier began the treatment with cold affusion of water at 68° Fahrenheit during five minutes, and gradually reduced the temperature to 50°, and prolonged the affusion for ten minutes. It was not, however, until affusion was practised at 39° that any effect was produced, but then the patient was seized with a tetanic spasm, and lost all consciousness. She was at once placed in bed, warmth was applied to the surface, and she speedily recovered, and asked for her mother, for whom she had before shown a marked aversion. Pen and paper being given her, she now wrote a perfectly sane letter, requesting her mother to visit her. The affusions were still continued at a temperature of about 64° for some days, and the patient recovered uninterruptedly.

In this case, which had followed an attack of meningitis, warm baths had been employed for a considerable time, without leading to any results. In the second case the same treatment was employed with success in a man who had become insane from spirit-drinking. In three different attacks the remedy had the effect of restoring his reason, but his intemperate habits were so deeply rooted that he eventually brought on a condition of permanent delirium, for which he was received into the Bicêtre.

Gazette des Hôpitaux. Mars 3, 1842.

Extemporaneous Production of Milk. By M. Dichost.

M. Dichost, a Russian chemist, proposed the following plan for the preservation and extemporaneous preparation of milk. He evaporates newly-drawn milk, at a very gentle heat, till it is all brought to a state of fine powder. It is then put into small glass bottles, which are completely filled and hermetically sealed, with ground glass stoppers. A small quantity of the powder thus obtained, dissolved in an appropriate quantity of water, affords on the instant a milk of very good quality. The powder will remain good for a great length of time.

Gazette des Hôpitaux. Janvier 22, 1842.

On the External Application of Croton Oil. By M. Bouchardat.

Whenever it is required to use this method of counter-irritation, M. Bouchardat strongly recommends a plaster which has been much used by M. Chomel at the Hôtel Dieu, and which is thus prepared: Four parts of diachylon-plaster are melted at a very gentle heat, and while it is half liquid one part of croton oil is mixed with it, and the mixture is then spread in a thick layer on calico. Pieces cut from this may be applied to the skin, like ordinary sticking-plaster, and quickly produce an active irritation.

Bulletin Général de Thérapeutique. Mars, 1842.

Tannin, as an Antidote for Strychnia. By Dr. Lüdicke.

The great care requisite in the employment of strychnia is well known. Of this and of the benefits of tannin when an over-dose has been administered the following case is an illustration.

A woman, thirty-seven years of age, had complained for a considerable time of great tenderness and of a wandering pain in the course of the transverse and descending colon, for which opium and various other remedies were unsuccessfully employed. Dr. Lüdicke then ordered one twenty-fourth of a grain of

nitrate of strychnia every hour. This dose, however, the patient ventured to increase, and continued to do so till she had taken half a grain of strychnia in the course of six hours. She was now suddenly seized with vertigo, and fell to the ground in a state of insensibility. A quarter of an hour afterwards she complained, though speaking with great difficulty, of having had a sensation as though her spine were being bent backwards, which had then passed off, leaving pain in the back, and tremor of the hands. The respiration was difficult, the pulse feeble and frequent, and the patient had a sensation of nausea with giddiness whenever she attempted to sit upright, and occasional vomiting. Ice was applied to the head, and half a grain of tannic acid was given every hour, at first in an effervescing mixture, on account of the sickness, afterwards in distilled water. After twelve grains had been taken the writer substituted for it a decoction of two ounces of oak bark in six ounces of water, with an ounce of syrup of cinnamon and a scruple of sulphuric æther. The symptoms of poisoning all disappeared, and with them the wandering pains from which the patient had previously suffered.

Mesner, in Dresden, recommends, as an antidote for strychnine, decoction of galls or of oak bark ; five ounces of which precipitate two grains of nitrate of strychnine. These decoctions likewise precipitate morphin, codein, brucin, veratrin, and many other vegetable alkaloids.

Medicinische Zeitung. März 16, 1842.

SURGERY.

Cure of Traumatic Tetanus by Division of the Nerve supplying the wounded part. By Professor Pecchioli, of Siena.

Two cases are related in which this method was successfully employed. The first patient was a lad 16 years old, who had received a lacerated wound of the great toe of the left foot, and in whom after severe traumatic fever the signs of tetanus set in on the third day from the reception of the injury, the wound at the same time being highly inflamed, and the whole foot painful. The branch of the internal saphenus nerve going to the toe was divided a few hours after the tetanic symptoms were established; the pain in the wound immediately ceased, the convulsions became less and gradually disappeared, the fever continued only two days longer, and the patient was ultimately discharged quite well. The second case was that of a man 50 years old, who had received a wound from an axe across the left foot, and in whom signs of tetanus appeared on the fourth day. He, as is usual in Italy, had been profusely bled for the fever that followed the injury to his foot. On the day after the tetanic symptoms were completely established, the anterior tibial nerve was divided an inch above the wound, which was acutely inflamed. The symptoms subsided more slowly than in the former case, but they at last ceased, and he also was discharged cured.

Bullettino delle Scienze Mediche. Marzo, 1841.

On Fractures of the Lower Extremity of the Radius. By M. Velpeau.

M. Velpeau entirely contradicts Dupuytren's account of the ill consequences of this injury when treated as a sprain or a dislocation, and says, that while he has seen many cases, which thus treated, have recovered with scarcely any deformity and no loss of motion, he has known many more which, though treated carefully with approved apparatus, have presented all the bad results of stiffness of the joint and defective power of the muscles. He believes that all the apparatus hitherto described do more harm than good; and says, the only useful mode of treatment is that with the dextrine bandage. After reducing the fracture he puts a compress, wet with camphorated spirit, round the wrist, and applies a dry bandage very lightly from the roots of the fingers to the middle of the arm. Over this he places graduated compresses reaching to the beginning

of the metacarpus, and then an anterior and posterior splint of moistened pasteboard, which are moulded exactly on the parts they have to cover, and descend to the roots of the fingers. A bandage wet with starch is then rolled in a double layer, from the fingers to a short distance above the elbow; and, till it has dried, all the parts are kept in their places by two long wooden splints. These last, however, are removed after six and eight hours, and the part left in its immoveable bandage supported in a sling, for from twenty to thirty days, by which time the union of the fracture is generally perfected. There are but few cases, M. Velpeau adds, in which this method of treatment is not sufficient.

Gazette des Hopitaux. Janvier 13, 1842.

Dislocation of the Thumb. By M. RADAT.

THIS was a case of dislocation of the last phalanx of the thumb backwards. A soldier was running on a moist clay soil, when he slipt, and putting out his hand to save himself, his thumb stuck in the stiff ground, and, as it was thus fixed, he fell over it. The symptoms were, considerable shortening of the thumb, retroversion of the phalanx in the direction of extension, so that it formed almost a right angle with the metacarpal bone, immobility of the joint, and a prominence on the palmar surface. It was reduced in the following manner: A piece of stout bandage was taken with a hole in its middle, through which the thumb was passed; the two ends being then carried forwards, were strapped close down upon the dislocated phalanx, and extension made by pulling them while another person held the metacarpal bone, and a third pressed in the phalanx as soon as it was drawn nearly to the level of the joint.

Gazette Médicale. Mars 12, 1842.

Artificial Anus. Closure of the Intestinal Canal by a Tumour in the left Iliac Fossa. By M. VIDAL (de Poitiers).

THIS case contains an account of the fifth operation performed by M. Amussat for the formation of a false anus. After frequent postponements of the operation because the intestinal canal did not seem to be completely closed, though the abdomen was tender and excessively distended, it was performed on the 10th day, after nothing but gas had been passed by the anus, and on the 40th from the commencement of the obstruction. The ascending colon was with some difficulty opened, and the patient from that time slowly recovered his health, though the tumour which was supposed to be carcinomatous, remained. Ten weeks after the operation he was in a satisfactory state; he had gained much of his former strength; the artificial anus gave free passage to the fæces, and the discharge of these had become less frequent.

Gazette des Hopitaux. Mars 26, 1842.

Reunion of two Fingers completely separated from the Hand and each divided into two pieces. By SIGNOR DELLA FANTERIA.

A GIRL, fourteen years old, was engaged with another person in some domestic occupation, when the latter accidentally let fall a knife which cut off two of her fingers below the first phalanx. The author being soon after summoned found the two pieces in some meal on which the patient's hand was resting at the time of the accident; but he discovered, to his great surprise, that each of them was divided into two portions. However, he determined to try to unite them, and having put the bits together, he kept them all in their places with sutures and strips of plaster. In a few days the adhesion was completed, and the patient ultimately recovered the entire use of her fingers.

It is necessary to mention that the authenticity of this strange case was confirmed by Professors Centofanti and Vacca.

Annali Universali di Medicina, 1841; *and Gazette Medicale, Mars* 5, 1842.

On the Nature and Treatment of Erysipelas. By M. VELPEAU.

THE main object of this paper is to recommend the use of sulphate of protoxide of iron as a local application in cases of simple erysipelas. After the failure of every other means tried in numerous cases, this was tried on forty patients, and in no case did an inflamed spot resist its influence for more than from twenty-four to forty-eight hours. Erratic erysipelas, however, often continued its course.

The modes of applying the remedy are two, namely, in a lotion of thirty grains to the pint of water, kept constantly applied on rags to the inflamed part; and in an ointment composed of one part of the salt in an impalpable powder with four parts of lard, and applied freely three times a day. The former is the more efficacious plan: the only objection to it is that it spoils the rags.

Annales de la Chirurgie. Fevrier, 1842.

On External Fistulæ of the Larynx. By M. TROUSSEAU.

THE cases here detailed are the results of inflammation of the submucous tissue of the larynx producing first ossification, and then necrosis of the cartilages. In some cases the matter which is formed makes its way to the interior of the larynx, and being discharged as fast as it is formed the disease goes steadily on till the necrosed portion is separated, and a cicatrix formed over its place. In others, however, matter forms on the outside of the larynx, and opening in the neck a fistula is produced, which either remains permanently open, or closes for a time and then reopens, and so on. Four striking cases unconnected with phthisis are related, and the main practical point deduced from them is, that when there is an external fistula with necrosis no attempts should be made to close it; for the necrosis is never superficial, but extends through the whole thickness of the ossified cartilage, and if the matter be prevented from discharging itself externally it will certainly continue to be formed, and produce much more serious mischief by pressing inwards upon the mucous membrane of the larynx, or by exciting inflammatory swelling about the glottis.

Journal des Connaissances, Méd. Chir. Mars, 1842.

Radical Cure of Reducible Inguinal Hernia. By Dr. C. HALLER.

SEVEN cases are given. The operation, as detailed in the first of these, was as follows: The patient, a man of twenty-three years, had ruptured himself in 1837. In 1840, when the operation was performed, his hernia, which was on the right side, had attained the size of a hen's egg, was soft, elastic, and contained a small knuckle of intestine. It made its appearance on the patient coughing or violently exerting himself, but receded on his assuming the horizontal position. The inguinal ring was so far enlarged as to admit the index-finger. The rectum having been emptied by a clyster, the scrotal integument was invaginated in the inguinal canal, by the index-finger of the operator; while, with the right hand, by means of the *sonde à dard* two stitches were made; the one as high as possible on the inner crus of Poupart's ligament; the other just over the angle of the two crura. A ball or cork of lint was introduced into the loop thus formed on the thread; traction was applied to one end of the thread, by which the lint-ball was carried as far as possible up the inguinal canal; the double threads were then separated, and tied down over two quills united by sticking plaster. Ice was applied to the seat of operation; the testicles were supported; the patient was ordered to bed; and put on low diet. There were slight pain and redness, but considerable swelling and hardness around the cylindrical aperture; and on the sixth day there were light febrile symptoms. As suppuration commenced from the punctures, the threads were loosened; the lint slightly withdrawn, and thus a free exit permitted to the matter. Warm applications to the part, daily ablution, and syringing of the invaginated integument, laxative clysters, constituted the treatment. The sup-

puration continued during fourteen days, and as it declined, the integument gradually subsided into the inguinal canal. On the 28th day from the operation, the patient, with the aid of an elastic band, could stand up, and move about a little. The inguinal canal was appreciably narrowed, and even violent coughing or straining did not reproduce the hernia. In nine months the patient dispensed with the truss. In two or three of the other cases the cure was only temporary, the hernia again descending and carrying the integument before it. But in several instances the operation has been successful.

Oesterr. Medicinische Jährbucher. März, 1842.

On the Cure of Spina Bifida. By M. BEYNARD.

THE patient was a day old, and had a tumour from spina bifida over the third lumbar vertebra. It was as large as a hen's egg, had a narrow base of attachment, and the skin over it was healthy. M. Beynard took two quills, through each of which a string was passed, and placed one on each side of the pedicle of the tumour, which he then constricted by gradually tightening the string till the skin became hard and rather deeply coloured. The quills were retained in their places by strips of plaster, and on the following day the tumour was livid, hard, cold, and covered with vesicles. Some fluid was let out of it till it became flaccid, and its base was more tightly constricted. On the 7th day, the pedicle being dry and hard was cut across, and the wound thus made was simply strapped: it had no communication with the spinal canal, the walls of the neck of the sac having been completely obliterated by the pressure. On the 26th day a cicatrix had formed and the cure was perfected.

Bulletin Médical de Bordeaux. Fevr. 1842

On Gilding of Surgical Instruments. By M. CHARRIERE.

THE electrotype has been applied to this purpose in Paris. A letter from M. Charriere was read at the Institute on the 21st of March, in which he says, "Having gilded by M. de Rustz's process a considerable number of surgical instruments and pieces of cutlery, I have submitted them to experiments which seem to me to merit attention. The cutting instruments which I have repeatedly tested on the dead body, have suffered no damage either in the quality of their edge or in their gilding; and the instruments for pressing have preserved all their power of resistance. I have moreover obtained a positive proof that the instruments thus gilded are not subject to rust; and this is an advantage of which the importance may be easily understood, especially for instruments which are intended to remain for some time in the body. I may add that the silver and the platinum plating, applied in the same manner, afford the same results as the gilding."

Gazette des Hôpitaux. Mars 24, 1842.

Case of Exophthalmus from Atheroma of the Orbit. By Professor ROSAS.

A MAN, aged forty-five, received in 1827 a contusion on the root of the nose; and in June 1830, a sharp blow on the inner angle of the right eye. These accidents were followed by swelling, &c. This swelling went on increasing, advancing in a direction from without inwards to the root of the nose, and pressing at the same time the eye out of the orbit and downwards. This organ was, however, neither inflamed nor swollen. Professor Rosas saw the patient for the first time in 1831; the following were the appearances presented. (It may here be noticed that prior to this, the man had been advised by several practitioners to submit to the extirpation of the eye.) The eye (as is seen in an annexed engraving) was situated low on cheek, yet not much altered either in its general figure or interior appearance and structure: but vision was entirely destroyed,

and even the sensation of light extremely imperfect. There was not much suffering; but the general health of the patient was affected. Obscure fluctuation in the site of the swelling, and the continuance of the lachrymal secretion, &c. guided Professor Rosas in his diagnosis, and led him to believe that the swelling was not owing to exostosis, or enlargement of the lachrymal gland. The operation was simple. An eye-bistoury was introduced about the middle part of the upper eyebrow, and cautiously pushed into the orbit, until resistance was experienced. Immediately there issued a dirty greenish-gray gelatinous fluid, of the consistence of syrup. This discharge continued to the extent of six ounces, and gradually as it proceeded, the eyeball receded into the orbit. Pressure with the finger was then exercised to complete the evacuation of the matter: and forthwith the patient recovered not merely an undoubted perception of light, but also the power of distinguishing the outline of near objects. The cavity was then syringed and filled with lint; cold water applications for twelve hours, purgatives, and restricted diet prescribed. The case proceeded satisfactorily, and a perfect cure was effected, though a slight fistulous discharge seems to have taken place from the place of incision for five months. This closed ultimately, and vision was perfectly restored.

Oesterreich. Medicinische Wochenschrift. Januar 1, 1842.

On the operation for Hare-lip. By Louis Berg-de-Varsovic.

The author says that in all the operations for hare-lip there is no prevention of the deformity subsequently arising from the existence of a notch in the upper lip at the lower end of the cicatrix. To prevent this he recommends that the edges of the fissure should be cut off, not in a straight line, but with an angle of which the shorter leg is directed upwards and outwards, and the longer at nearly a right angle to the former, upwards and inwards to the upper part of the fissure. By this mode of incision he saves an angle of the lip on each side at the lowest part of the fissure; and these when turned downwards so that their cut surfaces may be opposed will, if they are well proportioned, exactly fill the space where the notch usually exists. According to the direction, the position, and the extent of the fissure, the direction and extent of the portions excised must vary; but the same principle is applicable to all cases. The operation is performed with a sharp-pointed narrow scalpel.

Journal de Médecine et d'Histoire Naturelle, par l'Académie Méd.-Chir. de St. Petersbourg, 1841.

On Hemorrhage after Lithotomy. By M. Begin.

The author says one patient in every five or six that are cut for stone dies: and about a fourth of these die from hemorrhage, the source of which is scarcely ever to be ascertained, but which probably often proceeds from the small vessels unnaturally dilated by the irritation of the calculus. He recommends irrigation of the perineum with cold water in such cases. A flexible tube being dipped into a reservoir placed above the patient, who lies on his side with his buttocks turned over the side of the bed, some one should raise his upper buttock and direct the stream of water continually upon the perineum and the wound, and let it pass off the bed upon a sheet of oiled silk. He relates several cases of severe hemorrhages successfully treated in this manner.

Bulletin de l'Acad. Roy. de Médecine. Mars, 1842.

On the Radical Cure (so called) of Reducible Hernia.
By Dr. C. L. Sigmund, of Vienna.

The author says that he is acquainted with several cases in which operations have been performed after the method of Gerdy, Signoroni, and others, for the cure of hernia, and that he has himself been more than once induced to operate:

but he has written this paper to prove that there are scarcely any, if indeed there be any, cases in which such a proceeding should be adopted. In some of the cases which he has witnessed the patients have only preserved their lives after the greatest peril; in a few death has been the result; in all who have recovered the cure has been but temporary, and the hernia has again protruded and required the wearing of a truss. The chief dangers of the operation, in whatever manner it is performed, are peritonitis, which is not avoidable by any circumspection, and inflammation of the aponeurosis of the abdominal muscles, with suppuration spreading along them. If these do not occur, the inflammation in the inguinal canal is scarcely ever sufficient to effect a firm union of the walls of the sac, and on the least exertion the intestine is forced through the adhesions. The author concludes, therefore, that this operation must again be excluded from the practice of surgery, except in cases where its performance is urged by patients in whose cases there is no obvious reason to believe that it will be injurious.

Hufeland's Journal der praktischen Heilkunde. März, 1841.

Case of Varicose Tumour situated in the Groin simulating a strangulated Crural Hernia in an individual who died of an acute Entero-peritonitis, produced by a Cancer of the Rectum. By Dr. De Castella.

The long title of this paper tells us all that is interesting in it. The chance of having committed an important error by supposing that the patient had a strangulated hernia was not discovered till after death, for though the author had examined the inguinal region he had not observed the swelling. Probably also a sufficient means of diagnosis would have been the absence of signs of peritonitis at the tumour while they existed over the rest of the abdomen.

Gazette Médicale. Mars 12, 1842.

MIDWIFERY AND DISEASES OF WOMEN AND CHILDREN.

Occurrence of confluent Smallpox in a Child before birth, without any similar eruption appearing on the Mother, who had been vaccinated. By Dr. C Gnoli.

Rosa Galvani, 37 years old, a healthy woman who had been vaccinated successfully when an infant, was delivered of a male child on June 3, 1841. The labour was easy and the mother recovered without any bad symptoms, but the infant was an eight months' child; labour seemed to have been induced by the smallpox with the pustules of which the child was covered. At birth the child was in a comatose state, from which it was relieved by allowing some blood to flow from the umbilical cord. On the second day the pustules appeared at their height, on the fifth day maturation began, but on the sixth a black spot showed itself in the centre of each pustule and the child was attacked with febrile symptoms, subsultus, and trismus. On the seventh and eighth days this condition became aggravated, and the child died at 3 a. m. of the ninth day after birth.

When interrogated about her own health, the mother stated that about a week before delivery she felt generally ill, was feverish, lost her appetite, and suffered much from heat in the stomach, but not so severe as to make her seek for medical advice.

Bulletino delle Scienze Mediche di Bologna. Aug. and Sep. 1841.

Experiments on the best Agent to be employed in Cauterizing Ulcerations of the Cervix Uteri. By M. Lisfranc.

Two agents have hitherto been employed indifferently for this purpose, the solid nitrate of silver, and a brush dipped in the acid proto-nitrate of mercury, both which remedies have brought about the cicatrization of the ulcers. M. Lisfranc has recently instituted experiments with a view to ascertain the com-

parative merits of the two applications. From these experiments it results that cauterization with the acid nitrate of mercury has seldom excited any discharge of blood, while blood frequently flowed in greater or less quantity after the employment of nitrate of silver. From this fact the inference would be that whenever the ulceration is accompanied (as is frequently the case) with a certain degree of engorgement of the body of the uterus the nitrate of silver must not be used.

This statement of the comparative effects of the two caustics is supported by the results of seventy-two cauterizations of the cervix uteri, forty-four of which were made with the nitrate of silver, in thirty-one of which there was a discharge of blood afterwards, while that occurrence took place in three only out of twenty-eight instances in which the acid nitrate of mercury was employed.

Bulletin Générale de Thérapeutique. Feb. 28, 1842.

Indentation of the Os Frontis of a new-born Infant, produced by an exostosis between the fourth and fifth lumbar vertebra of the Mother. By Dr. Duntzer, of Cologne.

A rachitic woman, only four feet three inches high, who had passed through three difficult labours, and the conjugate diameter of whose pelvis measured only three and a quarter inches, suffered during her fourth pregnancy from dyspepsia and derangement of the hepatic functions. These symptoms were relieved, and the general health of the patient became very good, but in the last two months of pregnancy she complained of a dull pain and sense of pressure in the situation of the last lumbar vertebra. The discharge of the urine and fæces was not impeded, but the patient was prevented by the pain from lying on her back. When labour came on, the passage of the head was attended with extreme difficulty, and could be effected only by the application of the long forceps. As soon as the child was born a large depression of the frontal bone was seen, of an oval form, extending transversely from the superciliary ridge to the coronal suture, being two inches and a half long, one inch and a half broad, and one inch in depth, but not accompanied with either fissure or fracture of the bone, or with sugillation or any unusual redness of the skin. The child's health continued good, though it was in a state of asphyxia at birth. In the course of a few days the depression of the bone was lessened, in three months' time it had disappeared, and at the time of this report, the child being then six months old, the curve of the os frontis was nearly the same on both sides. On examining the mother per vaginam, an oval exostosis of the size of a pigeon's egg was detected between the fourth and fifth lumbar vertebra, and the promontory of the sacrum projected somewhat more than natural. To the case are appended references to others in which similar injuries occurred to the head of the child, and which are of considerable importance in a medico-legal point of view.

Neue Zeitschrift für Geburtskunde. 11 Bd. 3 Heft, S. 360.

Case in which the Placenta was retained for eleven weeks after the birth of a premature Child. By Dr. Schöller.

A poor woman, 37 years old, having overexerted herself, was taken in labour in the fifth month of her second pregnancy. A midwife who was summoned tore the funis in her endeavour to remove the placenta, and an accoucheur who was then sent for could not succeed in extracting it. The woman now resumed her usual occupations till she was compelled to seek medical advice by the occurrence of hemorrhage from the uterus.

Ten weeks after her miscarriage she applied to Dr. Schöller, who on making a vaginal examination found the cervix uteri thick, the os sufficiently open to admit the finger, and the uterus itself felt large and as though it contained a foreign body. The woman was ordered to remain in bed, and to take gr. x. of ergot of rye every two hours. After the administration of twelve doses pains

like those of labour came on, and were followed by the expulsion of coagula mixed with fibrous and membranous matters, and having a very offensive odour.

Dr. Schöller now fancied that the case was at an end, and supposed that this was an instance of real absorption of the placenta, but after the lapse of some days, having administered a purgative, pains came on in the abdomen and recurred periodically for some hours until a thick mass was expelled from the uterus. This mass was ascertained to be the placenta, which had not undergone the slightest decomposition, was hard, surrounded by a coating of fibrine, and shrunk to the size of half a goose's egg. On a section it presented the peculiar structure of the placenta. The patient did well.

[The case is interesting in connexion with the question which has been much debated of the occurrence or non-occurrence of absorption of the placenta in cases where it is retained for a considerable time in the uterus.]

Medicinische Zeitung. Feb. 16, 1842.

Cure of Acute Hydrocephalus by the spontaneous Discharge of Water from the Ear. By Dr. RIECKE.

A boy, two years and a half old, well nourished, robust, and with a prominent forehead, became unwell on the 8th December, 1841. The symptoms were those of an inflammatory cerebral affection, attended with fever. The disease had made considerable progress by the 14th, which was the day on which the author first saw the patient. The prognosis was the more unfavorable, inasmuch as the great fontanelle was unclosed to the extent of an inch in diameter. Every means was put in requisition—bloodletting, calomel internally, mercurial friction on the nape of the neck, blisters, cold applications to the head—without benefit. The disease went on increasing, and seemed on the 26th to have reached its height. The little patient lay powerless and stupid, the head and face flushed and hot, there was grinding of the teeth, the pupils were relaxed and insensible to light. The child had ceased to scream. The diuretic medicines resorted to latterly had failed to promote the urinary secretion. While in this state, on the 20th day of the disease, there flowed from the right ear such a quantity of pure and limpid fluid as drenched thoroughly the child's neckerchiefs. On the same evening, the patient was much relieved. By the use of diuretics, the flow of urine was now maintained, in a copious current, during many days. The coma, in which the child had been for some time, disappeared; the pupil regained motion; in six weeks the little patient was completely cured.

Another case of acute hydrocephalus is given, in which a similar discharge, but to a less extent, took place. This was on the 20th of February, 1834, and the child recovered from that attack; but subsequently on the 6th of December, 1836, had a second attack of the same disease, from which it also recovered, though on the last occasion no watery discharge from the ear took place.

Casper's Wochenschrift. No. xviii. *April* 30, 1842.

MEDICAL STATISTICS.

Results of Revaccination in the Prussian Army in 1841.

IN the year 1841, out of 44,941 men, who had been vaccinated, and were revaccinated in that year, 23,383, or 52 in 100, exhibited a genuine pock, which went duly through all its stages. The results of this year, therefore, confirm those of the several preceding ones, that the number of cases requiring *re*-vaccination is gradually on the increase. Thus in 1840, there were 48 in 100; in 1839, 46; in 1838 and 1837, 45; in 1836, 43; in 1835, 39; in 1834, 37; in 1833, 31.

Medicinische Zeitung. No. xix. *Mai* 11, 1842.

II. THE BRITISH JOURNALS.

ANATOMY AND PHYSIOLOGY.

Congenital Luxation of the Inferior Maxilla. An idiot, thirty-eight years of age, died of a pulmonary affection, the particulars of which, having no relation to the object of the paper, are not given. On entering the room where the body was lying, the author's attention was arrested by a curious deformity of the countenance, a representation of which, along with various woodcuts, illustrative of the abnormal development of the facial bones, is given in the original article. The right and left sides of the face seemed as if they did not belong to the same individual; the left being in every respect larger and more fully developed than the other. The muscles of the right side, as the masseter, pterygoid, and temporal muscles appeared small in comparison with their fellows of the opposite side, yet as regarded colour and consistence, perfectly healthy. The external lateral ligament of the lower jaw, instead of taking its natural direction downwards and backwards, was seen descending obliquely forward, to be fixed into an imperfectly developed condyle, which was not in contact with the articulating portion of the temporal bone, but was separated from it by an interval of at least a quarter of an inch. There was no inter-articular cartilage, nor cartilage of incrustation, the osseous surfaces of the joint being invested by a thick periosteum alone. The bones in general of the right side of the face were smaller than the corresponding ones of the left. The right half of the inferior maxillary bone was considerably smaller than the left, the atrophy extending forwards as far as the mental foramen, and affecting the bone in its length, breadth, and thickness; the parotideal margin was thin, and terminated above in a small curved process, directed nearly horizontally inwards and upwards. This process, which somewhat resembled in form the coracoid process of the scapula, was the only trace of a condyle, and was destitute of cartilage. There was, in short, a complete arrest of the condyle.—The *temporal bone:*—The superior longitudinal root existed, but the inferior transverse root, or the articular eminence, was not developed, there being in its place merely a flat surface destitute of cartilage. The transverse root of the zygoma not being developed, there was consequently no glenoid cavity. The right malar bone was smaller than its fellow; its zygomatic process of extraordinary length extended back as far as the tubercle of the zygoma, thus forming the entire of the zygomatic arch, which was concave externally, and convex towards the zygomatic fossa, the reverse of what obtains in the normal state. The superior maxillary bone was much smaller than its fellow; the palate did not consist of two symmetrical portions, the suture being directed from before and from the right, backwards to the left side. The motions of which the lower jaws were capable, were much more extensive than the normal ones, especially lateral motions.

It is obvious that all the other deviations, from the normal state of the bones, were consequent on the arrest of development in the transverse root of the zygoma, since it is, in fact, by the growth of this process, that the glenoid cavity is formed, and therefore that the absence of the condyle of the jaw, the atrophy of its ramus, of the superior maxillary and malar bone were consequent on and to be referred to the orignal malformation of the glenoid portion of the temporo-maxillary articulation. The resemblance of this form of the articulation to that of the rodentia must be apparent to the comparative anatomist. No analogous malformation has been recorded; it is therefore an addition to our knowledge of congenital luxation, and another instance of original articular malformation coexisting with idiotcy. Robert W. Smith, Esq., *Dublin Journal of Medical Science*, No. 62, *May*, 1842.

Functions of the Spinal Cord in Cold-blooded Animals. The object of the author of this paper is to prove that we are not justified in attributing to the reflex function of the spinal cord, many of those movements which are seen in

frogs and other cold-blooded animals after decapitation. We cannot but consider, however, that his arguments are insufficient. In the first place he states it as an established fact that no cineritious matter has been detected in the spinal cord of frogs and other reptiles. We are not aware of the authority on which this assertion rests; and we have no hesitation in saying that it is virtually untrue, for though no nervous matter having the cineritious *colour* may be present there, yet a substance having a *structural* correspondence with that which is found in the spinal cord of warm-blooded animals undoubtedly exists there. Dr. Paton adduces many experiments which appear to him to prove that not only sensation but perception and volition remain in the spinal cord of frogs after the removal of the brain and medulla oblongata,—a position in which we apprehend that few will concur with him. His arguments are based solely on the *adaptiveness* of the movements performed by the animals, which adaptiveness, being equally great in actions which are universally acknowledged to be automatic, cannot be relied on as a test. There is no doubt that it is more remarkable in the cold-blooded vertebrata than it is in the higher classes; but this corresponds with the general fact that the influence of the brain on the motor acts diminishes as we descend the scale, and at last almost ceases. Among the invertebrata we find the most perfectly *adaptive* actions performed by ganglia, in which volition cannot be imagined to exist, and in which we have no reason to suppose that sensibility resides. Is it to be imagined, for example, that when the nervous column of a centipede has been cut into several divisions, each becomes a distinct centre of consciousness and will,—an independent *ego*,—because the legs move when in contact with a hard surface? Or to go to the vegetable kingdom, is the perfect adaptiveness of the movements of the dionœa to be taken as an indication of *its* consciousness and voluntary power? Without enlarged comprehensive views on this subject, we do not think that right conclusions are likely to be attained.—Dr. PATON, *Edinburgh Medical and Surgical Journal, April,* 1842.

PHYSIOLOGY OF THE SALIVA. The following is the summary of a series of papers on the physiological uses of the saliva:

Active uses. 1. To stimulate the stomach and excite it to activity by contact. 2. To aid the digestion of food by a specific action upon the food itself. [The author here adds in note, that during the act of assisting the digestion of food, *the saliva is itself digested.*] 3. To neutralize any undue acidity in the stomach by supplying a proportionate alkali.

Passive uses. 1. To assist the sense of taste. 2. To favour the expression of the voice. 3. To clear the mucous membrane of the mouth, and to moderate thirst.—Dr. WRIGHT, *Lancet,* No. viii. *May* 21, 1842.

NORMAL DIMENSIONS OF THE HEART IN THE ADULT. The author gives an account of the plan of measurement which he adopted, which seems to have been one calculated to ensure accuracy: of upwards of 100 hearts, of which the dimensions were taken, care was had to exclude every one which exhibited any trace of organic change. We shall only give the *mean* measurements.

Of 15 male hearts, the mean *circumference* was 9 inches 27-48ths; of 17 female hearts, 8 inches 16-48ths.

The mean *length* of the male heart was 4 inches 16-48ths; of the female, 4 inches 36-48ths.

The mean *thickness* of the *left* ventricle in the male was 27-48ths of an inch; in the female, 23-48ths; of the *right* ventricle, in the male, 8-48ths of an inch; in the female, 6-48ths.

The septum ventriculorum has, in the male, a mean thickness of 22-48ths of an inch; in the female, 14-48ths.

The aortic orifice in the male has a mean circumference of 2 inches 31-48ths; in the female, 2 inches 22-48ths.

In the male, the pulmonary artery at its origin has a mean circumference of 2 inches 34-48ths; the right auriculo-ventricular orifice, 4 inches 35-48ths; the

left auriculo-ventricular orifice, 3 inches 45-48ths; and the corresponding parts in the female are relatively less.

The following are the author's deductions: 1st. The male is larger than the female heart. 2d. The length of the healthy heart to its circumference is rather less than 1 to 2. 3d. The thickness of the right ventricular parietes to the left is as 1 to 3, nearly. 4th. The pulmonary artery is slightly wider than the aorta. 5th. The right auriculo-ventricular orifice is considerably larger than the left.

Dr. Ranking, *Medical Gazette*, No. xxiv. 1842.

Can the Male Influence the Duration of the Fœtus in Utero? The Earl of Spencer has observed that cows in calf to a particular bull belonging to his lordship carry their calves about four days longer than cows in calf to any other bull, The average period of gestation of the cow his lordship finds to be 248 to 285 days; but cows in calf to the bull referred to do not bring forth till 290¼ days. His lordship has therefore proposed the query, whether the bull can exert any influence on the duration of the calf in the uterus of its mother, and whether there be any work extant in which this question is handled?—Dr. Hall, *Medical Gazette*, No. xxiii. *May* 6, 1842.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

On the Globules in Health and Disease. The first solid compound of vital affinity beyond the boundary of health, is coagulable lymph, which follows in straight succession on the effusion of serosity; and in ordinary circumstances pus is the second. It is between the one and other of the two, that tubercle seems deservedly to occupy a place. The morbid preparation is, to appearance, nearly the same for all of them; and the secretion of each respectively may go on simply from the mass yet in circulation, or be attended with ultimate congestion of blood of a permanent kind. But beyond this they differ widely. The globule of pus is an orbicular membrane, full of a more fluid matter; organizable lymph puts on the cellular and fibrous arrangement; and tubercle, with all its points of resemblance to the former in the early periods, and to coagulated albumen, and particles of fibrine in the later periods, gradually changes from a fluid to a solid friable mass, only to soften again with the solution of the texture it occupies, resolving into globular bodies of considerable consistence, and sufficiently characteristic. When secreted, so like the product of plasticity, and fully formed, resembling much that of suppuration—it yet attains to neither. It goes progressively through the course of generating, maturing, and softening tubercle; the ultimate symptom of scrofula.

In addition to some of the appearances of tubercle formerly described by the author, the following are given: In the maturer form of yellow matter, it presents the most satisfactory appearances. When compressed between the object-glasses with a drop of water, it bruises with more or less facility, assuming a milky look, and a part of it composed of globular fragments mixes with the fluid. The entire mass appears granular in the microscope, and of a light sienna colour. The borders are irregular, jagged, and broken, and many projecting fragments, about to separate themselves, are apparently attached to it by softer and more elastic matter, resembling in some measure a connecting tissue. This is not unlike what we have seen uniting the particles of fibrinous substance, which joins the fibres of muscle previously boiled. At an earlier stage, this softer material of union is less remarkable, because of the greater homogenity of the morbid matter. When, however, it approaches towards softening, the connecting tissue grows more elastic and apparent, but to disappear ultimately in leaving innumerable globular bodies.

Besides the mass of globular bodies, fibres of a particular sort are found in tuberculized lungs. They are most arranged at angles to one another, and for the most part united in bundles of about half a dozen threads. In sound parenchyma, these fibres are lax, huddled together, and the threads indistinct: in the diseased

lung, the mesh of tissues being full of tubercle, they are separated by the morbid matter. The author seems disposed to believe that the interrupted respiration of commencing phthisis may be owing to some irregular and irritable contraction of those fibres.

Most probably expectorated tubercle is very often the portions of tuberculized lung, which are less advanced in softening, but separated away by dissolution of the mass surrounding them. Their form is often lenticular, but not regularly so; they are mostly broader than thick, having thereby, at times, a wafer-looking shape, as if they were portions of the tubercle lining cavities, and we have found bodies exactly similar in extensive excavations of the lungs. They are opaque-white, yellowish-white, and grayish-white, according as it may be. Some of them are firmer in comparison to others; but generally they are of soft consistence, and easily compressed by the object-glasses of the microscope. They are evidently softer when expectorated than on their separation from the mass of lung. Again, they have more consistence than particles of ripe tubercle about to loosen altogether into globules. This is probably owing to their primitive quality on being cast off from the mass of disease. We have seen the firmest tubercles soften by prolonged maceration in water during summer; and this accords with our opinion that it is for the most part imperfectly matured tubercle, which gains the exterior in morsels mixed in sputa.

The following ingredients have been found, by the author, in the sputa of phthisical patients; shot-like granules, as large as swan-shot, sometimes of the size of a minute pea: epithelium in layers; a globular mass, composed of an aggregation of eight or a dozen saccular-looking globules, of the volume of colchicum seeds, which cannot be traced to any morbid process in particular; various crystals, one sort having the form of a wafer the size of a sixpence, with a polished scaly appearance, and a variegated yellowish and slightly brown colour; another resembling three pins placed at right angles to each other, with their points joining; again, a larger crystal of parallel prisms; vegetable matter, which most generally occurs in the form of long stalks, of the thickness of blades of grass to that of the stem of dandelion; sometimes they seem to have joints, and to terminate in an asparagus-looking head.—Dr. WATTS, *Dublin Journal of Medical Science*, No. lxi. *March*, 1842.

DIAGNOSIS AND TREATMENT OF CERTAIN CARDIAC AFFECTIONS. If we examine the heart and pericardium, when removed from the subject, seeing the former collapsed, and loosely surrounded by the latter, we cannot understand how various morbid sounds can be produced, by the motion of the one within the other. But such is not the condition of those parts in the living body; the pericardium is there firmly fixed at its apex and base, and it is tense and stretched, like the parchment of a drum; and if in this bag we have an enlarged heart, moving slowly backwards and forwards, and if the internal surface of that bag have been previously the seat of morbid deposits or effusions, punctiform or ridge-like, such conditions of the cardiac sac must cause very varying sounds emanating from the same. Pericarditic sounds may be as *loud and as prolonged as valvular sounds*, a fact hitherto scarcely sufficiently dwelt on by pathologists. Pericarditic sounds, like valvular, may be accompanied by *frémissement;* and consequently, in endeavouring to make the diagnosis between the two sets of sounds, we must keep in view the fact, that pericarditic sounds appear, to the careful ear, to issue from a more superficial source, to be more extensively diffused, and to be almost equally audible in regions of the chest very distant from each other; as for instance, under both clavicles. Pericarditic sounds, too, undergo much quicker alteration in character than valvular, and seem to be conducted by the solid parietes of the chest, while valvular sounds are chiefly propagated by the contents and parietes of the great vessels.

The author gives a case, which he considers to be "unique" in this respect, that the rheumatic inflammation seized the pericardium *before* the joints. This fact proves that physicians have been too apt to attribute pericarditis, carditis, or

endocarditis, to metastasis, a doctrine applicable to some cases, but by no means to all. Dr. G. conceives that rheumatic fever, an inflammation *sui generis, may exist without inflammation of the joints, or arthritis; as, on the contrary, arthritis may exist without rheumatic fever.* This view he founds on his observation of the fact, that he has noticed, in the same patients, a fever having all the characters of rheumatic, but which in some cases was accompanied with, and in other cases existed without, inflammation of a single joint.

In some cases of pericarditis the heart's action becomes increased in strength for many hours before any physical signs of pericarditis can be detected, and before any pain is felt in the region of the heart. In such cases, when the usually acknowledged symptoms of pericarditis are added to this already existing augmented action of the heart, the latter goes on increasing, and finally becomes excessively violent, and does not begin to decrease notably for several days after the peculiar symptoms of pericarditis have disappeared. This course may, perhaps, be explained by supposing that the muscular substance of the heart became inflamed before the pericarditis came on, and continued to be so after the pericarditis subsided.

The author gives it as his opinion that the functional derangements produced by disease of any particular part of the heart, are seldom sufficiently characteristic to enable us to make out whether the disease be situated in the auriculo-ventricular or semilunar valves, and owns that it has frequently occurred to him that all the symptoms supposed to be indicative of disease of the right side of the heart, have been occasioned by disease of the left side, and *vice versa.*

As to the motions of the heart, their derangement scarcely ever, according to Dr. Graves, indicates the seat of disease with any precision.—Dr. Graves, *Dublin Journal of Medical Science,* No. lxii. *May,* 1842.

Cancer of the Lungs and Mediastinum. In this paper a number of interesting cases, collected and original, are given. The author sums up as follows:

1. That the facility of diagnosis mainly depends on the anatomical disposition of the disease.

2. That we may divide the cases with a view to diagnosis into those in which isolated tubercles exist, with the intervening tissues healthy; those in which simple degeneration occurs without ulceration, and with ulceration; and those in which a tumour of the mediastinum exists, causing compression.

3. That the diagnosis in the first case is difficult, from our being seldom able to avail ourselves of the signs of irritation and ulceration, so important in ordinary tubercles, and the fact of the equable distribution of the disease preventing comparison.

4. That in some cases of isolated cancerous masses, the diagnosis may be founded on the same general principles as that of acute phthisis.

5. That in simple cancerous degenerations of the lung, the principal physical signs are the gradual diminution of the vesicular murmur, without râle; its ultimate extinction; and the signs of perfect solidification.

6. That the evidences of perfect solidification are better found in this disease than in any other pulmonary affection.

7. That this form of the disease may exist, simply, or in combination with empyema, and may be secondary to cancerous tumours of the mediastinum.

8. That the sides may be symmetrical in this affection, and that either dilatation or contraction of the side may occur.

9. That the mediastinum may be displaced, even though the side be contracted.

10. That under these circumstances we may have the signs of perfect solidification, accompanied by imperfect pectoriloquism, and increased vibration to the hand.

11. That the mediastinum may be displaced and the liver depressed without protrusion of the intercostal spaces.

12. That the heart may be compressed and dislocated in this form of disease.—*Hughes, Syms, Houston.*

13. That the flattening of the upper part of the chest may occur from degeneration of the upper lobe.—*Hughes.*

14. That the absence of signs of ulceration is very characteristic of this disease.

15. That we have observed these signs in but a single case, and that the phenomena, though they might be produced by other diseases causing the same physical conditions of the lung, have never before been met with.

16. That cancerous tumours of the mediastinum generally coexist with either degeneration of the lung, or isolated tubercles in its substance.

17. That they may be solid or fluid.

18. That they may coexist with cancerous infiltration of the lung, or the deposit of cancer in the bronchial tubes.

19. That they are to be recognized more by the signs of the tumour, than by those of disease of the lung.

20. That dysphagia, tracheal stridor, feebleness of one pulse, difference of respiratory murmur from pressure on the bronchial tube, displacement of the diaphragm, and dilatation of the heart, may occur in this form of the disease.

21. That a cancerous tumour may exhibit pulsation with or without bellows murmur, but that pulsation is not always attendant on it

22. That though the previous existence of external cancer may assist in diagnosis, yet that the disease may be all through internal, or the visceral precede the external cancer.

23. That the feebleness of pulsation connected with the extent of dulness may assist in distinguishing the disease from aneurism.

24. That in the advanced periods, as in aneurism, gangrene of a portion of the lung may supervene.

25 That the following symptoms are important as indicative of this disease: pain of a continued kind; a varicose state of the veins in the neck, thorax, and abdomen; œdema of one extremity; rapid formation of external tumours of a cancerous character; expectoration similar in appearance to currant jelly; resistance of symptoms to ordinary treatment.

26. That though none of the physical signs of this disease are, separately considered, peculiar to it, yet *that their combinations and modes of succession* are not seen in any other affection of the lung.—Dr. STOKES, *Dublin Journal of Medical Science,* No. lxii. *May,* 1842.

PATHOLOGY OF TYPHUS. Rokitansky first boldly announced the doctrine that the typhous product is as peculiar, and stands in the same relation to the disease producing it, as the matter of scirrhus or tubercle does to the morbid affections producing these formations; and that, in the case of typhus, this cause is a *dyscrasia,* expressing itself by a tendency to the deposition, in certain localities, more especially in the ileum, of a substance as peculiar in its nature as the matter of scirrhus or tubercle. The authors, then, in drawing this alleged analogy between tuberculosis and typhus, point out the peculiar mode and character of the tubercular deposit, and affirm that precisely similar to this, is what Rokitansky teaches to be the typhous process. In one important particular they seem to differ. Tuberculosis is a chronic, typhus a rapid disease; in the former, moreover, there are always distinct local symptoms, and seldom general fever; the contrary, in the latter. But if we could conceive a variety of tuberculosis, equally rapid in its course as typhus, presenting no obvious local symptom, and attended with violent fever, the resemblance between this disease and typhus would be obvious. Now it so happens there *is* such a disease, which has been adverted to by some of the most recent writers under the name of acute tuberculosis. One case has been witnessed by the authors, which was characterized by great prostration, dry tongue, burning skin, quick pulse, stupor amounting almost to coma, without any indication of the peculiar disease being discoverable either by general symptoms or the stethoscope. This disease generally occurs in the course of chronic phthisis, but sometimes as an independent affection. The appearances, on dissection, are an almost universal

dissemination of very small tubercles over the surface of the lungs, liver, kidneys, and generally also over the arachnoid membrane.

There are two theories as to the cause of typhus: the one of which is, that the disease is an essential one, without any local lesion to account for the symptoms, and that any appearances of local affection which present themselves are merely incidental, and neither cause nor characterize the disease. The other view is, that they are symptomatic of inflammation of the intestines, or of some other organ. Against the first view, it is urged that in some forms of the disease a lesion of the intestines, quite peculiar, is generally present; and although it is doubtful whether the disease can exist without the lesion, it is certain that the lesion never exists without the disease. This proves the lesion to be, if not the pathological cause, yet at least the anatomical symptom of the disease. On the other hand, it is urged that these local changes are not always present; that the severity of the disease does not correspond to their greater or less development, &c. Now Rokitanski explains these seeming anomalies, by holding that as the scrofulous diathesis may be present without the formation of tubercle, so typhous dyscrasia may exist with very little, or even altogether without typhous matter. The local lesion may be small or null, but the dyscrasia which the lesion indicates is great. Continental typhus is generally characterized by deposits, usually in the ileum; British typhus is more usually without local symptoms, and the authors cannot see how it is possible to separate them into distinct diseases, without the arbitrary division of two groups of morbid phenomena, much more closely resembling each other than is ordinarily seen in cases of any epidemic, as for example, scarlatina.—Drs. Drysdale and Russell, *London and Edinburgh Monthly Journal of Medical Science*, No. iv. *April*, 1842.

Antiquarian Notices of Leprosy. In this, the concluding paper on the subject, we are informed that station or rank in life was not a guarantee against the disease. It is suspected, if not certain, that the royal families both of England and Scotland had members afflicted with it. There is reason to believe that Henry IV. of England was a victim, and there is little doubt that King Robert Bruce, of Scotland, died of it, after having been for many years under its influence. Baldwin IV., king of Jerusalem, was, owing to the malady, obliged to quit the throne in his twenty-third, and perished of it in his twenty-fifth year. The male sex appears to have been the one more peculiarly subject to the disease. The affection appears also to have been clearly hereditary. As to the external exciting causes of it, these seem to have been very obscure. The Saxon habit of bathing appears to have been little imitated by the Norman conquerors; yet filth, poverty, poor diet, and wretchedness of all sorts, do not sufficiently explain the occurrence of the disease; since there are at present in Europe, and even in our own country, spots where all these conditions subsist in full force, without leprosy manifesting itself in consequence. Nor can it be explained from any varieties of temperature, climate, situation, soil, &c.; since it has been found in Sumatra, under the equator; in Iceland, within the arctic circle; in the arid plains of Arabia, and in the wet and malarious districts of Batavia and Surinam; in the interior of Africa, Hindostan, Asia Minor, and Asiatic Russia; on the sea-coast, as at Carthagena; and on the table-land of Mexico, thousands of feet above the sea-level.

It seems to be doubted, by most modern pathologists, that tubercular leprosy, as it at present exists, is of a contagious nature. When the disease is imported in the person of an infected individual, from a district where it is endemic to one where it is unknown, the malady seems to have no tendency to spread. In the Edinburgh Hospital, in 1590, two of the lepers' wives lived uninfected with their husbands, and some of the English leper hospitals served as retreats at the same time for the merely poor and for the leprous. Lepers were, in former times, regarded as defunct persons, by the civil law. Even the church took the same view of them, since the ceremonial of the burial of the dead was performed over the patient on the day on which he was consigned to the lazar-house, and separated from his healthy fellow-creatures.—Dr. J. Y. Simpson, *Edinburgh Medical and Surgical Journal*, No. cli. *April* 1, 1842.

VALVULAR DISEASES OF THE HEART. The valvular diseases may be arranged as follows:

1. Mitral valve contracted, but capable of closing.
2 „ „ incapable of closing.
3. Aortic valves contracted, capable of closing.
4. „ „ incapable of closing.
5. Combination of 2d and 4th cases.
6. „ 1st and 3d.
7. „ 1st and 4th.
8. „ 2d and 3d.

In all these the *rhythm* of the systole and diastole of the cavities remains as in the healthy state; the flow of blood and the sounds being shortened or prolonged in consequence of the valvular disease.

The following table shows the mode in which these sounds are produced, and the modifications they undergo in each case:

<table>
<tr><th></th><th>1</th><th>2</th><th>3</th><th>4</th><th>5</th><th>6</th><th>7</th><th>8</th></tr>
<tr><td></td><td colspan="4">Ventricular systole.</td><td colspan="2">Ventricular diastole.</td><td>Rest.</td><td>Auricular systole.</td></tr>
<tr><td>Healthy state.</td><td colspan="4">First sound.</td><td>Second sound.</td><td colspan="3">Silence.</td></tr>
<tr><td>1</td><td>First sound.</td><td colspan="3">Silence.</td><td>Second sound.</td><td colspan="3">Murmur.</td></tr>
<tr><td>2</td><td colspan="4">Murmur of regurgitation.</td><td>Second sound.</td><td colspan="3">Murmur.</td></tr>
<tr><td>3</td><td colspan="4">First sound, masked by murmur.</td><td>Second sound.</td><td colspan="3">Silence.</td></tr>
<tr><td>4</td><td colspan="4">First sound, masked by murmur.</td><td colspan="2">Murmur of regurgitation.</td><td colspan="2">Silence.</td></tr>
<tr><td>5</td><td colspan="6">Double murmur.</td><td colspan="2">Single murmur.</td></tr>
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<tr><td>7</td><td colspan="2">First sound, masked by murmur.</td><td colspan="2">Murmur.</td><td colspan="2">Double murmur.</td><td colspan="2">Single murmur.</td></tr>
<tr><td>8</td><td colspan="4">Double murmur.</td><td>Second sound.</td><td colspan="3">Single murmur.</td></tr>
</table>

If the time of one series of the heart's actions be divided into eight parts, the ventricular systole will occupy four, during which, in the healthy state, the first sound will be audible; the ventricular diastole two, the second sound occurring at its commencement. During the systole the blood flows into the aorta, and the mitral valve closes. During the diastole, the aortic valves having shut, the blood flows into the ventricle from the auricle. The action then ceases for one eighth of the whole time, and the remaining one eighth is occupied by the silent contraction of the auricles, the blood quietly flowing into the ventricles, and thus stimulating them to contract again. It is stated by the Committee of the British Association that the systole of the auricles is accompanied by a sound; but if present, this is so slight, that in practice it may be disregarded. In the natural state,

then, we have a silence of one fourth intervening *between the second and first sounds*, or before the latter.—Dr. Anderson, *Edin. Med. and Surg. Journ* No. cl.

Case of Hysterical Affection of the Eyes, with obstinate Closure of the Lids. About ten years ago a lady, now twenty-seven, active and healthy, was attacked, the morning following a party, with intolerance of light, pain and watering of the eyes, and then complete closure of the lids, but without spasmodic action or distortion of any kind. She was unable to open her eyelids herself, and *no force which could be employed sufficed to do so*, until she was bled to ℥viij, when they opened spontaneously; but in forty-eight hours they closed anew, to be again opened by the same means. During two years and a half the attacks recurred at irregular intervals, especially in the right eye, which never remained for more than a week unaffected; and during this period, besides venesection and arteriotomy, acupuncture of the lids, electricity, and the moxa to the vertex, were employed with success. When the eyelids were soon opened, the conjunctiva appeared natural; but when the opening was delayed, this membrane appeared flocculent and granular, and discharged a wheyish, purulent fluid. Almost every medicine in use, alteratives, tonics, antispasmodics, narcotics, have been tried, along with sea-bathing and voyages and travelling; and almost all the eminent medical men in Dublin, and several in London, have been consulted without the slightest benefit. It should have been stated that vision was perfect from the first, and still is so. The lady's circumstances are now less comfortable than formerly, and her habits more sedentary. Yet her health is good, though acupuncture does not now, as formerly, relieve. The attacks are, however, more rare.—Dr. Peebles, *Dublin Med. Press*, No. clxiv., *Feb.* 23, 1842.

Case of Hydrencephalocele. The patient was a boy of eleven days, and had, on each side of the upper part of the nose, a tumour; that on the right side of the size of a small plum, that on the left of a small almond. The tumours had a glossy appearance and a spongy feel, and could easily be reduced on pressure. When the finger was pressed upon them, there was an indistinct feeling as if the contents of the tumours retired into some cavity. The tumours were discovered the day after birth, at which time they were the size of a pea.

It was resolved to puncture one of these swellings, which was done accordingly. A puncture was first made with a sewing-needle, but not more than a drop of fluid escaping, the aperture was enlarged with a lancet, and a drachm of limpid fluid was discharged slowly, partly by means of pressure. When pressure was made over the tumour of the left side, the fluid flowed more freely from the other, indicating a communication betwixt them. When the child cried, the serum flowed in greater quantity, showing the tumours to communicate with the brain and to be influenced by the turgescence of the vessels of that gland. The operation was performed on June 18th, and, on the morning of the 26th, the child, after a slight convulsive fit, expired.

The dura-mater and pia-mater extended into the tumours, through openings situated below the nasal processes of the frontal bone. The anterior cornua of the lateral ventricles extended into and probably communicated with the cysts; at any rate, the cerebral substance was distinctly seen in them. The openings between the dura-mater lining the skull, and that portion of it extending into the cysts, were about the size of a crow-quill.

The author points out the possibility of such a case being mistaken for aneurism, by anastomosis, or for nævus, and in the present case, the real nature of the case was first suspected from the tumour being viewed by transmitted light, and seen to be translucent. The author would regard chronic hydrocephalus, hydrencephalocele, and spina bifida, as only three varieties of the same morbid condition; the only difference being one of extent between the two first, and of locality between them and the last one. He is averse to operation in all such cases, and regrets having operated in the present. In his experience, the children operated on for hydrocephalus, or spina bifida, though healthy at the time of operation, and although every precaution was taken, have derived no benefit; febrile disturbance first,

then convulsions, and finally death, have invariably ensued. He would, therefore, leave the cure to nature, the more especially as there are as many instances of natural as of artificial cure.—WM. LYON, Esq., *London and Edinburgh Journal of Medical Science*, No. v. *May*, 1842.

VIOLENT HYSTERIA IN A MAN. This was a well-marked case, characterized by the most convulsive laughter, crying, &c. His strength was such that it required seven or eight men to hold him. There was great heat over the parietal bones, over which cold water was freely applied; and the heat on this region of the skull was generated so fast, that the cold water evaporated as if thrown on a hot substance, and rose in vapour. The man's age was twenty-six; he was small, weak, and effeminate, of an excitable temperament. It is not stated, as it ought to have been, whether he be married or not.—ALFRED SMEE, Esq., F.R.S., *Medical Gazette*, No. xxvii., *March* 25, 1842.

[Another and still more remarkable case is given in the 33d No. of the same Journal (May 6, 1842), by Mr. Stanger, of Nottingham. In this case the hysteric convulsions were controlled, and a cure effected, by the threat of a red-hot poker being applied to a particular part of the back. The patient was a robust-looking man, in his eighteenth year.]

DIABETES MELLITUS: CURE (?). The patient was a man of fifty, and had been ill for a year. Had voided, during the twenty-four hours preceding the time when Mr. B. saw him, 108 ounces of straw-coloured urine of the smell and taste of honey, and of the specific gravity 1·0493. He had had cardiac palpitations during many years. Opium, quinine, and the tincture of the sesqui-chloride of iron were administered in conjunction and in a fluid form. This was on the 24th of March; and on the 10th of April a cure was effected, the urine being reduced to 42 ounces, and having lost its saccharine odour and admixture.—Mr. JOSEPH BELL, *Medical Gazette*, No. xxxii., *April* 29, 1842.

[If this was a case of genuine diabetes mellitus, we strongly suspect that Mr. B. will have occasion, at no distant date, to report another result.]

CASE OF VEGETABLE ORGANISMS OF AN UNDESCRIBED FORM, EJECTED FROM THE STOMACH. A patient, nineteen years of age, had laboured for four months under stomach complaint, and every morning, ejected from his stomach, without any effort of vomiting, a quantity of fluid, varying in volume from two thirds to a whole wash-hand basinful.

On examining the fluid ejected, which smelt like fermenting wort, Mr. Goodsir expected, if he found any vegetable form at all, to see some of the globular or moniliform algæ, which it now appears pretty certain are concomitants of some of the fermentations. What was then his astonishment to find, in the first drop examined, not the vegetables he was led to expect, but numerous individuals of a form, with which the zoologist is familiar! namely an organism, which, whether animal or vegetable, was closely allied to certain genera of baccilariæ, and much more closely to the genus gonium among the volvocinæ. The author, after making the statement in the preceding sentence, and which seems to imply some doubt as to whether the organisms were animal or vegetable, speaks in the succeeding page, of "at once recognizing them as belonging to the *vegetable* kingdom." Before giving an account of the organisms, it may here be mentioned, that the formation of the morbid product was greatly controlled, and nearly put a stop to, by the use of creosote.

The following description is drawn up from examination of the ejected fluid for a period of nearly two months: In every instance the organisms presented themselves in the form of square or slightly oblong plates. The thickness of an individual was about one eighth of the length of one of its sides. Under a moderate power the sides and angles appeared straight and well defined; but under deeper glasses, the angles were rounded, and the sides sinuous; appearances which resulted from the uncompressed forms of the component cells in their particular directions.

The flat surfaces were divided into four secondary squares by two rectilinear transparent spaces, which, passing from side to side, intersected one another in the centre like two cross garden walks. Each of the four secondary squares was again divided by similarly arranged, but more feebly developed spaces, into the four ternary squares. The sixteen ternary squares thus constituted, when examined with deeper powers, were seen to consist each of four cells, which were not separated by transparent spaces, but simply by dissepiments formed by the conjunction of the walls of contiguous cells. These sixty-four cells, of which the organism consisted, did not present in perfect individuals distinct nuclei; although in certain instances appearances presented themselves, having relation to the reproduction of the organism, and falling to be described in another part of the paper. The individual organisms were transparent and slightly yellow or brown. When carefully examined under favorable circumstances the cell-walls appeared rigid, and could be perceived passing from one flat surface to the other as dissepiments. These dissepiments, as well as the transparent spaces, were from compression of contiguity rectilinear, and all the angles right angles; but the bounding cells bulged somewhat irregularly on the edges of the organism, by reason of the freedom from pressure. These circumstances gave the whole organism the appearance of a wool-pack, or of a soft bundle bound with cord, crossing it four times at right angles, and at equal distances. From these very striking peculiarities of form, I propose for it the generic term Sarcina. Perfect individual Sarcinæ, of the species now under consideration, vary from 800 to 1000 of an inch linear, along each of their sides.

Mr. Goodsir requested Dr. Wilson to subject the ejected liquid to chemical analysis; and it was found that, differently from any other liquid obtained from the human stomach, it contained at one and the same time, lactic and acetic acid. One of these acids, then was abnormal; but which? It is probable that both acids are developed during healthy digestion. Lactic acid, free or combined, abounds in the body; but acetic acid is a much rarer constituent of animal fluids, and no doubt, lactic acid has often been mistaken for it. The quantity of acetic acid found in this case was enormous.—Mr. Goodsir, *Edin. Med. and Sur. Journ.* No. 151.

Spontaneous Perforation of the Stomach A woman, twenty-six years of age, unmarried and hitherto healthy, was seized with an acute pain at the epigastrium, while putting on her stays. Dr. Thompson found her labouring under excruciating pain, which she described as existing at a point about an inch and a half to the left of the xiphoid cartilage. Leeches, cupping-glasses, camomile, and poppy stupes, bran poultices were employed, along with a hip-bath, an aperient, and afterwards a sedative draught. The relief obtained was merely partial and temporary, and the woman died thirty-five hours from the commencement of the attack. In the outward appearance of the stomach, there was nothing to attract notice, but on opening it, its surface was seen to be generally inflamed; and on closely examining it there was found an oval opening in the mucous membrane of the lesser curvature. The submucous coat was also similarly perforated; but the opening in the serous coat was not observable on the outer surface at first sight, but was only recognized (unless very closely inspected,) by passing a probe from the interior of the stomach. It was an oblique slit; and through it about a pint of the stomachic contents had passed into the cavity of the abdomen, causing diffused peritonitis —Dr. J. B. Thompson, *Med. Gazette*, No. 29. *April* 8, 1842.

Two Cases of Glanders in the Human Subject. These two cases, occurring in a man and his wife, followed the usual course, that is, proved utterly unamenable to treatment, and issued fatally. There was no post-mortem examination in the case of the man, and that which was instituted in the woman's case showed merely the lesions usual in such cases. The reporter observes: "In neither of these cases was inoculation accounted for, and we have no reason to believe that the virus was endermically introduced into the system, and indeed the same might be said of many cases on record. I would be much inclined to

believe that constantly inhaling an atmosphere saturated with the noxious effluvium from diseased horses would, if incapable of producing the disease, at least very much dispose to the reception of the infection or contagion." The author suggests that the immediate destruction of all glandered horses should be made a business of the public authorities or police.—Dr. DUNNE, *Dublin Med. Press*, No. clxxiv., *May* 4, 1842.

SURGERY.

CASE OF ENLARGEMENT OF THE NOSE TREATED SUCCESSFULLY. The patient was a young lady, who had a peculiar enlargement of the nose, unaccompanied by pain or inconvenience, except that arising from the size. Dr. C. suspected it to be owing to deficient menstruation, for which he instituted constitutional treatment. His local application was as follows: "Taking a quantity of plaster of Paris, I made a mould of the nose, and whilst wet, I placed tapes in the plaster to secure it afterwards; the middle of one tape fastened to the mould was intended for securing it laterally by each end crossing the cheek on the same side, and tying together behind the neck; a second tape directed its course between the eyes over the centre of the os frontis, over the head, and secured to the first tape behind the neck; when sufficiently hard, the mould was removed, baked, and well seasoned with oil; when thus prepared it was replaced on the nose, and secured by the tapes so as to effect a gentle and equal pressure on the organ, the weight of the mould assisting, as it was made purposely rather thick, the lower part being left open to facilitate breathing. After wearing it in this manner a week, I found the mould much too large for the nose, and sat very loosely upon it. I was therefore certain the pressure had effected a considerable reduction in the size of the part affected: encouraged by this, a second mould was made on the reduced organ, which was accompanied with the same satisfactory results; a third, fourth, and fifth mould followed, when the nose had assumed its natural size and appearance. On comparing the last with the first mould, the contrast was very striking, and would scarcely have been believed by any person who had not witnessed the process: each mould was worn about a fortnight, with the exception of the first and last; the former about a week; the latter was advised to be worn longer, and relinquished by degrees; the constitutional treatment succeeded in effecting menstruation regularly, and in a sufficient quantity. The nose still remains its natural size. I think this plan might be applied with advantage in many cases; the effect of pressure in chronic enlargements is well known; it is only the novel way of employing it that deserves attention in this case."—Dr. CHARLES CLAY, *Lancet*, No. iii., *April* 16, 1842.

FIVE CASES OF STONE TREATED BY LITHOTRITY. The first patient, fifty-six years of age, required five sittings, seemed to have suffered very little from the operation, and was cured. The passage of the fragments of stone subsequent to the operations was not followed by any irritation. The second patient, aged thirty, required three sittings. In this case the pain attending the operation was trifling; but the first manipulation was followed by feverishness and inflammation of one testicle, which rendered a postponement of further proceedings necessary, from the 1st to the 18th of June; cure. In the third case, which was that of a female whose age is not stated, three sittings were required; cure. In the fourth case, that of a man sixty-three years of age, who had a stone measuring seven lines, the operation, performed on the 12th of October, was accompanied with little pain; but on the 16th the urine deposited a considerable quantity of viscid, transparent mucus, which gradually disappeared under the use of the oil of cubebs. The operation was repeated on the 7th and 25th of November, and the 5th, 19th, and 26th of December, without being again followed by catarrh of the bladder, or any other inconvenience; cure. But a year after, symptoms of stone reappeared, which were again removed after two lithotritical sittings; and the man being examined by three hospital surgeons, was found free from stone. The fifth was the only unsuccessful case. After lithotrity had been performed and fragments of stone removed, the bladder became so irritable as to make lithotomy

necessary. This was performed, and three calculi as large as hazel-nuts, and numerous small, soft concretions, were removed. But fresh symptoms of stone have since manifested themselves. This patient was a man of seventy-two, but healthy.—Mr. Teale, *Prov. Med. and Surg. Journal*, No. xxiv., *March* 12, 1842.

Operation for Hare-lip performed on a Child four days old. There was nothing remarkable connected with the operation, except the early age at which it was performed. The parts seem, moreover, to have united with unusual rapidity and completeness The author's reason for operating so soon was, that the mother was disagreeably affected by the appearance of the hare-lip in her child, and wished it removed as speedily as possible; but it is open for consideration whether, in every case, it may not be better to operate at a much earlier period than is usually done in such cases.—Dr. Dawson, *Dublin Med. Press*, No. clxviii., *March* 23, 1842.

Dry Gangrene of the Arm. John Silver, aged fifty-one, a stout, muscular man, whose constitution had become much impaired by free living, and constantly driving a night-coach to and from Exeter to this town, felt considerable pain in his left arm whilst performing the journey on a cold, rough night in the month of February, 1813. On the following morning the hand was found to be dark-coloured, cold, and shrivelled, which appearance, on further examination, extended to the elbow. He consulted an old practitioner, who recommended fomentations with mustard and horse radish, bark, wine, and brandy. These remedies produced no good effect, and the disease proceeded until it had nearly reached the shoulder-joint. At this time I saw him, and proposed amputation as the only thing to be done. It was refused, but ultimately the limb was removed immediately below the joint. The pulsation of the brachial artery was so feeble that it could scarcely be felt before the operation; and when the vessel was divided it bled very languidly, the discharge being so trifling that it seemed scarcely necessary to apply a ligature. The wound slowly, but never completely, healed, and he recovered sufficiently to enable him to go about for some months, when he died.

The progress of this disease resembled gangrena senilis, many examples of which I have seen affecting the lower extremity in old people; but this is the only case in which I have observed the upper extremity affected. As no post-mortem examination could be obtained, it was impossible to ascertain the condition of the blood-vessels, which would have been very desirable."—Jonathan Toogood, Esq., *Prov. Med. and Surg. Journal*, No. xxiv., *March* 12, 1842.

Air-douche of the Eustachian Tube. This paper is violently dissuasive of air-injections of the Eustachian tube. The author seems to think that the injection is not actually effected in a great many cases in which it is pretended by the operator and imagined by the patient to have been done. Among the disastrous effects which have resulted from attempts to perform the operation, or from its actual performance, he enumerates inflammation of the throat and tube itself, as well as of the tympanum, so severe as sometimes to terminate in suppuration; emphysema caused by laceration of the mucous membrane of the Eustachian tube or posterior nares; rupture of the membranum tympani; deliquium so protracted as to threaten life; death. These last two formidable consequences the author considers are to be referred to the *direct pressure of air upon the brain*, in consequence of the enormous force sometimes exerted in injecting with air a narrowed Eustachian tube.

The author has found remarkable good effects from the use of iodine in conjunction with sarsaparilla, in the swelled tonsils succeeding the inflammatory sore throat of scarlatina, by which deafness is so often induced.—Anonymous, *Medical Gazette*, No. xxxiii., *May* 6, 1842.

Operation for Artificial Anus. The object of this paper is to show the superiority of Amussat's method of operation over that of Callisen, since by the latter the peritoneum is twice opened; by the former, that membrane is left un-

touched. The author details a very interesting case of Amussat's, at which he himself was present. It occurred on the 20th of January last. A child, within a few hours of its birth, was taken to M. Larrey on account of some impediment to the exit of the fæces. A cul de sac, about an inch and a half from the anus, was detected. M Larrey having tried without success the introduction of the catheter, plunged a trocar into the cul de sac (as was supposed), but no meconium followed the withdrawal of the instrument. The child was brought to Amussat; it was now forty-eight hours old. The abdomen was hard and distended, and the child's face dusky. It had vomited frequently. Dr. Amussat, on examination, was led to believe that about two inches from the anus there existed an interruption of the rectum, the caliber of the gut being at this point totally obliterated; and he was of opinion that it was totally impracticable to form an artificial anus either in the anal or coccygeal regions, but that an incision into the colon, in the left lumbar region, afforded the only chance of life to the child. Amussat's mode of operation has already been described in this Journal.

The case, up till the time of report (four weeks from the date of the operation), has done well. Tepid injections are administered every twenty-four hours, and the fæces escape readily through the artificial anus. A small tent is kept constantly in the aperture, which prevents the closure of it.—Mr. PARROTT, *Medical Gazette*, No. xxvii., *March* 25, 1842.

OPERATION FOR ARTIFICIAL ANUS. "Amussat," remarks Mr. Teale, "has shown that the failure of the operation on the dead subject was owing to the intestine being empty, and that in such cases as require the formation of an artificial anus, the colon is greatly distended, in which condition the layers of the peritoneum, forming its imperfect mesentery, are so far separated as to admit of the intestine being reached without opening the peritoneum. He has farther introduced an important modification of the operation of Callisen, by adopting the transverse instead of the vertical incision of the muscles." The operation, as performed by Mr. Teale, was as follows. The child seems to have died the sixth day after the operation: "The patient being placed upon a table, on the right side, with the face and abdomen inclining downwards, Mr. Teale made an incision through the integuments four inches and a half in length, extending forwards from the outer edge of the sacro-lumbalis and longissimus dorsi muscles, midway between the lower ribs and the crest of the ilium, nearly parallel to the latter. The different layers of aponeurosis and muscle having been divided in succession upon a director, a considerable mass of fat was forcibly protruded at the wound. By the finger the packets of fat were detached, and the posterior surface of the colon was very readily felt, extremely tense and elastic, and was soon exposed to view, its pale blue tint and translucent aspect contrasting strongly with the opaque white fat in the neighbourhood. Two temporary ligatures were passed through the muscular and mucous coats of the colon, and a considerable quantity of air escaped through the punctures, which so far diminished the tension of the intestine, that Mr. Teale was enabled to pinch up a fold of it, and to open it with the scalpel, after which there was a profuse discharge of air and liquid fæces. The opening in the intestine was further dilated in a vertical direction by a probe-pointed bistoury to such an extent as to allow of the introduction of three fingers. The edges of the intestinal aperture were then fixed to the external wound by four points of suture, and the wound of the integuments was united by two twisted sutures. The operation being completed, Mr. Teale and other surgeons present introduced the fore-finger into the artificial anus, and ascertained that the colon immediately below the aperture formed a thickened corrugated pouch, from which no opening into the lower part of the intestine could be detected without instituting a more tedious search than was considered justifiable. The intestinal tunics forming this pouch, although much thickened and firmer than natural, did not communicate to the touch the indurated feeling of scirrhus. On passing the finger upwards into the descending colon, it was found to be capacious, its coats thin and elastic, possessing a perfectly healthy structure."—Mr. TEALE, *Prov. Med. and Surg. Journal*, No. xxv., *March* 19, 1842.

Reduction of a Dislocation of the Lower Jaw, 98 Days after the Occurrence of the Accident. There is nothing peculiar in this case, except the length of time which had elapsed between the occurrence of the dislocation and its reduction: and the author's principal object in publishing it, is to encourage other surgeons to undertake the operation, even under the most discouraging circumstances.—Dr. Donovan, *Dublin Med. Press*, No. clxxvii. *May* 25, 1842.

The Seton in Chronic Disease of the Brain. A farmer, sixty-four years of age, was seized with fever and acute inflammation of the brain, in consequence of sleeping on the damp grass. These seemed to have been treated in an active and judicious manner, yet an obtuse pain in the head remained, with at first slight mental obscuration; but which deepened, at length, into perfect childishness and idiocy. His bodily strength was also greatly impaired.

Dr. B. established a seton in the nape of the neck. In ten days there were faint traces of returning intelligence, and in five weeks the patient became again a useful member of society, and enjoys at present complete bodily and mental sanity.—Dr. Barton, *Lancet*, No. ix. *May* 28, 1842.

Subconjunctival Dislocation of the Lens. William Weavers, aged sixty-four, received a violent blow on the left eye, from a man's fist, more than five weeks since, which caused at the time severe pain and swelling, and loss of vision.

March 8th, 1841.—The upper part of the cornea had now become opaque, sloughy, and ulcerated; but the centre remained sufficiently transparent to allow of the examination of the pupil, which was contracted, fixed, and nearly filled with pus. At the upper part of the globe, a short distance behind the corneal and sclerotic junction, there was a circumscribed semi-transparent tumour of the conjunctiva, which both Mr. Barton and myself considered to be caused by the dislocated lens. I therefore divided the conjunctiva with a cornea knife, and extracted the lens inclosed in the unruptured capsule, both structures being perfectly transparent. I also punctured the lower part of the cornea, to relieve the eyeball from the distension occasioned by internal suppuration. He suffered much less after the removal of the lens, all the inflammatory symptoms subsided, and the eye gradually sank into a state of atrophy.—Mr. Hunt, *Med. Gazette*, No. xxxv. *May* 20, 1842.

Rare Fracture of the Neck of the Thigh-bone. The following fracture of the femur has not often been noticed by surgical writers. The following was the state of the patient when seen by Mr. B. He was, and had been, from the time of the receipt of the accident, unable to bear upon the affected limb, the attempt to do so causing intolerable pain. He could, however, with the assistance of his hands in lifting it, flex it to a considerable extent upon the trunk, while lying on his back in bed, without experiencing any particular uneasiness in the part. The affected limb had become shortened to the extent of about an inch, and was everted, but it was easily rolled inwards into its natural position. It was, however, impossible, or at least difficult, without great violence, to extend the limb so as to make it appear of the same length as the other sound one. There was an extensive ecchymosis of the integument round the joint, involving the skin of the nates and scrotum, of an extremely dark purple colour, like what is usually observed to follow sprains in very old persons. The limb could be rotated to some extent, both inwards and outwards, without much pain; this latter movement producing a grating or rubbing sensation, but very indistinct, and not at all resembling the feeling of crepitus commonly discernible in fractures of the neck of the bone. On pressing rather strongly with the fingers upon the posterior part of the trochanter major, a movement, as if this posterior portion of the trochanter had been detached from the shaft of the femur, was perceptible when the limb was in a quiescent state, but when it was flexed, or rotated, this feeling was not communicated, but only the dull rubbing or grinding sensation which I have described.

On account of the existing ecchymosis, and some little pain which the examination had produced, the limb was at first laid upon pillows in a flexed position, and allowed to remain so for two or three days; I then (the pain having left him) thought that I might use with advantage an apparatus constructed on the same principles as Boyer's, with an endless screw, having a foot-piece or shoe working upon it for the purpose of keeping up permanent extension; but as I found it was utterly impossible, by any mechanical means, to draw out the shortened limb to its natural length, without injurious compression of the perineum by the padded girth, and as he felt otherwise very uneasy from its application, it was relinquished, and Amesbury's double-inclined plane was substituted in its stead; he, however, soon became weary even of this, and he was ultimately placed, without any restriction of the limb (with the exception of firmly binding it to the other to prevent eversion,) upon one of the double-inclined plane beds of the hospital, with which he expressed himself as quite satisfied, and felt comfortable and easy.

The man sank from bronchial inflammation and diarrhœa, and the results of the post-mortem examination of the limb are thus recorded: The subcutaneous ecchymosis had disappeared before his death, but a considerable quantity of blood had remained diffused among the muscles surrounding the fractured portion of the bone. The capsular ligament was entire, but it was somewhat thicker than natural, owing probably to the effects of the rheumatic inflammation in the joint to which he had been subject. On cutting through the capsule, the neck of the bone, which was somewhat thicker and shorter than common, was found to be uninjured, the fracture not having in the slightest degree encroached upon this portion of the bone. The head and cervix of the bone had been obliquely separated from the shaft, just at the root of the trochanter minor, and had passed downwards into the cancellous structure of the bone, which they were enabled to do, owing to the posterior portion of the trochanter major having been broken off, opening as it were to receive them.

The fracture of the trochanter major commenced exactly in the centre of its upper part, and, passing downwards through the length of the trochanter, the fissure wound inwards, and terminated at a little distance below the trochanter minor, which was included in the fractured piece.

It was the movement of this portion of the bone, which I felt during the lifetime of the patient, when I pressed my fingers strongly on the posterior part of the trochanter, while the limb was still and straight, but which was not observed when it was flexed, owing, as I now found, to the fractured neck passing (when flexure was attempted after death) lower down into the cancellous structure, and wedging out, and making immovable this fractured portion, the movement of which was before so clearly distinguished.

On partially removing the fractured portion of the bone, the cancellous structure was found to have been hollowed out, as if by the attrition of the fractured neck within it, which had been caused by the patient injudiciously moving the limb, which had at the same time interrupted the reparative process, and a complete hollow had been formed at this part. The spongy texture itself was not more vascular than is commonly seen after a fracture has occurred a few days. The outer shell of the bone was, however, remarkably thin, and the medullary cavity, for some distance down the shaft, was somewhat larger than is usual in persons even of a very advanced age.

I could now discover what had prevented the extension of the limb; owing to this excavated condition of the cancellated interior of the bone, the upper and fore part of the trochanter major, which had not been broken, had been drawn inwards by the muscles inserted at its root, and had closed upon the end of the fractured neck after it had passed downwards, below it, and on endeavouring to extend the limb after death, the neck of the bone catching under this firm portion of the trochanter prevented the extension of the limb, and I found that I was unable, even after the muscles were removed, to cause the trochanter to pass over the fractured end of the cervix without applying more force than would have been justifiable in the lifetime of the patient. The thorax was not examined.—Mr. Bulley, *Prov. Med. and Surg. Journal,* No. iv. *April* 30, 1842.

Paracentesis of the Abdomen with the common grooved Needle. As paracentesis of the thorax, (observes Dr. P.) by means of a trocar, is much more painful than the introduction of the needle, and is sometimes followed by troublesome results, I determined to try for the future, whether the same success would follow the use of the latter instrument. In many instances this was found to be the case, but occasionally the fluid was too thick to run off through so small a passage. This was obviated by getting a needle made with a somewhat larger groove. Such a needle has been used with perfect success in every case that has fallen under my observation during the last two years. The pain is so slight, that a patient who has once experienced it totally disregards it. The following remark of Dr. P. is worthy of serious attention: I am persuaded that the practice of deferring paracentesis in the former disease (ascites) till all the other means, many of which are very exhausting ones, have been long tried and have failed, is one principal cause of the frequent return of the effusion. The use of the needle affords so much greater facility that the operation will cease to be dreaded, and will be performed more easily, and if so, with greater success.—Dr. Prichard, of Bristol. *Medical Gazette*, No. xxvii. *March* 25, 1842.

Traumatic Tetanus: Cure. This was a decided case. The patient had wounded a finger with a straw-cutting instrument, on the 5th of February. On the night of the 21st, he slept in an exposed situation; and on the 25th, the author found him complaining of violent spasmodic pain at the epigastrium, with great difficulty of breathing; stiffness in the muscles of the neck, and inability to open the mouth; the pulse full; 120. He was bled to ℥xx, and on the succeeding day to ℥xxx, which relieved, but only temporarily. He had two grains of opium every three hours. On Sunday, he was in a state of complete opisthotonos, and intoxicated from the effect of the opium; but suffering little or no pain, when undisturbed; he could breathe, talk, and swallow easily. On Monday, his circumstances were exactly the same. Mr. H. found him in precisely the same position as he had left him in on the preceding day—that is, his body resting on his heels and head. He was again bled to ℥xvj. On the night of March 2, he had profuse perspiration, with great relief. The opisthotonos was so far gone as to allow the patient to turn on his side. At the date of report he was nearly well.—Mr. Higgins, *Lancet*, No. v. *April* 30, 1842.

[This case is extremely creditable to Mr. Higgins. Another case is reported in the Provincial Medical and Surgical Journal for April 30, 1842, No. iv., which was successfully treated by sesqui carbonate of iron. In this case there was complete emprosthotonos, with incomplete trismus.]

Spontaneous Obliteration of the Axillary Artery. A lady aborted on the 7th November, 1831. On the afternoon of the 8th, she felt a sensation at the extremities of the fingers, as if they were scorched. The integuments at the points felt hard, looked white, and were painful, so that she apprehended she was going to have whitloe in all her fingers. She awoke on the following morning from a disturbed sleep, with intense pain and numbness of the whole arm, and almost total blindness. The left arm was now cold and insensible. The wrist and the tops of the fingers were growing discoloured: the ring-finger especially was becoming black. And now no pulsation could be felt in the axillary artery, or any of its branches, below the superior margin of the pectoralis major. This was on the 9th. On the 13th, the sense of feeling and temperature began slowly to return, though the arm still continued very painful; and the discoloration declined: but the ring-finger became black and hard at its extremity, and around the dry crust at that point appeared a line of separation, consisting of a slightly elevated circle, containing a minute quantity of pus. The top of this finger exfoliated, like a dry, hard, black, horn button, nine months after the obliteration of the vessel. So did the integumental extremities of the thumb, first, second, and last fingers. The author thinks the obliteration of the artery is to be accounted for from sudden spontaneous rupture of the internal coat of the artery.—Dr. Oke, *Provincial Medical and Surgical Journal*, No. iii. *April* 23, 1842.

Case of large Calculus in a female Child three Years and a half old. After various fruitless attempts at extraction, by dilatation of the urethra, and by lithotritizing, the lateral operation was performed, and a lithic-acid stone was extracted, measuring an inch and a quarter in length, seven-eighths of an inch in width, two inches and a half in circumference, and weighing two drachms, forty grains. The case did well.—Mr. Grantham, *Med. Gaz.* No. xxiv. *March* 4, 1842.

Penetrating Wound of both Lungs: Recovery. A policeman, aged twenty-two, was stabbed on the right side, about four or five inches below and in a direct line from the axilla; the wound was rather more than an inch in width, and was between the seventh and eighth ribs. On the left side there was a corresponding wound, also between the seventh and eighth ribs, but higher up and more posterior, situated almost in a direct line from the lower angle of the scapula. There was emphysema on both sides, and air was passing freely through the wound on the right side. The wounds were closed with plaster, and the patient, who was cold, had some tea. The man was wounded at four o'clock in the morning, and Mr. R. saw him between five and six. At nine, there were symptoms of reaction, for which venesection to ℥xx, and spermaceti and ipecacuan wine were ordered. At three o'clock of the same day he was bled to ℥xxxiv, and at nine o'clock of the same evening to ℥xxx. On the following day, the air did not pass so freely out and in of the wounded apertures, on the dressings being removed. He was bled to a small extent the same day. He continued to improve from this time, and eventually recovered. Adhesion of the pleura, to a considerable extent, had taken place on the right side; the state of parts, on the left, could be less easily ascertained. Mr. Ruddock, *Prov. Med. and Surg. Journal*, No. xxvii. *April* 2, 1842.

Successful Case of Tracheotomy. The patient was a man of twenty-four years of age, and the symptoms were, of course, urgent. In performing the operation, Mr. A. attempted to use the tracheotomy trocar, having no other tube but the canula fitted to it, but he could not make the point of the stilette enter the trachea, as the cartilages bent under the pressure. He therefore opened the trachea with the knife, but, ere this could be done, the man was insensible. He, however, soon rallied, and a cure was effected. About eight months after the operation (the date of the last report) he was still obliged to employ the canula, as the want of it for more than a minute or two caused distressed breathing.—Mr. Alford, *Prov. Med. and Surg. Journal*, No. xxvi. *March* 26, 1842.

MIDWIFERY, AND DISEASES OF WOMEN AND CHILDREN.

Case of Twins with union of the bodies of the Children. This was the case of a woman, about thirty years of age, and who was pregnant for the second time. On examination, after the labour had made considerable progress, one head was found protruded, and in the recess, between the chin and breast, a second was in progress of expulsion, which last was pressed so firmly on the throat of the former, as to produce great congestion, the colour of the skin changing from a reddish hue to a deep purple, and the tongue being forced beyond the lips. *Both children were expelled together;* no cry was uttered by either, nor was any movement made by them; nor could pulsation at the precordial region be detected. The mother made a good recovery. The children, both males, were united by the chest anteriorly, the union extending a few inches towards the side. The abdominal parietes of both children were deficient below the umbilicus; the viscera, having consequently no support, hung down in front. There was only one source of nourishment from the parent, and one liver, which was larger than usual. There were two hearts, *inclosed in one pericardium;* that supplying the child whose head was born first, was the smaller; the other about the normal size. Each had the usual number of hands and feet, and the organs of generation in both were perfect.—Dr. Skipton, *Dublin Medical Press*, No. 177. *May* 25, 1842.

Birth of a Living Child, on the 179th day. The mother of this child was married on the 22d of July, 1839; had menstruated duly the week before; and was well at the time of her marriage. Her menses did not return after that event. Premature labour came on, on the 17th of January, 1840, and on the 18th, she was delivered of a female child. There were no nails on the fingers or toes of the infant; a thick dark down covered the head instead of hair; the skin was unusually florid and thin, the extremities imperfectly developed, the membrana pupularia were entire. Before it sucked it was shrivelled and covered with down similar to that on its head. It was too feeble to grasp the mother's nipple, and was fed for the first three weeks by milk taken from the breast, and introduced into its mouth by a quill or spoon. It was not weighed or measured until forty days after its birth. It was then nearly the weight of three pounds; and the length of thirteen inches. The centre of the body was about an inch above the umbilicus. It died on the 29th of May, after two days' illness of measles. Before that period, the nails had grown, and the down had almost disappeared from the body.

William Tait, Esq., *Lancet*, No. iv. *April* 23, 1842.

Breech Case. Dr. Reid on being called to a woman in her eighth labour, found a midwife had been attempting to extract the child, by pulling at the body, which was already born; the head being still retained. No endeavour had been made to adjust the head. Dr. Reid, on making examination, found the face in the hollow of the sacrum, and on introducing the forefinger into the mouth, and gently depressing the chin, the head was expelled immediately without the smallest difficulty. Great ignorance had been displayed in this case, and neglect of the rule "arte non vi," for on examining the body of the child, it was found not only extensively bruised, but to have both its thigh bones fractured. Dr. Reid has had, from June 1840, to February 1842, 670 cases, producing 678 children, 351 males, 327 females, of which 71 were still-born; 44 premature births; 11 in a decomposed state.—Dr. James Reid, *Medical Gazette*, No. xx. *March* 11, 1842.

An Enquiry into the Results of Puncture of the Head in Cases of Chronic Hydrocephalus. This is a most elaborate paper, highly creditable to its learned author, and claiming the perusal of all likely to be called on to treat the disease which is the subject of it. It contains a tabular analysis of fifty-six cases of hydrocephalus in which puncture was performed, giving in each case the sex, age, symptoms before puncture, size of the head, number and date of the punctures, quantity of the fluid evacuated, subsequent progress of the case, date of the report and lastly the authority. The following extracts give a brief summary of the principal results of the enquiry and the conclusions deduced by Dr. West.

The writer has found mention of 63 cases of chronic hydrocephalus in which the cranium was punctured. In two of these cases, however, the puncture was accidental, while in five instances the results were not such as would justify classing the cases either as fortunate or unsuccessful. Fifty-six cases then remain, in 40 of which the patients died, while in 16 they are alleged to have recovered; or, in other words, the proportion of recoveries to deaths was as 1 : 2·5, and as 1 : 3·5 of the total cases. These results, though considerably less favorable than those obtained by Dr. Conquest, still appear at first sight to afford ample justification of the operation; but the particulars contained in the table will, perhaps, in some degree modify such an opinion.

It would have been interesting to have been made acquainted with the circumstances to which the brilliant success of the operation in Dr. Conquest's hands is attributable. But unfortunately, no data are given in 15 out of 19 cases, beyond the mere statement of the number of punctures, and the quantity of fluid removed. The age of the patient, the duration of the disease, the symptoms attending it, the size of the head, and the condition of the intellectual faculties before and after the operation, are not noticed. We are left in perfect ignorance as to the time which elapsed before each patient was reported as cured; and yet, on grounds so slender, an impression has got abroad in this country and elswhere that paracentesis capitis

is a means to which recourse may be had in cases of chronic hydrocephalus, with a well-founded expectation of success.

In 30 of the 40 cases, (in which the operation was unsuccessful,) the interval which elapsed between the performance of the operation and the patient's death is stated; and it appears that the deaths after the first puncture were as follows:

Deaths.			Average duration of life after the puncture.
6	within	4 days	53 hours.
6	"	14 days	6 days 8 hours.
3	"	1 month	20 days 16 hours.
9	"	3 months	56 days 10 hours.

Of the remaining 6, only 1 survived the puncture 6 months; and the average duration of life in each of these was 3 months 4 days 12 hours. In 18 of these patients, the operation was performed more than once; but in no instance did the children survive the last puncture more than 35 days, while the average duration of life was 12 days 22 hours.

The instances, then, in which life was prolonged by the operation appear to be very few, and the cases in which any reasonable prospect of the patient's recovery existed after a week had elapsed from the first performance of the puncture, are still fewer. The table shows that sometimes the puncture was followed by an almost immediate aggravation of the cerebral symptoms, and by death. Usually, however, a degree of apparent improvement followed the puncture, but the fluid soon collected again, and less marked relief followed the second operation. With its repetition the quantity of fluid increased, and while the size of the head continued undiminished, or even grew larger, the body of the patient became emaciated: and death either took place from exhaustion, or cerebral symptoms came on, and life was terminated by coma or convulsions.

If the symptoms observed during life yield little encouragement to resort to the operation, the appearances disclosed after death afford a powerful argument against it. An account is given of the post-mortem examination of 26 cases In every instance fluid, sometimes in considerable quantity, was contained within the ventricles or in the cavity of the cranium, and the substance of the brain was softened and attenuated. But, in addition to these appearances, there existed in 16 cases, serious organic disease or malformation of the brain itself, though no symptom during life had betrayed the existence of a condition which mechanical interference could only aggravate.—Charles West, m.d. *Med. Gaz. April* 15, 1842.

Artificial Premature Labour. A woman, married seven years, and aged twenty-eight, has been pregnant six times. Having a slightly contracted pelvis and generally large children, she has never been able to give birth to a fœtus at the full period, without embryotomy. Mr. Braine desired her to inform him of the fact of her pregnancy at the end of the 7th month, on the next occasion of her being in that condition, which she did. Mr. Braine then waited until she was somewhat advanced in the 8th month, when he punctured the membranes, thirty-two hours after which operation, a living child was born. Both mother and child did well.—Mr. Braine, *Prov. Med. and Surg. Jour.* No. iii. *April* 23, 1842.

New Mode of accelerating Labour. The practice consists simply, in imitating the influence of. the child's head, or membranes on the natural passages, and thus producing expulsive efforts by reflex sympathy. This is accomplished by introducing the finger or fingers as far as the point of the os coccygis, and passing them downwards along the whole surface of the vagina, so as to give the sensation of distension.—Mr. Staniland, *Provincial Med. and Sur. Journal*, No. xxvi. *March* 26, 1842.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

Remarkable case of Suicide. A woman aged 29, and previously in robust health, was found dead in her apartment. In accordance with the provisions of the anatomy act, her body was received by Dr. Handyside for dissection, and was by him, along with an assistant, carefully examined. Nothing was found suf-

ficient to account for the death; and Dr. Handyside believing her to have died of "simple apoplexy," had the lips sewed together, proposing to reserve the body for his lectures. About two months after, while about to demonstrate on this subject, the muscular structure of the pharynx, Dr. Handyside introduced his finger into the back part of the mouth, in order to stuff that cavity with hair; but found this space preoccupied by a dense foreign substance, apparently round in form, and impacted between the roof of the tongue and the soft palate, so very firmly, as to have cut off the supply of air into the lungs during life, by forcibly closing the mouth and posterior nares. The materials of this plug consisted of portions of soft cotton, called spindle ends. Some of this cotton, the woman had untwisted and rolled up very closely, coiling over it two strips of flannel. Finally, she had fastened these materials together with a large rough pin. On examination, the soft palate presented to view, on the left side, (the one corresponding to that portion of the plug, where the rough head of the pin projected,) a small but deep laceration, and which notwithstanding the long interval, was surrounded by a circumscribed patch of ecchymosis, still of a vermilion hue. The right side of the soft palate, the anterior fourth of the tongue and the hard palate opposite to it, the epiglottis, and the arytenoid cartilages, which appeared to have been violently separated by the last expiration, also exhibited ecchymoses.

The author infers from the above case, the necessity in cases of medico-legal enquiry, of paying attention to the natural apertures. He also calls attention to the length of time that the appearance of recent ecchymosis lasted.

The author cites a somewhat similar case, described by Dr. Wagner, as having occurred in Berlin. It was that of a criminal who was found dead in his cell. Another case also occurred about five years ago, in Edinburgh. [Qy. might not this have been a case of murder in place of suicide?]—Dr. Handyside, *Edin. Med. and Surg. Journ.* No. 151. *April* 1, 1842.

Detection of Arsenic in Complicated Liquids. After remarking that Marsh's test, from its very perfection of delicacy, is not easily manageable in the hands of any not thoroughly conversant with chemical research, the author proposes the following plan, in the employment of which, not a single instance of failure has occurred.

When dilute sulphuric acid is boiled with the greater number of the substances used for food, and which are likely to become the objects of chemico-legal investigations, the invariable effect is the acquisition, by such substances, of a great degree of thinness and limpidity, partly by the conversion of the starch and mucilaginous matters they contain, into dextrin and sugar, and partly also by the coagulation of the albumen and casein present. This is very strikingly shown by boiling, for a few minutes, a quantity of thick gruel with a little dilute oil of vitriol; the mixture becomes quite thin, and runs through a paper filter, almost as freely as pure water, a coagulum of azotized substance being left behind. The same thing happens with milk and beer, and many other complicated liquids; they become as thin as water, and filter quite easily. A clear solution being thus got, a stream of sulphuretted hydrogen is, when cold, passed through it, the liquid boiled for a few seconds, and then passed through a small filter, and the orpiment washed. In this state it is seldom pure enough for advantageous reduction, even when its colour is bright; it is better to dissolve it in aqua regia, to evaporate gently to dryness, take up the residue with water, and again to precipitate with sulphuretted hydrogen, wash, dry, and reduce with black flux. The second precipitation by sulphuretted hydrogen requires some care, as the arsenic is often not at first thrown down from being in the state of arsenic acid; it is proper, after passing the gas some time, to heat the liquid to its boiling point, suffer it to cool, send through it an additional portion of gas, and again boil; by which all the orpiment falls down at once, and is easily collected on a little filter. The solution should also be acidulated with a little hydrochloric acid. When such a substance as soup is to be examined, rich in gelatine, it is better to get rid of that body by the aid of an infusion of gall-nuts before proceeding to the treatment by sulphuric acid.—George Fownes, Esq., *Pharmaceutical Journal*, No. x. *April* 1, 1842.

PART FOURTH.

Medical Intelligence.

Report on the Results obtained by the Use of the Microscope in the Study of Human Anatomy and Physiology. Part I. By James Paget, Demonstrator of Morbid Anatomy at St. Bartholomew's Hospital.

The design of the present report is to bring together in the briefest possible space the conclusions regarding the structure and the functions of the several tissues of the human body which have been rendered certain or most probable by microscopic investigation. The task will, it is hoped, be deemed worthy of the labour which has been bestowed upon it; for in no department of medical science has there been so great an addition of facts in the last ten years as in minute anatomy; and in none has the access to knowledge been more difficult. The greater part of the original records of microscopic anatomy are scattered through a multitude of monographs, of brief dissertations, and of essays in the foreign journals, to which few can refer; and in our own language there is no work which affords an adequate notion of their contents. In France, Holland, and Italy there is the same defect; and even in Germany it existed till, very recently, the systems of general anatomy of Henle[1] and Bruns[2] appeared.

This First Part of the report relates to the structure of the general component parts of the body in their complete state; in a Second Part, their several schemes of development will be described.

The writer has endeavoured to keep within the strict limits of the office he has assumed; he has not pretended to do more than report what has been already published; but that he might do so accurately and impartially he has been careful to draw his materials from none but their original sources.

Some of the numerous reference-notes may seem superfluous; but they are inserted in the belief that with their aid it will not be difficult for any one who has access to the works quoted to fill up the outline of knowledge which the text affords. By the aid of some, interesting histories of discovery may be traced; by others, facts may be found which, though they now seem unimportant and are, for brevity's sake, omitted, may hereafter become valuable; by means of others again, the description of the structure of particular organs, and the details of the facts which are related in general terms in the text, may be at once referred to.

The fact of the single origin of all the tissues from primary cells suggests that the most natural arrangement of them must be that in which they are placed in a succession corresponding to the degrees in which, in their perfected condition, they severally deviate from the primary form. And although, from the imperfection of the knowledge hitherto attained, such an arrangement cannot yet be certainly and completely established, there are sufficient advantages in even a partial adoption of it to warrant its employment on the present occasion, with only such modifications as the physiological relations of certain parts seem, in some instances, to render more convenient than a strict adherence to system.

[1] Allgemeine Anatomie, von J. Henle—Leipzig, 1841, being the 6th volume of the new German edition of Sömmering's Anatomie. The present part of the Report was nearly completed when this admirable work arrived in England: but the numerous references made to it will sufficiently prove of how much avail it has been for addition and confirmation to what had been written.

[2] Lehrbuch der allgemeinen Anatomie des Menschen, von Victor Bruns.—Braunschweig, 1841. 8vo.

With this preface, and with the aid of the second part of the report, the plan here followed will be easily intelligible.

I. Blood-corpuscles. The discovery of the blood-corpuscles by Malpighi[1] was one of the first-fruits of microscopic study, and since that event few objects have been more solicitously examined. It is now agreed that they are minute, flattened, transparent cells, containing (at least during one period of their existence) round, or oval nuclei, and having incorporated in them all the red colouring matter of the blood.

a. Form and Size. They are circular in man and in all mammalia, except the camel tribe in which they are elliptical;[2] and they are elliptical in all other vertebrata, except certain cyclostomes in which they are circular.[3] In all they are flattened and have rounded borders. Whether their surfaces be slightly concave or convex depends on variations in the quantity of their contents which may ensue either within, or after their removal from, the body. In invertebrata they are less numerous but more varied in form; for the most part they are irregular, granular, roundish, nucleated corpuscles.[4]

It is difficult to discern any strict connexion between the various sizes of these bodies and the other parts of the organism of different animals. Among mammalia those of the elephant are the largest;[5] then come those of the capybara and rhinoceros;[6] then those of man, which have an average diameter of about $\frac{1}{3500}$ or $\frac{1}{4000}$ of an inch.[7] In general those of ruminants are smaller than those of other mammalia; and the smallest yet known are those of the little chevrotain and Napu musk-deer, of which the average diameter is less than $\frac{1}{12000}$ of an inch.[8] An examination of the elaborate tables by Mr. Gulliver shows that the size of the corpuscles in mammalia is not unconditionally proportionate to the size of each animal, or according to the nature of its food. Yet there is evidence enough that in each great division of the class, the size of the blood-corpuscles is, with few exceptions, directly proportionate to that of the animal's body; and that, in general, those of omnivora are larger than those of carnivora, and those of the latter larger than those of herbivora; so that if the kind of food and the size of the mammal be known, the size of its blood-corpuscles may be probably estimated.

In birds there is a greater uniformity of size and shape in the blood-corpuscles than in mammalia, and, according to Mr. Gulliver, a nearer relation between

[1] Athanasius Kircher, Malpighi, and John Swammerdam have all received from different writers the honour of this discovery. Whoever will, may satisfy himself of the justice of the award given in the text by consulting A. Kircher, (Scrutinium Phys. Med. Pestis, Lips. 1659, p. 240 and the context); Malpighi, (De Omento, Pinguedine, &c. p. 42;) and his Autobiography in the Opera Posthuma, (pp. 25, 92, in the Fol. Ed.;) and Swammerdam, (History of Insects, first published in 1669, at p. 31, of Hill's edition of 1778.) Lists of the principal writers on the blood-corpuscles may be collected from the General Anatomies of E. H. Weber, Gerber, and Henle.

[2] This remarkable and unexplained exception was discovered by Mandl in the dromedary and alpacha (Comptes Rendus des Seances de l'Acad. des Sc., Dec. 30, 1839), and has been amply confirmed.

[3] R. Wagner, (Lehrbuch der Physiologie, B. i. 153.)

[4] Wagner, Lehrbuch, and Beitrage zur vergleich. Physiologie. But he regards them as only chyle-corpuscles.

[5] Mandl, (Anatomie Microsc. p. 17.) Owen (Contributions to the Comp. Anat. of Blood-discs, Lond. Med. Gazette, Nov. 15, 1839) says most of them are $\frac{1}{4}$ larger than human blood-corpuscles. According to Gulliver (Appendix to Gerber's General Anatomy, p. 42,) the average diameter is $\frac{1}{2475}$ of an inch, which is nearly accordant with Mandl's statement.

[6] Gulliver, *l. c.* *All the sizes mentioned in this part of the report are stated in fractions of an English inch.*

[7] Perhaps the strictest measurements are those of Mr. Bowerbank for Mr. Owen *l. c.* In these the average was $\frac{1}{3687}$, the extremes being $\frac{1}{3343}$ and $\frac{1}{3279}$. Copious lists of the measurements by different observers are given by Köstlin, (Mikroskop. Forschungen, p. 55;) in the Microscopic Journal, vol. i. and in Mandl, *l. c.* p. 10.

[8] Owen, *l. c.* Dec. 20, 1839; and Gulliver, *l. c.* p. 44.

their size and that of the body. They are the smallest of the elliptical blood-corpuscles, those of the camel tribe excepted; they are generally rather less than twice as long as they are broad, measuring about $\frac{1}{2000}$ by $\frac{1}{4000}$ of an inch, and about six times as long as they are thick. In reptiles, the largest and, by comparison, the thinnest, blood-globules yet known occur; and Wagner[1] remarks it as a general rule, which Mr. Owen confirms, that the longer the branchial apparatus persists, the larger are the blood-corpuscles. Thus, in the Proteus they are about $\frac{1}{350}$ of an inch long, in the Syren $\frac{1}{435}$ by $\frac{1}{800}$ [2]; in the batrachian reptiles generally, about $\frac{1}{1000}$ by $\frac{1}{3000}$; and their thickness is not more than one eighth of their length.[3] This rule, however, fails when one comes to fish, in which the branchial apparatus is persistent and perfect; for in them the blood-corpuscles, though resembling those of reptiles, are generally smaller and less elongated.[4]

b. Structure and Composition. The blood-corpuscles are generally regarded as primary nucleated cells, and no one doubts that those of birds and the lower vertebrata consist of an external cell, formed of an extremely delicate, soft, and elastic membrane, in and within which all the colouring matter seems to be contained, and of an internal parietal nucleus, generally similar in form to the cell, but about one fourth its size, colourless, and in the large corpuscles of some of the amphibia containing a number of distinct granules.[5]

It is questioned, however, especially by Valentin, Wagner,[6] and Gulliver,[7] whether the corpuscles of mammalia have nuclei, or whether the central spot be not merely produced by the accumulation of the colouring matter at the circumference. Henle[8] would decide the question by saying that, in a few of these small corpuscles, there are nuclei; but that in the majority (and these the most fully developed,) there are none; so that he thinks it probable that here, as in some other cases, the nucleus, after the cell is perfected, is gradually absorbed.

According to Dr. Barry[9], the young blood-corpuscle in all the vertebrata is a mere disc, with a depression in the centre. In mammalia it retains this form; in the other classes the disc becomes a nucleated cell. The nucleus at first communicates by a pellucid orifice ("nucleolus") with the exterior of the corpuscle, this orifice occupying the place of the depression in the original disc. The orifice becomes narrower, and the nucleus finely granular, and these changes immediately precede the division of the nucleus into minute discs. The discs, whose number is multiplied by successive divisions, and by the gradual appropriation of the nucleus from its circumference towards its centre, arrange themselves so as to form a flat filament, having an appearance the same as that which he finds to be presented by fibre in all the filamentous structures of the body. According to the number of discs, this filament forms within the blood-corpuscle either a ring (as in man and most mammalia, where they are comparatively few), or a coil (as in birds, amphibia, and fishes, where the discs are much more numerous, and the filaments proportionally longer.)

The filament thus formed is flat and deeply grooved on both surfaces, being thereby thinner in the middle than at the edges. The edges are rounded; and,

[1] Lehrbuch, p. 153. [2] Gulliver, in Appendix to Gerber, p. 52.

[3] In the cryptobranchus japonicus (in which there is no persistent branchial apparatus) they measure $\frac{1}{472}$ by $\frac{1}{731}$ (Van der Hoeven, Tijdschrift fur Naturl. Geschiedenis, 1841, p. 270,) but considering the great size ofthe animal, these enormous blood-corpuscles are not disproportionate.

[4] In the blood-corpuscles of the siren, as many as 20 or 30 granules can be seen in one plane of the nucleus. (Owen, in the art. *Siren*, Penny Cyclop.) The proportionally-longest corpuscles are those of the crocodilus lucius: they measure about $\frac{1}{950}$ by $\frac{1}{2538}$ of an inch. (Mandl, Comptes Rendus, Dec. 23, 1839.)

[5] The anarrhicus lupus presents an exception in this respect. Its corpuscles measure $\frac{1}{1730}$ by $\frac{1}{3750}$. (Van der Hoeven, *l. c.*, p. 272.)

[6] Lehrbuch der Phys., i. 154. [7] *L. c.* Appendix, 13.

[8] Allgemeine Anatomie, p. 432.

[9] Philosophical Transactions, 1841, &c. The description in the text has been kindly furnished by Dr. Barry himself. See Appendix A.

when seen on its edge, the filament at first sight seems to consist of segments separated from one another by oblique lines. When perfected, the filament undergoes various changes: sometimes unwinding itself into a straight fibre; at others, continuing circular, while smaller coils of similar filaments are formed within it from a residual portion of the nucleus. In all cases the filament is reproduced by self-division, so that out of a single filament a fasciculus may be formed. Such changes are seen going on in coagulating blood. The filaments now mentioned exactly resemble those which are found in a great many, both animal and vegetable, tissues, nor can any definite line of distinction be drawn among the gradations from them to the double spiral filament, of which Dr. Barry believes that the primitive fibrils of muscle, and certain other tissues, are composed.

In all cases in which a nucleus is present, it differs in chemical characters from the cell. The colouring matter, or hæmatosine, is easily soluble in water, by which it may be completely washed out of the enveloping cells. The latter are composed of a peculiar albuminous substance (*globulin* of Berzelius), which only slowly dissolves in water; the nuclei consist of a different albuminous substance, more like coagulated fibrine, which is quite insoluble in water, and they contain so large a quantity of inorganic matter, that they completely retain their form, and, apparently, their substance, after combustion.[1]

II. Lymph- and chyle-corpuscles. In the villi of the small intestines, the chyle is a pure milk-white albuminous fluid, which does not spontaneously coagulate; its opacity is due to a number of minute oil-globules, varying according to the nature of the food, which float in it without any mixture of fibrine, or of the peculiar corpuscles which afterwards appear. Some of these particles of oil, distinguished by their immeasurably minute size, and by a more general similarity of character than the others present, are described by Mr. Gulliver[2] as forming the *molecular base* of the chyle. Besides these constituents, the more mature chyle, especially after it has passed through lacteal glands, contains the proper *chyle-corpuscles*,[3] which are colourless, moderately transparent, roundish bodies, some larger, some smaller than blood-corpuscles, apparently composed of numerous granules arranged round one or more central molecules. How these are produced, whether by an aggregation of a number of the minutest oil-globules that form the molecular basis,[4] or by a genuine development of cells from the fluid of the chyle, which may be considered as a cytoblastema, is not yet certain; but, be this as it may, as the corpuscles appear and increase, the oil-globules diminish, and the chyle, acquiring a greater proportion of fibrine, becomes more firmly coagulable.

The lymph-corpuscles closely resemble those of the more perfect kind in the chyle. Some of them are generally discernible in the blood, moving, as it circulates in the capillaries, in the peripheral portion of the current. They are somewhat larger than the blood-corpuscles, white, strongly refracting light, roundish and granular, or mulberry-like.[5]

[1] See Harting,(Gissingen betreffende de eerste vorming der cellen, &c., Tijdschrift voor naturl. geschiedenis en physiologie, 1841, 8 Deel;) an essay pointing out several remarkable analogies between the forms assumed by certain inorganic precipitates (such as those of the carbonates of lime and iron) and the forms of the nuclei and cells of organic tissues. The best microscopico-chemical analysis of the blood-corpuscles is in Wagner, (Lehrbuch, p. 160.) The brief notices which can alone be admitted here will but inadequately tell how much aid the microscope has afforded to animal chemistry.

[2] In Gerber, Appendix, p. 88. See also his Contributions, &c. Philosophical Magazine, June 1842.

[3] Discovered by Leeuwenhoeck, and first well described by Hewson.

[4] They do not indeed act similarly on the application of chemical tests; but the observations of Ascherson, presently to be mentioned, show how it is possible that in their aggregation the minute molecules may have their apparent chemical properties modified.

[5] The fullest history of the chyle and lymph is in Nasse, (Unters. zur Physiologie und Pathologie, bd. ii.;) and in Gerber, (Appendix, by Gulliver.) See also Carpenter's Principles of Human Physiology, p. 160.

III. FAT-CELLS. There are several instances of cells containing oil at certain periods of their development, but those to which the name of fat-cells is peculiarly given, are the minute vesicles which, lying in the areolæ of the fibro-cellular tissue, constitute the adipose tissue. These are true primary cells, whose contents are oil instead of the albuminous fluid with which most are filled.[1] They are, for the most part, nearly spherical, and vary from $\frac{1}{200}$ to $\frac{1}{1300}$ of an inch in diameter:[2] their membranes are structureless and remarkably thin; and they are usually aggregated in bunches traversed and enveloped by fibro-cellular tissue, conveying small blood-vessels.[3] Sometimes each cell bears on its wall a nucleus;[4] and in their early periods the oil in each is not in a single drop, but is composed of one comparatively large and several small drops, which subsequently coalesce.[5]

A fact may be mentioned here by which Ascherson[6] has endeavoured to explain the development, not of the fat-cells alone, but of all primary cells; namely, that if a minute drop of oil be placed in contact with a solution of albumen it directly becomes coated with a film of the latter, so as to resemble a fat-cell. Any one may observe the fact in preparations in which a greasy bone is put in weak spirit: for wherever the oil oozes slowly the bone becomes covered with this artificial fat, for which the filmy envelopes are furnished by the albumen dissolved in the water of the diluted alcohol. It is not probable that the fact admits of an extensive application in the physiology of cell-development: yet there is sufficient analogy between these artificial products and such bodies as the minute corpuscles of the chyle, the granulated oil-globules (*corps granuleux.*) of the milk, and some others, to render it probable that their developmental process is in great measure the same.[7]

IV. CUTICLE. The physiology of cuticle has received an altogether new aspect from recent investigations, and especially from those of Henle.[8] He has shown that, with very few exceptions, all the free surfaces of the body, both those of the integuments, of the serous cavities, of the mucous tracts, of the blood-vessels, and of the gland-ducts are invested by a membrane composed of one or more layers of primary cells, forming a cuticle or epithelium.

Forms and arrangements. The elements of these cuticles, however, have not all the same form; and while they all serve the common purpose of protecting the tissues beneath them, many, perhaps all, add special functions to which they

[1] Malpighi (De Omento, &c.) first described the adipose tissue with some accuracy. Fontana (Traité sur le Venin de la Vipere, &c.) first clearly described its elementary cells: to Raspail (Nouv. Syst. de Chimie organique) and Schwann (Mikroskopische Untersuchungen), however, are due nearly all the more important facts regarding their minute structure.

[2] See E. H. Weber (Hildebrandt's Anatomie, bd. i.), and Krause (Anatomie des Menschen, bd. i.)

[3] These are admirably figured by Mascagni, (Prodromo della grande Anatomia.)

[4] Schwann, (Mikrosk. Untersuch. über die Uebereinstimmung, &c. p. 144); Bruns, (Allgemeine Anatomie, p. 32.)

[5] Schwann, (*l. c.*) Henle, (Allg. Anat., p. 181.)

[6] Ueber den Physiol. Nutzen der Fettstoffe, Muller's Archiv, 1840.

[7] The minute anatomy of the secernent glands might find its place here, since it is probable the real agents of secretion are isolated primary cells: but most of the glands are of so complex a structure that it will be desirable to describe first some more of their component parts.

[8] Before these investigations the existence of the greater part of the internal cuticles was argued rather than proved. Leeuwenhoeck (Select Works, by Hoole, vol. ii. p. 126,) first described with some accuracy the scales of the epidermis and of the epithelium of the mouth and vagina. Della Torre and Fontana (Traité sur le Venin, &c., t. i. f. 8-10,) more clearly described the cells and their nuclei in the mucus of the eel; then Raspail, Breschet, and others gave accounts of similar cells in the epidermis; and then the cuticles of many different parts were described by Purkinje, Valentin, and other German anatomists. The existence (not the structure) of the epithelium of the intestines was shown by Lieberkuhn (De fabrica et actione Villorum.) There is a complete history of the observations in this part of structural anatomy in Henle, (Allgem. Anat., p. 259.)

are adapted by a peculiar form or energy of their elements. They are all, in their complete state, formed of nucleated cells, in which, while the cells in different parts present many varieties, the nuclei are generally round or oval, flat and colourless, or reddish;[1] and the latter contain one or two distinct small nucleoli, and others very pale and small, and varying in number. The nuclei measure from $\frac{1}{3733}$ to $\frac{1}{3760}$ of an inch in diameter,[2] and the nucleoli about $\frac{1}{10}$ as much. The cells are usually clear and colourless, but are sometimes beset with minute points.

According to the form of the cells, Henle[3] distinguishes three varieties of epithelium; but they are not separated by strict differences, for whenever a continuous surface bears at different parts two different epithelia, there is a very gradual transition from one to the other.

The first is the *tesselated* or *plaster-epithelium,* which is composed of one or more layers of flat, oval, roundish, or polygonal cells, each about $\frac{1}{1900}$ of an inch in diameter, and containing a nucleus of the same shape, which again contains one or more distinct, and several paler, granules. This is by far the most common form of cuticle: it covers the skin and lines the ducts of the cutaneous glands, the mouth, conjunctiva bulbi, pharynx, œsophagus, numerous gland-ducts, the vagina and cervix uteri, the entrance of the female urethra, the serous and synovial membranes, blood and lymph vessels, and a few other parts; and, as a general rule, has a thickness directly proportioned to the friction and other sources of injury to which it is exposed.

Cylinder-epithelium, the second variety, is found from the cardia, along the remainder of the digestive canal, to the anus, in most of the gland-ducts that open on the interior of this tract, and in the greater part of the male genito-urinary apparatus and the gland-ducts connected with it. It is composed of closely-set cells of a somewhat conical, pyramidal, or cylindrical form, about $\frac{1}{1200}$ of an inch long, whose apices are attached to the mucous membrane or to flat epithelium-cells lying on it, and whose bases, which are usually terminated by a truncated plane about $\frac{1}{6000}$ of an inch broad, are free. Each such cell encloses, nearly mid-distance between its base and apex, a flat nucleus with nucleoli.

In the *ciliary epithelium,* which constitutes the third variety, cells like those in the second have several fine, pellucid, blunt ciliæ, about $\frac{1}{30000}$ of an inch long, attached to their free extremities. These, by means of some unknown power, are during life in constant motion, either whirling round their fixed extremities, so that their ends describe circles, or else waving continually backwards and forwards and alternately rising and falling. Examples of this kind are found on every part of the respiratory mucous tract above and behind an imaginary plane drawn from the base of the nasal bones to the anterior maxillary spine, even into the air-cells, in the lachrymal sac and canals, along the Eustachian tube, and on the membrana tympani, on the palpebral conjunctiva, the cerebral ventricles, the commencements of the capsules covering the Malpighian bodies,[4] and in the female genital organs, from the middle of the cavity of the uterus through the Fallopian tubes, and for a short distance on the peritoneal surface of the latter.

The modes in which the elements of these several cuticles are connected are equally various. In the tesselated epithelium the component cells, when there are several layers, generally lie confusedly one over the other; and those in each layer adhere by their edges with the smallest possible quantity of intercellular tissue, their mutual pressure usually making each cell polygonal. In the cylinder variety they are generally almost in contact at their fixed extremities, while their free portions are immersed in a soft intercellular substance, which fills all the spaces between them, and forms a smooth surface over them.

[1] This colour exists particularly in the nuclei of the youngest and deepest layers, (Henle.) Dr. Barry (*l. c.*) points it out as one among many analogies between the epithelium-cells and the blood-corpuscles.

[2] Henle, Allg. Anat., p. 222.

[3] Ueber die Ausbreitung des Epitheliums, Müller's Archiv, 1838; and Allgemeine Anatomie, p. 220, from which nearly all this account of cuticles is derived.

[4] Bowman, (Cycl. of Anat.—*art.* Mucous Membrane;) who denies, also, the existence of ciliary epithelium in the air-cells.

Modes of growth. What has been said of the structure of cuticles plainly indicates the change that must be admitted in their physiology. They are, in the most proper sense of the term, organized; for, besides the peculiar definite form of their elements, each cell has in itself the power by which it is developed, and it depends on the subjacent vascular tissue only, as all other elementary structures do, for the supply of the materials which it may use for its own nutrition. These materials before they attain their ultimate form undergo several changes, which have been best traced in the laminated varieties of the tesselated epithelium, including the epidermis and the thickest of the epithelia of the mucous membranes. In all these, the youngest layers, that is, those which lie next the vascular membrane or *matrix*, seem to consist merely of reddish nuclei, and these formed by an aggregation of granules; the cells, if there be any, being too small to be discerned from the nuclei which they contain. As these are moved from the matrix by the new materials successively deposited beneath them, both the nucleus and the cell, which is soon obviously developed around it, grow larger; but the cell outstrips the nucleus, and at length assumes the usual appearance of an oval, nucleated corpuscle. Then, as they become older and still more distant from the matrix, the nucleus either remains stationary or grows gradually more obscure, while the cell still increases in size, but becomes flat, dry, polyhedral, or quite irregular; and at the last (as one sees daily in the outermost layers of the epidermis,) falls off alone or united with many of its fellows in a shapeless scale. In the same progress also its chemical constitution is altered: at first the cell dissolves easily in acetic acid; at last it assumes the peculiar characters of horn, and is altogether insoluble in that fluid.[1]

In the cylinder epithelium the mode of growth is the same as far as the formation of the cell around the nucleus; but then, instead of retaining nearly the same shape, the cell enlarges more in one direction than in the other; and in the ciliary variety proceeds to the production of vibratory ciliæ from one of its extremities.

Purposes. The uses of cuticles are probably, as already mentioned, various. One is to protect subjacent parts, another to render certain surfaces smooth. But many may have more active offices. It will be seen that the elements of the most energetic secretory organs have only the same structure as these have; and it is not improbable, for many reasons, that the epithelium-cells also prepare and carry off with them, when they desquamate, some peculiar fluid. The ciliary cells probably serve to fan onward the fluids and the minute particles in contact with them. The direction in which they act is most commonly, but not constantly, towards the external orifice of the canal on which they are placed; but in truth their special purpose is in many instances (e. g. the cerebral ventricles,) as uncertain as the power by which they act.[2]

v. Pigment-cells. In certain parts there are special organs for the production of colour, namely, in the pigmentum nigrum and uvea of the eye, in the membrane of the ampullæ in the internal ear,[3] in the whole of the epidermis of the negro and other coloured races, and in that of the scrotum, the areolæ of the breasts, the labia and some other parts of light-complexioned persons.[4] In

[1] It hardly need be pointed out that these facts prove the distinction of a rete Malpighii from the epidermis to be artificial.

[2] See, on all that relates to them, Sharpey, (Cyclopædia of Anatomy, art. *Cilia*;) Purkinje and Valentin, (Muller's Archiv, 1835; and B. and F. M. R., vol. i. p. 509); Pappenheim, (*ibid.* 1840, p. 510.)

[3] Wharton Jones, (Cyclopædia of Anatomy, *Organ of Hearing*,) to whom also is due the first accurate description of the pigm. nigrum; See Edinb. M. and S. J. xl. p. 77.

[4] Simon, (Ueber die Structur der Warzen, &c. Muller's Archiv, 1840, p. 151.) The black matter of the lungs and bronchial glands has no special apparatus. That which tinges expectoration seems deposited in ordinary mucus-corpuscles, (Martin Barry, Phil. Trans., 1841, p. 226, Pl. xx., f. 72.) The dark colour of many superficial nævi, of liver spots, and of freckles, is due to cells of pigment like those described in the text. (Simon, *l. c.*)

all these the pigment is composed of minute, dark granules, collected in primary cells. The latter, when they lie loosely, as they do on the iris, are oval or round; but when, as in the pigment of the choroid, where they are most distinct, they are set closely and in an even layer, their mutual pressure makes them almost regularly hexagonal. They are nearly flat, about $\frac{1}{1800}$ of an inch in diameter, and lie in apposition so as to form a beautifully tesselated membrane. Sometimes their edges are in contact, sometimes slightly separated by a pale line. Near the middle of each there is a clear spot, produced by the nucleus of the cell, in which there are usually one or two nucleoli; and around this the pigment-granules are arranged. They are most numerous in its immediate neighbourhood, are less thickly collected at a little distance from it, and at the very margin of the cell there are often none at all. They are collected also only at the posterior surface of the cells on the choroid; the anterior portion of each cell (that is, that part which is turned towards the cornea,) contains the nucleus, and is quite transparent.[1] In the negro's epidermis perfect pigment-cells, like those of the choroid, are found only in the deepest layers, and are most numerous at the bottoms of the fossæ between the papillæ. They do not differ from those already described, except in being smaller and in forming several layers; they sometimes seem to have separate granules, not inclosed in cells, mixed with them.

The pigment-granules are among the minutest structures in the body. They are flat, oval corpuscles, measuring about $\frac{1}{20000}$ of an inch in their longest diameter, and about $\frac{1}{4}$ as much in thickness; so that, according to the direction in which they are seen, they appear flat or linear or like mere points.[2] When set free by the bursting of the cell, they exhibit a very active molecular motion; and Schwann has noticed the same occurrence even while they are inclosed. It is only when aggregated that they give their full dark colour; when isolated they are nearly pellucid, or their borders only appear dark.

VI. NAILS. Bruns[3] alone has accurately described the structure of the nail in the adult. It consists of primary cells, almost exactly similar to those of the epidermis, firmly fixed together in several layers. The youngest cells (those which lie at the root and in the deepest layers of the body of the nail) are pellucid, round, elongated, or polygonal, and contain a round, granulated nucleus, with some other granular substance. They measure about $\frac{1}{1000}$ of an inch in their greatest diameter, and their nuclei are about $\frac{1}{10}$ as large. The oldest and most superficial cells are larger and broader, but much thinner and more irregular in their shape. Their nuclei are rarely discernible; between the old and new cells, those which are intermediate in situation, are so in structure also. The striæ and fibres, described by Gurlt,[4] are due, Bruns says, to the section of the cell-membranes; the granules of Tourtual[5] are traces of the nuclei.

The growth of the nail is effected by the constant generation of cells at its root and at its under-surface; and this process is carried on with the energy necessary to repair the constant waste, both at the free extremity and the exposed surface; as the successive layers are pushed forward and towards the surface, each cell becomes larger, drier, and flatter, and more firmly fixed to those around it. The identity of the structure of the young cells of nail and of epidermis, renders the question, whether the epidermis be continued under the nail, one of words only.

VII. CARTILAGE.[6] *True Cartilage.* The proper substance of true cartilage

[1] Henle, Allgem. Anatomie, p. 281. [2] Ibid. *l. c.*, p. 284.

[3] Lehrbuch, p. 197. Before examination he makes the nail very soft by immersing it in an alkaline ley. His description exactly confirms what Schwann had proved of its development.

[4] Unters. über die hornigen Gebilde, (Müller's Archiv, 1837.)

[5] Hornstoff im Kropfen, p.262. Müller's Archiv, 1840, p. 254.

[6] According to their minute structure the parts commonly called cartilages and fibro-

is a homogeneous and nearly transparent white basis, in which there are numerous small, round, oval, or flat and elongated, or sometimes angular, cavities (*cartilage corpuscles*), from $\frac{1}{1000}$ to $\frac{1}{3300}$ of an inch in diameter.[1] They usually seem to be mere cavities hollowed out in the basis substance; but, by long boiling, the latter is nearly dissolved, while they remain as distinct corpuscles; and sometimes a lining membrane is discernible in them.[2] They are, therefore, to be regarded as genuine cells, whose walls, during their development, have amalgamated with the intercellular substance.[3] Each such cell encloses one or more round nuclei which contain nucleoli, and sometimes are surrounded by cells, so as to give those first mentioned the character of *parent-cells*, that is, of cells inclosing a second generation of cells. The secondary contained cells vary in number, and have usually the form which mutual pressure gives them as they enlarge. The nuclei are often filled by fine particles of oil, which are at first isolated, but may coalesce, and, when very numerous, may fill their cavities so as to make them look like fat-cells; and, when this takes place, the cavity of the primary cell also usually contains particles of oil. But in all these respects, as well as in the character of the contents of the primary cell, whether they be cells or nuclei, and in the mode in which the contained corpuscles are separated or attached to the parent-cell, the varieties are manifold and as yet unexplained.

The number of the cartilage-corpuscles, and the direction in which they are arranged, are more regular. In the articular cartilages they are most numerous, and smallest in the part most distant from the bone. All of them have their long axes vertical to the surface of the bone, except a thin layer of those next the joint, which are flattened and lie parallel to the articular surface. Hence it is that such a cartilage will crack vertically to near its free surface, and then tears transversely. In all other cartilages, also, most of the corpuscles are set vertically to the surfaces, but there is a superficial layer in which they are flattened and lie parallel to them. In the cartilages of the ribs, the internal corpuscles are arranged in rows which radiate from the axis.

The substance in which the corpuscles are imbedded is usually homogeneous and structureless, but in certain cartilages it gradually assumes a finely fibrous aspect, and ultimately becomes truly fibrous.[4] With these changes two others coincide: namely, the formation of fat in the nuclei of the cells, and the acquirement of the yellowish colour in the place of the pearly blue of young cartilage; and all these appear to be preparatory to ossification, for they are never seen in cartilages in which that change does not take place.[5]

Fibrous Cartilages. The fibrous structure of some of the cartilages just described makes the transition to the fibrous cartilages, properly so called. In these, there are corpuscles similar to those in the preceding variety (except that the nuclei most commonly contain oil), but the fibres are much darker and coarser, and form the greater part of the tissue. In different examples the directions of the fibres vary: in some, as the symphysis pubis and interverte-

cartilages may be thus divided (Henle):—1. True Cartilages: the articular (with some exceptions presently mentioned), the costal, ensiform, nasal, trochlea, and those of the whole respiratory tract (with a few exceptions.) 2. Fibrous Cartilages: the intervertebral ligaments, the synchondroses, the cartilage of the ear, epiglottis and Eustachian tube, the Santorinian and Wrisbergian, the sterno-clavicular interarticular discs, and the cartilages of the inferior maxillary articulation. 3. Ligamentous discs: the interarticular ligaments of the lower jaw, wrist, and knee-joints (except the sharp edges of the latter,) the tarsal cartilages, the glenoid and cotyloid ligaments, and the fibro-cartilages of the sheaths and pulleys for tendons.

[1] Meckauer, (De penitiori cartilaginum structurâ, Breslau, 1836.) For measurements in different specimens, see Bruns, *l.c.*, p. 215.

[2] Bruns, Allg. Anat., p. 215.

[3] See Schwann, (Mikrosk. Untersuch, &c.;) Henle, (Allgem. Anat., p. 794.)

[4] These fibres are not demonstrated by snapping a cartilage asunder: the fibrous appearance of the broken surfaces, and the direction of the grain, are due to the arrangement of the cartilage corpuscles.

[5] Henle, Allgem. Anat., p. 798.

bral ligaments, they are parallel; in others, as the cartilages of the ear and epiglottis, they frequently bend, and seem as if they were matted together.

Between these fibrous cartilages and the ligamentous discs which are usually called fibro-cartilages, there are also intermediate structures: the fibres of the fibrous cartilages are not like those of tendinous tissue; those of the ligamentous discs are. But in some examples of the former (the sterno-clavicular and inferior maxillary interarticular ligaments, for instance), the chief substance is traversed by fasciculi of the fibrils of cellular tissue, and thus the transition is established from them to the ligamentous discs which are composed entirely of that tissue.

VIII. BONE—is composed of a basis of apparently homogeneous substance[1] (a compound of cartilage and earthy matter), in which are densely scattered minute cavities with delicate branched canals. The cavities (or bone-corpuscles[2]) are round or oval, and flattened; they measure from about $\frac{1}{2400}$ to $\frac{1}{1680}$ of an inch in length, from $\frac{1}{7200}$ to $\frac{1}{3000}$ in breadth,[3] and are about $\frac{1}{6}$ as thick as they are long.[4] They have somewhat jagged edges, from all parts of which there proceed fine branching canals (calcigerous canals[5]), which traverse the basis substance, and communicate irregularly with one another. The diameter of each canal, at its largest part, is between $\frac{1}{14000}$ and $\frac{1}{20000}$ of an inch; that of the smaller branches is between $\frac{1}{40000}$ and $\frac{1}{60000}$.[6] Müller[7] and Henle[8] have held that the corpuscles and canals are the chief seats of the bone-earth; but Mr. Smee[9] has rendered it more probable that they contain no solid matter, by showing that they can be filled with Canada balsam, and that, in the bones of those who have been embalmed, they are full of a waxen material; and Bruns[10] has found them quite empty in calcined bones, and perfectly transparent in very thin sections. At most, therefore, the earthy matter is more thickly deposited in the walls of the corpuscles and canals than it is elsewhere.

True bone always presents these elementary structures; its coarser structure is variously arranged in different parts, but always preserves a general character of lamellæ arranged round tubes and cellular spaces. The compact structure consists of osseous laminæ from $\frac{1}{5000}$ to $\frac{1}{3000}$ of an inch thick,[11] firmly united in concentric tubes, or in parallel planes, accordingly as the bone is cylindrical or flat.[12] They are most distinct on the exterior of bones, for there they are rarely interrupted by the canals for blood-vessels, which more internally are interposed in great numbers between them, and send off branches which pass through them. They appear either homogeneous or pellucid, or a little granular (like ground-glass), or sometimes very finely fibrous, like the

[1] An exception will presently be mentioned in which it seems to be fibrous. Dr. Carpenter (Principles of Human Physiology, p. 511,) thinks that it is entirely composed of cells adherent together, and filled with a perfectly homogeneous substance.

[2] They were obscurely seen by Leeuwenhoeck (Select Works, vol. ii. p. 129), and badly figured by Mascagni, (Prodromo, Tav. x., xix.) They were first clearly discerned by Purkinje, and described by his pupil Deutsch (De penitiori structurâ ossium, diss. inaug., Breslau, 1834.) They have since been chiefly illustrated by Müller, Miescher, Schwann, Henle, Smee, and Bruns.

[3] Miescher, (De Inflammatione Ossium, p. 42.) The measurements of Valentin, Krause, Bruns, and others are sufficiently confirmatory.

[4] Henle, Allg. Anat., p. 828.

[5] These also were discovered by Purkinje, and more clearly described by Müller.

[6] The latter from Müller, in Miescher, p. 268; the former from Krause. Both are confirmed.

[7] In Miescher, p. 268. [8] Allg. Anat., p. 829.

[9] On the Structure of normal and adventitious Bone, (Med. Gazette, Nov. 20, 1840.)

[10] Allgem. Anat., p. 241. [11] Müller, Deutsch, &c.

[12] The microscope was hardly needed for thus deciding the question whether bones are formed of lamellæ. The evidence of old Gagliardi was conclusive. Certain diseases separate these coarser lamellæ; and after softening in acid some may be dissected from all bones.

fibrous portion of the second class of cartilages;[1] and this last appearance is met with especially in cartilages ossified late in life, as those of the larynx, ribs, &c.

The blood-vessels of the compact structure of bone are conveyed in long cylindrical canals (*Haversian canals*[2]), of which the chief run straight in the spaces between the laminæ. These give off small branches which pass obliquely through one or more laminæ,[3] and form communications between the longer canals in different planes, so as to make up a coarse network of bony tubes permeating the compact structure; they open externally to admit vessels from the periosteum, and internally they merge by a kind of gradual expansion into the cells of the cancellous tissue. They are usually cylindrical, and they vary in diameter from $\frac{1}{200}$ to $\frac{1}{800}$ of an inch,[4] those nearest the surface being three or four times smaller than those near the cancellous tissue. Their walls, which are from $\frac{1}{300}$ to $\frac{1}{100}$ of an inch thick, are formed by from five to fifteen concentric lamellæ[5] of bone: and thus, each Haversian canal, whether its course be longitudinal or transverse, is surrounded, as the whole shaft of each cylindrical bone is, by a series of tubes arranged concentrically about its axis. The bone corpuscles lie thickly scattered and as if compressed between the lamellæ, and are so placed that one of their largest surfaces is turned from, the other towards, the axis of the canal. The calcigerous canals run both between and through the several lamellæ; and, since many of them are directed towards the axis of the Haversian canal, they look like a set of faint striæ radiating from it to the outermost lamellæ. Their apertures are discernible in small dots on each lamella, and, according to Krause[6] and Mr. Smee,[7] on the interior of the Haversian canals, into which they believe that some of them open.

Each Haversian canal contains an artery and a vein, which pass along its axis, surrounded by a very soft fat, to which they distribute minute branches. The vessels of the external canals are derived from the periosteum; those of the internal ones from the vessels of the medullary tissue; and the two sets freely anastomose.

As already said, the canals of the compact tissue gradually merge into the cells of the cancellous; and in both the type of structure is the same; for each cell also contains soft fat in which the minute branches of the medullary artery ramify, and the walls of each are formed by a series of delicate lamellæ, with corpuscles and canals arranged in the manner already described.

IX. Teeth. In no organs have the results of recent microscopic researches[8]

[1] Henle, (Allg. Anat., p. 826.)—Malpighi, (Anat. Plantarum, p. 20, &c.) De Lasône, (Mém. de l'Acad. R. des Sc., 1751, p. 98,) and others described bones as formed of laminæ composed of filaments; but these were artificially-made fibres, altogether different from those which are discernible but not separable.

[2] From Clopton Havers, who described them very fancifully in his Osteologia Nova, in 1729.

[3] They form the little processes which Gagliardi (Anatome Ossium, 1689) described as nails. The accuracy of his account of the structure of bones merits something very different from the ridicule commonly attached to his name: the description of Clopton Havers, which is usually praised, is not nearly so good.

[4] Miescher, &c. $\frac{1}{300}$ to $\frac{1}{500}$. Bowerbank, in Medical Gazette, Nov. 20, 1840.

[5] They are called lamellæ for distinction from the *laminæ*, just described.

[6] Handbuch der Anatomie des Menschen, b. i., p. 71.

[7] Medical Gazette, *l. c.*

[8] The chief discoveries were made coincidently by Purkinje, of Breslau, and Retzius, of Stockholm. The former published his observations in 1835, in the dissertations of Fränkel (De penitiori dentium hum. structurâ), and of Raschkow (Meletemata circa dentium evolutionem); the latter in the Transactions of the Royal Academy of Stockholm, and afterwards in Müller's Archiv, 1837. See B. & F. M. R. viii. 158. Interesting as were the facts they revealed, they are far surpassed in importance by the application which Mr. Owen, in his admirable Odontography, has made of the minute structure of the teeth as evidence in determining the general and specific relations of extant and fossil animals. It must be regretted that more use cannot be made of his work in so limited a report as this.

been so unexpected or so brilliant as in these. They have revealed structures before unknown in each of the three component parts of the tooth.[1]

Cement, crusta petrosa, or *bone,* forms the outermost layer of the teeth, and in its minute structure differs in no respect from common osseous tissue.[2] It visibly surrounds the whole fang, being thickest near the apex and on the grooves; and Mr. Nasmyth[3] has shown that it also extends in a very thin layer over the enamel of the crown. The existence of this substance which, if not always vascular, can probably easily become so, explains the possibility of ingrafting teeth upon vascular tissue.[4]

Enamel, invests only the crown of the teeth, and is composed of solid prisms or fibres, about $\frac{1}{5600}$ of an inch in thickness, set side by side upright on the ivory. One end of each prism is fixed in a little depression on the rough outer surface of the ivory; the other, which is somewhat larger, is turned towards the masticating surface of the tooth in that direction which is best for resisting external force. The course of the prisms is more or less wavy, their curves being, for the most part, parallel, but sometimes opposed. Most of them reach the surface of the tooth; and where they do not, small complemental prisms fill up, like wedges, the vacancies. In the perfect state the enamel contains a scarcely discernible quantity of animal matter, and its prisms are inseparably consolidated; but in young teeth it is soft, and may be broken up into its elements. In this early state it exhibits portions of a membranous animal substance, consisting of the cells in which each of the fibres was enveloped;[5] for the earthy matter is deposited, as it were, in a set of moulds, formed by the primary cells of the enamel-membrane; and as it accumulates, the cell-membrane is so far removed, that in the perfect tooth no trace of it can be discerned; unless, indeed, as Retzius suggests, the fine close-set transverse striæ which go round the whole or a part of each prism indicate the remains of it.

The *Dentine* or ivory forms the chief mass and body of the tooth. It is composed of a fibrous basis traversed by very fine, branching, cylindrical tubuli,[6] which run in an undulating course from the pulp-cavity, on whose interior they open, towards the adjacent part of the exterior of the tooth. Each tubule in its course outwards makes two or three curves, (*primary curvatures,* Owen,) and is, besides, bent at every part into numberless minute and very close undulations, (*secondary curvatures,* Owen;) but the course of those tubules which are adjacent to each other is as nearly as possible parallel. The chief ramifications of the tubules are dichotomous; but they also frequently give off minute branches, which, again, sending off smaller ones, fill up the spaces between the trunks. At the trunk, each tubule has an average diameter of about $\frac{1}{10000}$ of an inch, and the distance between each two tubules is about equal to the width of three. Towards the outer surface of the ivory, the tubules and their branches are immeasurably fine, and some of them pass from the ivory into the bone, and open into the canals of its corpuscles. Both the walls and cavities of the tubules, as well as the substance between them, are filled by the earthy constituent of the ivory which lies in fine granules. The basis of the intertubular substance is

[1] Malpighi (Anat. Plantarum Idea, p. 19, and Op. posthuma, p. 53,) seems first to have described all the three substances. He calls the bone *materia tartarea,* the enamel *substantia filamentosa,* and the dentine or ivory *subs. ossea.* The bone was subsequently overlooked till the time of Purkinje and Retzius.

[2] Its structure also always coincides with that peculiar to the bone of each species. (Owen, *l. c.* xx.)

[3] Three Memoirs on the Teeth and Epithelium; and Med.-Chir. Trans., vol. xxii.

[4] Owen, Odontography.

[5] Schwann, Ueber die Uebereinstimming, &c., p. 118.

[6] Malpighi is commonly quoted as having noticed the tubular or fibrous structure of the ivory; but whoever will examine the places already referred to will find that he never saw the tubules, and only discovered a coarse fibrous network like that of which he wrongly supposed the bones to consist.

[7] Henle, Allgem. Anat., p. 854.

composed according to Henle,[1] of bundles of flat, pale, granular fibres, whose course is parallel to that of the tubules.

x. Hair. Most of the hairs consist of two distinct substances: an external, cortical, hard, and fibrous part, and an internal, medullary, granular portion, on which their colour chiefly depends.[2] Moderately magnified, hairs look like empty tubes, but in fine transverse sections no central apertures can be seen.

The *cortical part* of the hair is fibrous. Very delicate longitudinal striæ may be traced on it, becoming more faint as they pass from the root to the tip, and in general invisible at a little distance from the latter. They are traceable through the whole thickness of the cortical substance to the very wall of the medullary portion, and indicate the outlines of the component fibres. The latter are, according to Henle,[3] each about $\frac{1}{1000}$ of an inch in breadth, flat, rigid, and brittle, with dark and rough edges. But they may probably be further split, for, after maceration in hydrochloric acid, Bidder[4] found the diameter of the thickest part of a single fibre to be only $\frac{1}{21000}$ of an inch; and Bruns[5] about $\frac{1}{9000}$; so that probably each of the fibres whose course is marked by the striæ is made up of several smaller ones. In some hairs moreover the fibres appear at certain parts, either irregularly, or at definite distances, enlarged; and thus the whole shaft sometimes assumes a beaded appearance.[6]

Besides these longitudinal striæ, indicating the fibrous structure of its cortical part, the surface of the hair is marked by transverse and oblique, and sometimes apparently spiral, wavy lines arranged in a very close series. Meyer[7] has shown that these are formed by the slightly projecting edges of tiers of minute scales, like those of the epidermis, but much smaller, which, being closely imbricated in whorls one over the other, invest the whole surface of the hair, and form a sheath around its cortical part, extending nearly to its tip. They make the hair look as if it were irregularly *hooped* round; or rather, when the hair is very strong, as if it were a closely-jointed reed.

The *interior medullary portion* of the hair is darker than the exterior and granular. It is composed, for the most part, of very minute globules, like pigment-granules or drops of oil agglomerated in small lumps. Sometimes these form one dark mass, continued along the whole shaft of the hair; but more commonly the mass seems broken up, so that there are intervals of different sizes along the axis of the shaft. These are sometimes filled by a substance like the cortical part, and the medullary matter then seems altogether deficient; but more often they are occupied by a colourless substance, clearer and softer than the exterior fibrous tissue. The diameter of this medullary part, when it is completely formed, is about $\frac{1}{3}$ or $\frac{1}{4}$ of that of the whole shaft; transverse sections of hairs exhibit it like a nucleus, with a clear ring around it; along its walls there are often complete pigment-cells, with clear nuclei and transparent membranes.[8]

At the tip, the hair gradually becomes more and more fine, and usually ends in a rounded point, at and near which neither striæ nor medullary substance can

[1] L. c., p. 856. Mr. Nasmyth considers it to be cellular, (Memoirs.)

[2] The medulla is absent in the fine hair over the general surface of the body, and at the very root and near the tip of all hair.

[3] Ueber die Structur und Bildung der menschlichen Haare, (Froriep's N. Notizen, April, 1840, and Allgem. Anatomie.)

[4] Einige Bemerk. über Entstehung, Bau, und Leben der menschlichen Haare, (Müller's Archiv, 1840, p. 538.)

[5] Allgem. Anatomie, p. 204. [6] Bidder, *l. c.*

[7] Froriep's Notizen, 1841, and in Henle, Allgem. Anat. p. 294. Henle coincides in this view: he had formerly (Fror. Notiz., April, 1840) considered the transverse striæ to be due to bands of a substance resembling the elastic tissue wound round the bundles of fibres forming the shaft; a view which closely agrees with that of Dr. Barry, who believes that the hair has the same general structure as the muscular fibre, &c., and is composed of a fasciculus of flat double-spiral fibres, held together by wider coalesced spirals. Valentin (Repertorium, 1841) entirely confirms Meyer's description.

[8] Meyer, *l. c.*

in general be seen. At the root it rather suddenly enlarges into a funnel-shaped extremity, which Henle has named the *knob* of the hair, and which is about three times as wide as the shaft. Just before the hair begins thus to enlarge, the transverse striæ produced by the outermost layers of imbricated scales, are very distinct and broad; but they suddenly cease to be discernible. At the same part the longitudinal striæ become finer, and seem to diverge. But, in addition to these, the knob is marked by coarse, dark, longitudinal striæ, which look like short, interrupted furrows, but which are produced by small, flat, metamorphosed nuclei, about $\frac{1}{1200}$ of an inch long, and $\frac{1}{18000}$ broad. They are largest at the upper part of the knob, and are often tortuous or connected together by fine filaments; lower down they are broader and oval or spindle-shaped, and lower still they pass into roundish or angular granules, like the nuclei of the rete Malpighii. They lie closely in a firm pellucid substance, and sometimes seem surrounded by cell-membranes, among which, in dark hairs, numerous pigment-granules are scattered.[1]

The *knob* of the hair and the nearest part of the shaft are pretty closely invested with a membrane, for which Henle proposes the name of the *sheath*, and of which some or the whole is pulled out when a hair is plucked from the skin. It is continuous with the epidermis, and may be regarded as the epithelium lining the hair-follicle. It is composed of two layers, of which the outer and thicker is yellowish, granular, and thickly set with superficial nuclei, the inner clear and much thinner, and perforated by numerous round, oval, and elongated apertures, but having no trace of cells or fibres. Below, the two layers are united together, and with the exterior of the knob; above, a small space filled with fatty matter intervenes between them and the exterior of that part of the shaft of the hair which is below the surface of the skin.

Cellular and fibrous tissues, or fibro-cellular tissue.[2] All the tissues which have passed under these names nearly agree in their microscopic structure: their chief anatomical differences depend on the mode on which their elements are aggregated. The common material of which they are composed consists of fine, transparent, undulating, cylindrical filaments, from $\frac{1}{30000}$ to $\frac{1}{10000}$ of an inch in diameter.[3] They are generally collected in fasciculi, from $\frac{1}{3750}$ to $\frac{1}{7500}$ of an inch wide, the filaments in which are connected by a firm, structureless cytoblastema; and the fasciculi either merely adhere together, or, as in the case of tendons and other similar tissues, dense bundles are united by others of more loosely-connected filaments placed in their interstices. In different parts either dense or loose fasciculi, or both, are woven into cords, membranes, &c.

In certain situations the elements of the fasciculi are bound together by small filaments of another kind, which, from their supposed origin, may be named *nucleus-filaments*, and which Henle[4] discovered by immersing fibro-cellular tissue in acetic acid, so as to make its proper fasciculi transparent. The latter are thus seen to be severally, and sometimes collectively, constricted at pretty re-

[1] See also on this subject some remarks by Mr. Busk, in the Microscopic Journal, vol. i. p. 26, and vol. ii.; and Simon, Müller's Archiv, 1841, and B. & F. M. R. xiii. 525.

[2] This compound term seems preferable to either of its components. *Fibrous* and *cellular* are both terms applicable to many tissues besides this; but *fibro-cellular* may well indicate a tissue composed of fibres which are in some cases woven so as to inclose cellular spaces.

[3] Treviranus (Beiträge, bd. i.), Jordan (Ueber das Gewebe der Dartos, Müll. Arch. 1831), Gerber (Allg. Anat., p. 123), Henle (Allg. Anat., p. 348), and many others nearly agree in these measurements. Lauth (Nouv. Manuel de l'Anatomiste, &c.) and Krause (Handb. der Anat., bd. i.), make them much greater.

[4] The best tissue for demonstrating this is that which forms the fine fibres connecting the nerves and vessels at the base of the brain; but a similar arrangement exists in nearly every variety. See on that in the pia mater of the spinal cord Purkinje and Luening, (Valentin's Repert. 1841.)

gular distances by one or more dark filaments, very similar to those of elastic tissue, which either wind spirally round them, or encircle them with distinct rings. This arrangement is most frequent when the fasciculi form cords; in other cases (for instance, where many fasciculi lie parallel, as in tendons and strong membranes,) similar dark filaments run, not spirally, but straight along the interspaces between each two fasciculi; and in others (in which the tissue is very lax,) the dark filaments are very numerous and run tortuously, or are even twisted up into little balls.

Other forms discerned in fibro-cellular tissue thus treated with acetic acid, are those of oval corpuscles, like dark cytoblasts, and of elongated, curved, or tortuous striæ, drawn out at their ends. The corpuscles commonly lie in rows with their axes parallel to the proper fasciculi of the tissue; and very often they are connected by longer and more slender bodies, so as to form continuous, undulated, and spiral filaments. Henle, therefore, (to whom all the account of these fibres is due,) has little doubt that the elongated nuclei represent the early state of the dark, *spiral,* and *interstitial nucleus-filaments;* and that, on the other hand, there is a regular gradation from the latter to the filaments of genuine elastic tissue; for all the transitional forms are found mixed with the proper fasciculi in different varieties of fibro-cellular tissue. Again, there is in another direction a transition from the fibro-cellular to the muscular tissue: for though most of the organs composed of the former seem to serve passively their purpose in the economy, yet some, though not distinguished by their structure, have more active properties and contract under appropriate stimuli. Such are the fibro-cellular tissues of the skin and of the dartos, which in their minute structure exactly resemble, on the one hand, the ordinary connecting fibro-cellular tissue, and on the other, the iris[1] and the longitudinal and circular contractile coats of veins and lymphatics, from which there is an easy gradation, both in function and structure, to the genuine organic muscles.

XII. ELASTIC TISSUE. (*Tissu jaune élastique.*) In the preceding section the lower forms of this tissue, of which individual fibres are connected with fasciculi of fibro-cellular tissue, are described; in the higher forms of it its peculiar fibres compose independent fasciculi, with which subordinate quantities of fibro-cellular tissue are mixed.[2]

The fibres of elastic tissue resemble, in general, flat, solid[3] bands, of various sizes, from $\frac{1}{3700}$ to $\frac{1}{22500}$ in width.[4] They are peculiarly distinguished by their sharp, smooth, dark edges, their frequent appearance of branching, their tendency to form arches or curls when their extremities are free, and their brittleness, so that they easily break into short pieces with abruptly-cut-off extremities. According to the predominant arrangement of their fibres, Henle divides all the examples of elastic tissue into three varieties. The first, of which the type is in the true vocal ligaments, differs only in degree from the interstitial form of the tissue which is combined with the fibro-cellular fasciculi. Its fibres are undulated, narrow, and very rarely branched or anastomosing; they form, however, independent fasciculi, which are connected by small quantities of the

[1] Krause, Handb., b. i. On the dartos, see Bowman, *l. c.,* who says that it contains organic-muscular fibres.

[2] Henle, Allg. Anat. 399. The best previous accounts were those of Eulenberg (De tela elastica, diss. inaug., Berol. 1836), Schwann (Encycl. Worterb. der Med. Wissenschaften, *art.* Gefässe), Räuschel (De arteriarum et venarum structurâ, diss. inaug. 1836), and Lauth (L'Institut, t. ii. 1834), by whom the peculiar minute structure was first discerned.

[3] Purkinje and Räuschel (*l. c.*) believe that they are tubular, having seen a small dark spot, like a section of a canal, on transversely divided portions of the fibres, and a line of points arranged along the middle of their surfaces. The latter observation, which has been often repeated by Valentin (Repertorium, ii.) and others, may also indicate that the fibre, apparently simple, is composed of two interlacing spirals, like the muscular and others in Dr. Barry's system.

[4] Henle, whose measurements are generally nearly confirmed.

fibro-cellular tissue. In the second variety, which includes the ligamenta subflava, (the most perfect of all the examples of the tissue,) the fibres are large and widely arched, and frequently give off branches. In the third, including the elastic coat of the blood-vessels,[1] the fibres are of smaller size than in the second, and very frequently anastomose so as to form, while they for the most part maintain one general direction, a network with meshes of various size.[2]

When the elastic fibres seem to branch it is doubtful whether a single fibre really splits, or a double fibre divides into its two component parts. Valentin,[3] Lauth, and Eulenberg maintain the latter view; Schwann, Bruns,[4] Henle, and most others, the former; and they regard a genuine branching and coalescing as the peculiar characteristics of these fibres. In Dr. Barry's opinion the reticular arrangement of some of the varieties of elastic tissue, whose fibres have the same general characters as those more simply arranged, affords a striking example of the breaking up of double spiral fibres into networks, which would find a close analogy in the formation of the reticular ducts of vegetables.

XIII. Muscular tissue. The transition in function, though not in structure, from the passive to the active fibro-cellular tissue, and from the latter to the organic muscular tissue has been already alluded to. Among the tissues of the present class there are similar gradations in regard to their several functions;[5] but by their structure they may be definitely arranged in two main divisions: the first including the muscles of organic life, which (with one exception) consist of simple, smooth filaments; and the second comprising the muscles of animal life and the heart, which consist of compound and apparently striated fibres, or tubes including fibrils.

A. *Muscles of organic life.* No full account of this tissue was given before that by Henle, who investigated it in his search after the properties of arteries.[6] He says that the fibres, in their most perfect form, are flat, from $\frac{1}{4700}$ to $\frac{1}{3100}$ of an inch broad, very clear, granular, and brittle, so that when they break they often have abruptly-rounded or square extremities. Some of them are uniform; a few bear nuclei; the majority are marked along the middle, or, more rarely, along one of the edges, either by a fine, continuous, dark streak, or by short, isolated, dark lines, or by dark points arranged in a row or scattered; and between these three kinds of marks there are such gradations as prove that they

[1] To be more minutely described in another section.

[2] The elastic tissue occurs in distinct fasciculi in several parts besides these mentioned as types of its various arrangements. For instance, it is found in the bands and ligaments of the larynx and respiratory passages, in a layer surrounding the œsophagus, and in one between the muscular and mucous coats near the cardia and the anus, in the fascia lata, and several other fasciæ, where, besides the interstitial and spiral nucleus-filaments, many independent fasciculi exist. In these parts the arrangement is on the plan of the first variety. Similar fasciculi, but usually arranged in networks, are found mixed in the tissue beneath the epithelium of several serous membranes, as the pleura costalis, the peritoneum on the anterior wall of the abdomen and on the fundus of the bladder, the ligaments of the liver, the intestines, &c. And lastly, fasciculi like those of the ligamenta subflava, are mixed with the proper tissue of many parts of the skin. (See Eulenberg, Lauth, Henle, *l.c.*, and Bowman, Cycl. of Anat., *art.* Mucous Membrane.)

[3] Repertorium, bd. ii. &c. [4] Allg. Anat., p. 75.

[5] The several members of this ascending series may be thus arranged:—1. Common connecting fibro-cellular tissue. 2. The tissue of the skin contracting under the influence of cold and mental emotions. 3. The dartos, which contracts more forcibly under the same influences. 4. The contractile coats of arteries and veins. 5. The iris, which contracts when its nerves are directly irritated, and with the quickness of muscle. 6. The lower organic muscles of the gland-ducts, &c.; and the higher of the stomach, urinary bladder, &c. 7. The muscle of the heart. 8. The perfect muscles of animal life. But it will be observed that the progress in structure does not coincide with this arrangement according to function. The tissue of the iris, for instance, is exactly like the lowest fibro-cellular tissue.

[6] Ueber die Contractilitat der Gefässe, (Casper's Wochenschrift, Mai 1840, and B. & F. M. R. vol. x., and Allg. Anat.)

have all the same origin from nuclei. Fibres such as these are collected in divers numbers in fasciculi, upon which the dark lines just mentioned sometimes form, by branches which they give off and receive, a sort of network, and sometimes run tortuously, like the nucleus-fibres of the fibro-cellular tissue.

Fibres of organic muscle, such as are here described, form the proper contractile coats of the digestive canal from the middle of the œsophagus[1] to the external sphincter ani[2], of the urinary bladder, the trachea and bronchi, the ducts of glands, the gall-bladder, the vesiculæ seminales, the pregnant uterus,[3] the arteries and the veins.[4]

B. *Muscles of Animal Life.* The voluntary muscles are composed of fleshy bundles inclosed in coverings of fibro-cellular tissue, by which each is at once connected with, and isolated from, those adjacent to it. Each bundle is again divided into smaller ones, similarly ensheathed and similarly divisible; and so on, through an uncertain number of gradations, till, just beyond the reach of the unaided eye, one arrives at the *primitive fasciculi*, or the *muscular fibres* peculiarly so called, the first fixed form in the system.[5]

1. *Structure.* The primitive fasciculi consist of tubes of delicate structureless membrane,[6] inclosing a number of filaments. They are cylindriform or prismatic,[7] with five or more sides, according to the manner in which they are compressed by adjacent fasciculi. Their breadth varies in different animals, from $\frac{1}{80}$ to $\frac{1}{1500}$ of an inch; in man from $\frac{1}{200}$ to $\frac{1}{500}$, the average of the majority being about $\frac{1}{400}$.[8] Their most striking, though not constant,[9] characteristics are their pale yellow colour, and their being apparently marked by striæ, which pass transversely round them, in slightly curved or wavy parallel lines, from $\frac{1}{10000}$ to $\frac{1}{12000}$ of an inch[10] apart. Other, but generally more obscure, striæ also pass longitudinally over the tubes, and indicate the size and direction of the filaments, or primitive fibrillæ of which the primitive fasciculus is composed.[11]

The *primitive fibrils* are the proper contractile tissue of the muscle. Each of them is cylindriform, but somewhat flattened, and about $\frac{1}{18000}$ of an inch in its greatest thickness.[12] They are marked by transverse impressions, which are at exactly the same distance apart as the striæ on the surface of the fasciculus.

[1] On the Fibres of the Œsophagus, see the observations of Mr. Gulliver, Proceed. of the Zoological Society, part viii.

[2] In the fibres of the stomach and intestines there is often an obscure division into fine rigid fibrillæ; in those of the upper part of the ureters there is an approximation to the fibro-cellular fasciculi: for they gradually split into undulated fibrils, (Henle.)

[3] Schwann (Mikrosk. Unters. p. 167), Lauth (Müll. Arch. 1835, Jahresb. p. 3), and Baly, in Müller's Physiology. On those of the unimpregnated uterus, which resemble the undeveloped muscular fibres of the embryo, see Purkinje and Kasper, Froriep's N. Notiz. Marz 1842.

[4] See further, p. 266.

[5] They were first described by Hooke, in 1678. The account of them became gradually more accurate in the successive descriptions of Leeuwenhoeck, Muys, Fontana, and Prochaska.

[6] Valentin, (Hecker's Annalen, 1835;) Schwann, (Mikrosk. Unters.) It is the sarcolemma of Mr. Bowman, (Philos. Trans. 1840,) whose complete description of it is generally confirmed by Henle, (Allg. Anat. p. 579.) The latter, however, adds that it is often absent; and that, when present, it bears numerous elongated nuclei like those on the fasciculi of organic muscle.

[7] Muys, (Musculorum Artificiosa Fabrica, pl. i.;) Prochaska, (De Carne Musculari, p. 45;) Bowman, (*l. c.* Pl. xvi. fig. 1, &c.)

[8] Bowman, (*l. c.*, p. 460.) These measurements are generally confirmed by those of Raspail, Schwann, Skey, Henle, and others.

[9] See Gulliver in Gerber, p. 235. See also Martin Barry, (Proceedings of the Royal Society, Jan. 6, 1842.)

[10] Schwann, in Müller's Physiologie, bd. ii. p. 33. Nearly confirmed by Prevost and Dumas, Lauth, Bowman, and others.

[11] Exceptions to these general characters are met with sometimes, but are not yet explained. See especially Gulliver, (*l. c.*,) and Henle, (Allg. Anat., p. 580.)

[12] The measurements of Henle, Lauth, Ficinus, Bruns, and several others, pretty nearly agree with this statement.

Hence it is generally concluded that, as Fontana believed, the striated appearance of the primitive fasciculi is produced by the filaments being so apposed that the transverse marks on all those near the surface lie at exactly the same levels.[1] At present there is much question of the true structure of the fibrils, and of the source of their seeming constrictions, or transverse impressions. Some, indeed, have denied the existence of such constrictions, except when the muscle is contracted, or in some particular condition after death;[2] but the majority differ only in their explanation of them. Some believe that the fibrils are rows of corpuscles, or discs, connected by a homogeneous transparent substance;[3] and others, following Dr. Barry,[4] regard the fibrils as a peculiar form of his grooved and compound filament; each being composed of two spiral threads, wound in opposite directions, and interlacing. In the former view the transverse marks on the fibrils, and the ordinary striæ on the fasciculus correspond to the spaces between the discs; in the latter to the spaces between each two successive turns of one of the spiral threads; for since each filament has its edge turned outwards, only one set of coils can at first be seen.

Each primitive fasciculus contains several hundred of the fibrils; and when fully formed they fill all the cavity of the sarcolemma with the exception of very small interspaces, which seem occupied by a glutinous pellucid fluid.[5] It is only in immature fasciculi that there is an appearance of a central cavity, which is filled either by fluid or by minute granules.[6]

Where a muscle is affixed to a tendon, each primitive fasciculus of the former terminates in an abruptly rounded extremity, which is embraced by a fasciculus of tendinous fibrils, expanding and inclosing it as in a sheath.[7] The coarser fasciculi of tendinous filaments are also continuous with the fibro-cellular tissue which intervenes between the secondary fasciculi of the muscle.

2. *Action.* The actual phenomena of muscular contraction have been often examined in both the voluntary and the involuntary muscles, but the mode in which it is effected is still disputed. Hales,[8] and after him Prevost and Dumas,[9] described the contraction as the result of the primitive fasciculi being thrown into zig-zag lines. But the view of others, (which has been especially illustrated by Mr. Bowman[10]), is that the change is effected by an approximation of the constituent parts of the fibrils (whether discs or coils) which, at the instant of contraction, without any alteration in their general direction, become closer, flatter, and wider; a condition which is rendered evident by the approximation of the transverse striæ seen on the surface of the fasciculus, and by its increased breadth and thickness. The appearance of zigzag lines is referred by Dr. Allen Thomson and Mr. Bowman, to the relaxation of a fibre which has been recently contracted, and is not at once stretched again by some antagonist fibre, or whose extremities are kept close together by the contractions of other fibres.

[1] Dr. Barry, however, (Proceed. of Roy. Soc. Jan. 6, 1842,) says there are states in which the fibrillæ have no share in the appearance of transverse striæ, and in which they are due to the flat and grooved filaments, which, he believes, are wound spirally and interlaced around the bundles of fibrillæ. Some of Henle's observations are strongly confirmatory of this view (Allg. Anat. p. 583); and perhaps Mandl's account (Anat. Microsc. p. 14,) is drawn from a similar appearance.

[2] Treviranus, (Beiträge, bd. ii.;) Ficinus (De Fibræ muscularis forma et structura;) Valentin, (De Functionibus Nervorum;) Skey, (Phil. Trans. 1837.)

[3] For examples (each, however, with some modification,) Lauth, Krause, Turpin, Schwann, Müller, Bowman, Bruns, and Gerber.

[4] *L. c.*, and Philosophical Magazine, April, 1842. Henle, (Allgem. Anat. p. 582,) confesses it impossible to decide the question, but he seems to think that the fibrils are not composed of rows of globules, but derive that appearance from being finely wrinkled. Fontana, also, (Sur le Venin de la Vipere, t. ii. p. 229,) remarked that the appearance seemed due sometimes to rows of globules, and at others to the mere wrinkling of cylinders.

[5] Skey, Bowman, Henle.

[6] Valentin, Skey, Gerber, Henle.

[7] Valentin, Bowman, Bruns, &c.

[8] Statical Essays, vol. ii. p. 59.

[9] Mém. sur la Phénomènes qui accomp. . . de la Fibre musculaire. (Magendie's Journal, vol. iii. p. 301.)

[10] Phil. Trans. 1840 and 1841. See also Owen and Allen Thomson, in Hunter's Works, by Palmer, vol. iv. p. 261, note; and Martin Barry, *l. c.*

Valentin[1] adopts a middle course of explanation, believing that the production of inflexions in the fibres depends on the degree of their contraction. Ordinary and moderate muscular contraction, he says, is effected by a vermiculation passing very rapidly over the whole length of the fibre, and in this act, the transverse striæ are approximated; but when the contraction is greater, geniculate (zigzag) inflexions are produced, and become the more acute and close the more violent the contraction. All agree in the account of the contraction commencing either at the extremity or at several intermediate parts of a fasciculus, and thence travelling over its whole length; so that the entire act is rather a succession of contractions and dilatations than a single contraction.[2]

In relation to the question of the connexion between the muscular contractility and the nervous influence, the following observation by Valentin may be recorded, but perhaps should not be built upon without much caution.[3] He galvanized on the field of the microscope several minute portions of muscle, and observed whether they contracted or not, which it was possible to do with a moderately high power. Then, with a much higher, he examined whether, in each portion of muscle, there were any portion of nervous fibre included; and he found that in every case in which nerve was present contraction took place, but that when the portion of muscle contained no nervous fibre, the galvanism was ineffectual to produce the same result.

XIV. NERVE.[4]—A. *Structure.* 1. *Cerebro-spinal nervous fibres.* In a nerve belonging to this system one finds numerous fibres inclosed in a common membranous sheath, or *neurilemma.* The latter is composed of fasciculi of fibro-cellular tissue, closely woven together in a generally longitudinal direction, and somewhat more wavy than the filaments in most varieties of this tissue; so that they give the nerve a satin-like glistening aspect. Commonly, also, it has a characteristic striated appearance, (the striæ being transverse or oblique, or even spiral), from a close alternation of bright and dark streaks, which are due to the fibres of the neurilemma being wrinkled,[5] or to the primitive nervous fibres within it being arranged in a slightly tortuous manner,[6] and which are destroyed by stretching and by maceration.

Within this common sheath the nervous fibres are assorted in subordinate bundles of nearly equal size, each of which is inclosed in a separate secondary sheath, continued from the external one, but composed of much finer and less perfectly developed tissue. Within each of these there are again subordinate divisions of the fibres similarly enclosed, till at last, arriving at the primitive fibres, each of these is invested by a covering analogous to the outer neurilemma, but possessing a fineness of structure proportioned to the minuteness of that which it envelopes. It is a most delicate membrane, exactly pellucid, and, in general, seems structureless; but Schwann[7] believes he has seen elongated nuclei in it, and Valentin[8] says he can, in favorable circumstances,

[1] De Functionibus Nervorum, p. 132. His observations were chiefly made by galvanizing portions of muscle on the field of the microscope.

[2] On the application of this to ruptures of muscles, see Mr. Bowman's Paper, in the Phil. Trans. 1841.

[3] And the more since it has not been confirmed, and Mr. Bowman says that in his experiments fibrils contracted after, as he believed, all adjacent tissues were completely removed. It is possible, however, that these were such spurious contractions as, according to Valentin, are produced merely by the action of water.

[4] A better example of the progress of minute anatomy in four years cannot be found than in a comparison of the following account with that in a review, in Vol. VI. of this Journal, of what was known on the same subject in 1838.

[5] Prevost and Dumas, Valentin (Ueber den Verlauf und die letzten enden der Nerven. Nov. Act. Acad. Nat. Cur. 1836, p. 66.)

[6] Fontana, (sur le Venin, &c.;) Burdach, (Beitr. zur Mikrosk. Anat. der Nerven, Ann. des Sc. Nat. 1837, p. 117;) Henle, (Allg. Anat. p. 616.)

[7] Mikrosk. Untersuch.

[8] In Sömmering (vom Baue des mensch. Korpers, bd. v. p. 5.)

see that it has a fibrous appearance, "as if two sets of fibres crossing one another ran screw-like round the nervous tube;" a view which remarkably confirms the observations of Dr. Martin Barry.[1] A longitudinal arrangement of fibres is also sometimes discernible.[2] The proper substance of the nervous fibre, which is contained within the sheath just described, is, according to Valentin,[3] a clear, opalescent, viscous, homogeneous, oil-like substance, without any trace of corpuscles, vesicles, or fibres. Henle,[4] confirming, on the whole, this account, describes each nervous fibre as resembling, in the living or just-dead state, a fine cylinder of clear glass, being pellucid and colourless, and having simple, well-defined, dark edges. But they agree that it is only immediately after removal that this simplicity of composition can be discerned; for very quickly, and especially when they are immersed in water, the fibres, as if by a coagulation of their substance, assume the appearance of being composed of two different materials.[5] In this state their edges are marked by dark parallel outlines, within which again are two internal lines, parallel to them and to one another, which make each fibre look like a tube.

Regarding this last as their natural condition, Remak,[6] whose description agrees very nearly with that of Fontana,[7] considers each fibre as a delicate contractile tube, containing a pale flattened band (the *primitive band*) which, he thinks, is composed of several very fine solid filaments. Rosenthal,[8] also, whose description was written under the guidance of Purkinje, regards each fibre not as homogeneous, but as composed of an *axis-cylinder*, of moderately firm nervous matter, inclosed in a cortical portion of softer substance (*vagina medullaris*): and nearly the same view is entertained by Hannover[9] and by Schwann. Dr. Martin Barry, on the other hand, believes that the nervous fibre possesses a structure analogous to that of the primitive fasciculus of muscular fibrils; that the outer white substance (*vagina medullaris*) is a fasciculus of double spiral flat filaments, and the central portion (*Remak's band*), a filamentous material from which they are continually being given off.

Whatever be the true original structure of the nervous fibre, its cylindrical is soon exchanged for a beaded or varicose form, from the accumulation of the contents into separate masses, which dilate small portions of the sheath while the intermediate spaces collapse; and these changes proceeding, the contents of the sheaths, either spontaneously or by the influence of the fluid in which they are placed, assume a granular or curdy appearance, and may be easily pressed out. From the supposition that these states are natural arose the errors in the first descriptions of minute nervous structure by Ehrenberg[10] and others. The changes take place with the more facility, the more coarsely the fibres are dissected, and the finer and more delicate their investing sheaths are.[11] They are therefore quickly produced in all the fibres of young subjects, and in those

[1] Henle has a less distinct description of a similar arrangement of interlacing spiral fibres around the primitive fibre, (Allg. Anat. p. 620.)

[2] Rosenthal, Valentin. Valentin (Repertorium iii. 262) believes he has sometimes seen movements as if produced by ciliæ on the inner surface of this primary sheath; but he has not much confidence in his observation, (Sömmering's Anatomie, bd. v. p. 6;) and it has not been confirmed by others.

[3] In Sömmering, p. 5.

[4] Allg. Anat. 617. See also Klencke, Ueber die Primitivnervenfasern.

[5] A fact remarkably confirmatory of this view, and of which the first notice is due to Mr. Clift, is the perfect transparency of the living or just-dead retina; it assumes the gray opacity by which it is generally known in the same time as the change here described is supposed to take place in the nervous fibres.

[6] Obs. Anat. et Micr. de Systematis Nervosæ Structurâ, Froriep's N. Notizen, Juni 1838, and in several other papers. See also B. and F. M. R. Vol. VII. p. 500.

[7] Sur le Venin de la Vipere, &c.

[8] Diss. Inaug. de formatione granulosâ in nervis, &c. in Valentin's Repertorium, 1840, p. 76, &c.

[9] Müller's Archiv, 1840, p. 552.

[10] Beobacht. einer auffallenden . . . Struktur des Seelenorgans, Berlin, 1836.

[11] On the influence of particular agents in producing these changes, see Burdach, *l. c.* Valentin, and Henle who also gives a full account of the evidence on all the controverted subjects.

of the brain and spinal cord, and of the nerves of peculiar sensation at all ages. Hence Ehrenberg was led in his first essay to make a marked distinction between the *varicose fibres* in these nerves, and the cylindrical fibres of others; and although his own later observations, and those of all others, prove that all nervous fibres are alike, in their natural state, cylindrical, yet, as Remak has observed, the degrees of facility with which they severally assume the beaded form may serve to distinguish them as well as if they possessed it during life.

The size of the cerebro-spinal primitive nervous fibres is variable, and the same fibres have not the same diameter through their whole length. They are generally largest at their peripheral extremities and in their course along the nervous trunks, where the majority measure, in round numbers, from $\frac{1}{2000}$ to $\frac{1}{3000}$ of an inch in diameter.[1] They gradually decrease in size as they approach the brain, whether directly or through the medium of the spinal cord; and in the brain itself they continue to grow less as they pass through the medullary to the cortical part, so that in the former they measure (on a similar general average) from $\frac{1}{7000}$ to $\frac{1}{8000}$, and in the latter not more than $\frac{1}{11000}$ of an inch.[2] The fibres of the olfactory and optic, and, in a less degree, those of the auditory nerves, are equally small in every part of their course, and thus resemble, in size as well as in structure, those portions of the other nervous fibres which are continued into the nervous centres. Remak[3] also observes that the primitive fibres for common sensation are smaller, and become more readily varicose than those for motion; and Henle[4] confirms this in general, but adds that there is no distinct line of demarcation between the two sets, for that in all mixed nerves fibres of every gradation of size occur.

2. *Organic or sympathetic nervous fibres.* The general neurilemma is tougher in this than in the preceding class of nerves, and has a layer of circular fibres in addition to the longitudinal ones. The several fibres in a nervous trunk are seldom assorted in secondary or more subordinate fasciculi.[5]

In the nerves of this system there are, besides the fibres supposed to be peculiar to them, many of the same kind as those already described; and the latter, which are always finer than the average of the ordinary nervous fibres, vary in their proportionate number in different parts. In the roots of the sympathetic they do not form more than $\frac{1}{4}$ or $\frac{1}{5}$ of the fasciculus; in most of the nerves proceeding from the ganglia to the viscera they are more numerous; in the main trunks, such as the splanchnic and the cardiac nerves, they greatly predominate; and the ciliary nerves, as well as those of the lachrymal and mammary glands and those which accompany the blood-vessels, are wholly composed of fibres like those of the nerves of common sensation.[6]

The nature of the other and peculiar fibres in the branches of the sympathetic nerves has been much disputed. They are described by Remak,[7] Pappenheim,[8] Müller,[9] and some others as distinguished by their fineness, paleness, and yellowish hue, and by the constant absence of the lateral, dark lines which give the cerebro-spinal nerves the aspect of tubes. Rosenthal and Purkinje[10] consider them to be formed by an axis-cylinder similar to that of an ordinary nerve-fibre, and surrounded by a granular nucleated sheath; so that they differ from cerebro-spinal nerve-fibres only in the absence of the outer medullary layer (*vagina medullaris,*) of the nervous substance. Henle thinks they are a peculiar set of nervous fibres arising from the central organs, put into connexion in the gan-

[1] Wagner and Krause, (Müller's Physiology, by Baly, i. 597;) Nasse, (Müller's Archiv, 1839, p.245.) The measurements of several others agree. For comparison, see Ehrenberg, (*l. c.*) and Wagner, (in Burdach's Physiologie, bd. v.)

[2] Treviranus, (Beiträge, hft. ii.;) Henle, (Allg. Anat.)

[3] Vorlaufige Mittheilungen, (Müller's Archiv, 1836, p. 145,) and in other papers.

[4] Allg. Anat. p. 669. The difference is most perceptible in comparing the fibres of the anterior and posterior roots.

[5] Henle, *l. c.* p. 630. [6] Ibid. *l. c.* p. 631-2.

[7] Neurologische Notizen; Froriep's N. Notizen, Aug. 1837, and elsewhere.

[8] Die specielle Gewebelehre des Gehororgans.

[9] Archiv, 1839. Jahresb. cciv.

[10] De formatione granulosâ, &c., in Valentin, *l. c.*

glia, and destined for the contractile fibro-cellular tissue and the blood-vessels. He calls them *gelatinous* nervous fibres, and describes them as pellucid, flat fibres, between $\frac{1}{5600}$ and $\frac{1}{3750}$ of an inch in breadth, with numerous oval or round nuclei arranged at pretty regular distances on their flat surfaces, and more or less elongated and approaching the characters of other nucleus fibres. Sometimes also, he says, the nerve-fibre is split at its extremity like the fasciculus of fibro-cellular filaments in course of development.

On the other hand, Valentin,[1] who is, on both anatomical and physiological grounds, the chief opponent of the notion of a peculiar set of fibres in the organic nerves, regards those which are so described by others as merely the imperfectly developed filaments of fine, fibro-cellular tissue, which are formed into sheaths for the investment, not of collections of nervous fibres, as in other nerves, but of each nervous fibre separately. These sheaths he believes to be continued over the fibres from ganglion globules presently to be described.

The conflicting views may be probably reconciled according to the explanation in a recent description by Volkmann and Bidder,[2] which has received the confirmation of E. H. Weber. It seems most probable that the fibres described by Remak are those of the investing membrane of the true sympathetic fibres, as Valentin holds, and which, in old frogs, exactly resemble the common wavy filaments of fibro-cellular tissue. (E. H. Weber.) But the true sympathetic fibres are still distinct from the cerebro-spinal, and were probably well-discerned by Rosenthal and Purkinje. According to Volkmann and Bidder, they are distinguished by their fineness, (their diameter being constantly about half as great as that of the cerebro-spinal nerves,) their paleness, the absence under all circumstances of a double contour, the very small quantity of curd-like contents which they exhibit when decomposed, and their yellowish-gray colour when they lie in bundles. The difference in size, they add, is so distinct that though both sets of fibres vary to some extent, there is not nearly a complete series of gradations between them.

B. *Course.* The observations of Fontana and of Prevost and Dumas, confirmed by those of Müller[3] and others, prove that the nervous fibre is uninterrupted from its central to its peripheral extremity, and that in all that course there is no anastomosis or confusion of substance between any two primitive fibres—facts proved by both experiment and sight. They have received additional confirmation from Kronenberg[4] and Valentin,[5] the latter of whom examined particularly the nervous fibres in the recti and other minute muscles of very small animals.

The microscope has also materially assisted Volkmann[6] in proving certain facts respecting the course of nervous fibres which could not have been discerned by ordinary dissection. Such, for instance, is the fact, that in several instances fibres proceed for a certain distance from the centres and then, without passing into the substance of any organ, form loops and return again to the brain or spinal cord; so that different parts of the nervous centres may be supposed to be connected by long, nervous fibres, arranged in arches, of which the extremities are at the centres, and the arcs far external to them. This is the case in the plexus of the descendens noni with the cervical nerves, through which some branches of the latter ascend to the brain; and probably also in the arched fibres which form the inner border of each optic tract, and the posterior border of the chiasma; and in those fibres which experiment has nearly proved to proceed in arches from the posterior to the anterior columns of the cord, through the roots of the nerves.[7] It has thus also been made not improbable that the

[1] Ueber die Scheiden der Ganglienkugeln, &c. (Müller's Arch. 1839,) and elsewhere.

[2] Verhaltniss des nervus sympathicus zu dem übrigen nervensysteme beim Frosche, &c. in Froriep's N. Notizen, Marz 1842.

[3] Physiologie, bd. ii.

[4] De plexuum nervorum structurâ, Berol. 1836, and Müll. Arch. 1837, Jahresb. ii.

[5] Ueber den Verlauf. &c. p. 77.

[6] Beob. und Reflexionen ueber Nervenanastomosen, (Müll. Arch. 1840, p. 510), and B. and F. M. R. Vol. XII. p. 239. See also Appendix, B.

[7] Magendie, (Comptes Rendus, Mai et Juin, 1839;) and Kronenberg, (Vers. ueber die motorische und sensibeln Nervenwurzeln, (Müll. Arch. 1839.)

two retinæ are connected by similar arcs, both extremities of which are peripheral; and perhaps there are some other similar instances.[1]

c. *Modes of Termination.* 1. *Peripheral.* This arched arrangement of the nervous fibres is repeated in the substance of the organs in which they are said to terminate; for, as far as they have been examined, the general, if not the constant, mode of termination is as follows: After repeated divisions into smaller bundles of fibres, the fasciculi, which consist of from two to six fibres each, form plexuses, whose arrangement bears a general resemblance to that of the elements of the tissue in which they are placed. These are the *terminal plexuses.*[2] Then each fasciculus of the plexus breaks up into its primitive fibres, and each fibre, either after passing over several elementary structures of the containing tissue, or, as in the sensitive papillæ, the iris, &c., after forming a single narrow loop, returns to the same or an adjoining plexus, and pursues its way back to the nervous centre, from which it set out. In other words, each fibre forms an anastomosis or *terminal loop* with another from the same or a neighbouring fasciculus. There is thus, strictly speaking, no more termination of nerves than of blood-vessels; both alike form circles. The characters of the fibres are scarcely altered in the substance of the organs receiving them: their sheaths become finer, but they are not lost or *laid down,* nor is there any fusion of the nervous into the adjacent substance.[3]

But it is questioned whether this, which is the only really observed arrangement, be a just account of the distribution of the minutest elementary nervous structures. Another view of the whole matter must be taken if it be true that those which are called the primitive fibres be, as the observations of Treviranus,[4] Remak, and Martin Barry indicate, fasciculi, each containing several fibrils which, though they are not discernible in the substance of the tissues, may be distributed to their minutest parts. If it be so, the real ultimate arrangement of the nerves is utterly unknown. There are some imperfect observations respecting the distribution of the minuter fibres, by Schwann[5] and Remak.[6] But it must be admitted as a strong argument against the distribution of such finer branches, that the course of those already described as the ultimate fibres is clearly discernible, that they do not appear to give off branches, and that small as the finer divisions are supposed to be they would not be less than the fibres of many other kinds which can be recognized in their respective tissues.

[1] See Henle, Allg. Anat.

[2] Valentin, Ueber den Verlauf, &c., and in B. and F. M. R. Vol. XI.

[3] No mention is here made of the cells found near the peripheral distribution of the nerves of the higher senses, and supposed by Valentin and some others to be analogous to the ganglion-globules at the nervous centres; for they are not constant elements in the peripheral nervous structures, and Henle's opinion (Allg. Anat., p. 664) that they are epithelium-cells covering the layer of the extremities of the nerves seems more probable than any hitherto offered. Their history may be found in his work, and in the essays on the retina referred to in a following note.

[4] Vermischte Schriften, bd. i. p. 129; and Beiträge, hft. ii. p. 39.

[5] Müller's Physiologie, bd. ii.

[6] Zur Mikrosk. Anatomie der Nerven, &c. In reference to the distribution of the nerves in each organ it must suffice to refer generally to Valentin's edit. of the 5th vol. of Sömmering's Anatomie, and his treatise Ueber den Verlauf und die letzten enden der Nerven; and to Müller's and Carpenter's Physiologies. For particular organs the following references may be sufficient: for the *Retina:* Hannover, (Müller's Archiv, 1840, hft. iii.;) Grube, (ibid. 1840, and Microsc. Journ. 1841, p. 71;) Bidder, (Müll. Arch. 1841;) Remak and Henle, (ibid. 1840;) Gottsche, (ibid. 1834;) Henle, (Allg. Anat.;) and Valentin, (Repertorium, bd. v. &c.) The *Ear:* Wharton Jones, (*Hearing, Organ of,* Cycl. Anat.;) Pappenheim, (Die specielle Gewebelehre des Gehororgans;) Valentin, (Repertorium;) and B. and F. M. R. Vol. VIII. p. 68. The *Nose:* Treviranus, (Beiträge, hft. ii. p. 56.) The *Skin:* Valentin, (Ueber den Verlauf, &c.;) Burdach, (Beiträge, pp. 108, 161;) Breschet, (Recherches sur la Structure de la Peau.) The *Muscles:* Valentin and Burdach, *l. c.* The terminations in the ciliary ligament and iris, the tongue, the blood-vessels, and the teeth-pulps are also described by Valentin and Burdach. The latter shows that in general, though the rule is not universal, the sensitive nerves terminate in plexuses, or complex loops, the motor in simple loops.

2. *Central.* When the nervous fibres have passed centripetally through the dura mater, the general neurilemma becomes thinner, and there is a still further reduction both in it and in the investment of each fibre when they have penetrated the superficial layer of the brain or spinal cord. In the same progress the size of the fibres gradually diminishes, and the tendency to assume the varicose form on the least injury of the investing membrane increases. Having entered the brain, whether directly or through the medium of the spinal cord, the fibres proceed in bundles (which are the coarsely demonstrated *fibres of the brain*,) towards the cortical substance, forming on their way the most intricate plexuses, but never anastomosing. Arrived at the cortical substance, the fasciculi form plexuses among the gray globules exactly comparable with the terminal plexuses already described; and, at the last, the fasciculi, having broken up into their component fibres or very small collections of them, these form loops in the cortical substance of the brain like the terminal loops in the substance of the tissues.[1]

The white substance of the spinal cord, like that of the brain, is composed entirely of the continued nervous fibres. Bundles of them form intricate plexuses without anastomoses, and have all a general direction towards the brain. As far as the microscope can discern, the fibres, as they ascend get nearer to the gray matter, being successively overlaid by those which abut upon the cord higher up than themselves; and the cord, as it passes from below upwards, is thus continually augmented by external layers of fibres, till it comes to the medulla oblongata.[2]

It is not probable that either the brain or the spinal cord contains any fibres but such as are continuations from those of the several nerves of the body: of these they form what Valentin has called a *condensed collection.*

c. *Structure of the Gray Nervous Substance.* The general character of this constituent of the nervous centres is that it is composed of numerous globules, called ganglion-globules, from $\frac{1}{300}$ to $\frac{1}{1250}$ of an inch in diameter, which are usually of a spherical or oval form, more or less flattened, and of a reddish colour. Each contains one or more nuclei with subordinate nucleoli, is inclosed in a very fine filamentous investment, and is often marked with superficial spots of pigment.[3] These investments or sheaths of the ganglion-globules are, according to Valentin and others, formed of several strata, of which the exterior consists of a thin layer of fine granular corpuscles, the next of elongated cells with nuclei, and the most interior and thickest of concentric lamellæ of very delicate cylindrical filaments. By its sheath, each ganglion-globule is isolated from its neighbours; but by the interchange of filaments passing from one sheath to another, the whole of the globules are held together in one group, and lie, as it were, imbedded in the meshes of a network formed by their investments.

The ganglion-globules, which seem sometimes to lie free in their sheaths, so that they may be extracted from them, consist mainly of a red or reddish-gray

[1] Nearly all the minute anatomy of the fibres in the nervous centres is due to Treviranus and Valentin. The arrangement in plexuses has been generally confirmed. It was first minutely described by Dr. Macartney in 1833, in his "Observations on the Structure and Functions of the Nervous System," (see Abstract in Med. Gazette, vol. xiv. p. 842,) The junction of fibres in central loops, first described by Valentin, has been confirmed by Carus, (Einige Aphorismen aus der Phys. des Nervenlebens, Müll. Arch. 1839,) and by Klencke (Ueber die Primitivnervenfasern, Gött. 1841), but by no other anatomist. Valentin himself has lately spoken very doubtfully of it (Repertorium, 1840, p. 96).

[2] The microscope does not confirm the opinion of E. H. Weber, Bellingeri, and Grainger that some fibres pass straightway to the central substance of the cord; nor that, which Valentin's experiments seem to prove, namely, that some of the fibres of the anterior roots which are sent to extensor muscles go at once to the posterior columns, and some of those of the posterior roots to the anterior columns; but the positive results of experiments are in these questions much better evidence than the negative ones of microscopic examination.

[3] The greater part of this account is from Valentin's works.

granular material, held together by a clear soft gelatinous substance; and from this, the ganglia and the gray parts of the brain and spinal cord chiefly derive their characteristic colour. The true original form is probably spherical, but in different situations, either to adapt themselves to the surrounding parts, or according to some law of development, they are elongated to an oval or ovate form, or are heart-shaped, or kidney-shaped, or angular, or altogether irregular. In general, each globule has but one nucleus with one nucleolus in its wall; but numerous exceptions to this also are found which, as well as the varied forms of the globules, seem connected with their progress in development, and with their particular offices in each part.[1]

D. *General arrangement of the two substances in the nervous centres.* The plan of arrangement of the fibres in the brain and spinal cord is already described. The gray substance in the cortical and other parts of the brain, is composed almost entirely of ganglion-globules, which deviate from the general character only in being peculiarly soft, and invested with extremely delicate sheaths. They are collected in small groups in the interstices of the fine vascular network by which the gray matter is everywhere traversed; and the layers, ganglia, and all other forms of gray substance, are due to the different modes in which they are aggregated. On the surface of the cortical substance of the brain, however, another kind of structure is present, and it is found in smaller quantity about other parts of the gray matter: this is a finely granular substance, containing pellucid spherical or oval vesicles, with one or two dark granules in them. In a rather deeper layer, these vesicles, instead of being irregularly scattered through the granular substance, seem each to have appropriated to itself a portion of the latter for an independent covering; and from this condition there seems to be a regular gradation till, in the yet deeper layers of the cortical substance, the vesicles, with their granular coverings, are replaced by perfect ganglion-globules with their filamentous sheaths.[2]

In the purely gray substance of the axis of the spinal cord, the ganglion-globules are arranged in the same manner as in the brain. They are continued even below the giving off of the last spinal nerves in a fine cord-like process, which occupies the termination of the canal of the dura mater.[3]

The ganglia, from whatever part of the nervous system they are taken, present one general plan of structure: their chief mass is composed of a congeries of the ganglion-globules, with which nervous fibres are brought into relation and mingled in various ways. Each ganglionic mass is enveloped by a covering of cellular tissue, continuous with, and analogous to, the neurilemma of the nervous cords that enter into it. The strength of this covering is directly proportionate to that of the sheaths of the globules contained within it; and prolongations from the one are closely interwoven with the filaments of the others.

The nervous fibres that enter into the composition of the ganglia, are similar in structure to those of the sensitive and motor nerves, except that they are finer and their sheaths more delicate. A portion of the fibres which enter one side of a ganglion separate as soon as they have penetrated its substance, and, after forming a plexus in its interior, unite again into one or more cords, and emerge from the other side in the same manner as they entered. But another portion do not pass thus simply: separating from the plexus in individual fibres, or in fasciculi containing each a very few fibres, they wind about in the interior, and usually near the surface of the ganglion, in the most varied manners. In most cases the plexus of fasciculi occupies the central parts of the ganglion, and the fibres and bundles which compose it may be named the *traversing fibres;* the others are, as it were, *spun* round the central parts, and may be called *winding fibres.*[4]

[1] On all these see Valentin, Henle, and Klencke, in the works already quoted.

[2] See Henle, (Allg. Anat. p. 677,) and Valentin.

[3] Remak, Burdach, Arnold, &c.

[4] Valentin, (Ueber den Verlauf, &c.;) Remak, (Froriep's N. Notizen, Aug. 1837.) Nearly all this account of the gray substance is taken from the writings of the former.

The interspaces between the fibres of both kinds are occupied by the ganglion-globules which lie variously entwined by small vessels and nervous fibres. The exact manner of their distribution varies in different ganglia: in some the globules are chiefly arranged around the traversing fibres in the centre of the mass; in others, they are evenly dispersed throughout, or are absent only at the very exterior; in others, they are almost all placed at one side. But, however the component parts are arranged, the general plan of construction is in all ganglia the same.

Valentin believes that the winding fibres are those which are about to be distributed in the organs to which they are destined, and that the traversing plexus-forming fibres are those which are as yet far from their destination and which have to pass through other ganglia, in which they also will at last be arranged as winding fibres. There is no appearance of fibres generated within the ganglia; none leave them but such as are continued from those which entered them.[1] Whether they be connected with any fibres of a peculiar structure is already considered: it is only necessary to add, that some of the nerves proceeding from ganglia, and having a gray or reddish-gray colour very plainly marked, contain ganglion-globules mixed with their fibres. This is the case in the connecting cords of the human cervical ganglia of the sympathetic, into all of which ganglion-globules extend from the ganglia themselves,[2] such nerves might indeed be truly called ganglia, for they scarcely differ, except in form, from the bodies usually so named.

XV. BLOOD-VESSELS. A. *Arteries.* After a variety of conflicting and unsatisfactory accounts, Henle[3] seems at length to have discerned such structures in the arteries as are adapted to the functions which experiment shows to be performed by them.

His account of their general structure is briefly this: 1st. They have an epithelial lining,[4] consisting of a very thin layer of elliptic or rhombic lamellar cells, which are sometimes elongated into longitudinal spindle-shaped fibres. 2d. There is, immediately external to this, a layer of peculiar tissue, the *striated* or *fenestrated coat*, (corresponding to the *internal coat* of the older anatomists,) consisting of a very thin, rather stiff, and brittle membrane, bearing pale, flat, very narrow fibres, which have, for the most part, a longitudinal direction, and give it a peculiar delicately-striated appearance This coat, which is often morbidly thickened, and, when an artery is contracted, is commonly thrown into longitudinal folds, is produced by a metamorphosis of the epithelium, which, as the nuclei of its cells disappear, becomes a homogeneous membrane, on which the fibres are afterwards deposited, and which, at last, is completely removed, leaving the fibres free. 3d. In some arteries there is, next, a coat formed by a single layer of longitudinal granular fibres, flat, and tolerably wide, analogous to a coat which is much more prominent in the veins. 4th. A coat composed of *circular fibres* (the *middle* or *elastic* coat of most former writers, the *muscular* coat of Hunter), which forms the chief part of the arterial wall, and comprises all that can be torn from it in a transverse direction. Its fibres are flat, clear, and granular, and break with abrupt ends. Each of them is commonly marked along its middle by dots scattered, or regularly arranged in a longitudinal row, or by a narrow streak: these are the remains of elongated nuclei, which have formed as it were, the pattern, according to which the homogeneous membrane in which they lay has broken up into the fibres.

[1] De functionibus nervorum, p. 66. But see the Appendix, B.

[2] Valentin, (Ueber den Verlauf. tab. vi.) See also, on a similar case in the glosso-pharyngeal nerve, Volkmann, (Ueber die motorische Wirkungen, &c. Müll. Archiv, 1840, p. 488.)

[3] Ueber die Contractilitat der Gefässe, (Casper's Wochenschrift, Mai 28, 1840,) and more fully in his Allg. Anat.

[4] First described by him in his essay, Ueber die Ausbreitung des Epitheliums, (Müller's Archiv, 1838.)

The streaks formed of the elongated nuclei often branch and anastomose, so as to form that kind of network which has led to this coat being mistaken for elastic tissue; whereas it is, in fact, the proper contractile coat of the artery, and is, in all respects of development, and microscopic structure, similar to the layers of organic muscle in the stomach, &c. 5th. On its exterior there is a coat of genuine elastic tissue (*tissu jaune*, the *elastic coat* of Hunter); this exists, however, only in the larger arteries, and its thickness, in comparison with that of the preceding, diminishes in direct proportion to the size of the artery. The direction of its fibres varies greatly in different arteries.[1] 6th. The *external cellular coat* consisting of common cellular tissue, with longitudinal closely-woven filaments.

The conclusions from these facts which, as already said, are the first of the kind that have accorded with the results of experiment and observation of the functions of the arteries, may be expressed in Hunter's words: "From the account we have given of the substances which compose an artery, we may perceive it has two powers, the one elastic and the other muscular. We see also that the larger arteries are principally endowed with the elastic power, and the smaller with the muscular; that the elastic is always gradually diminishing in the smaller, and the muscular increasing, till, at last, probably, the action of an artery is almost wholly muscular; yet I think it is not to be supposed but that some degree of elasticity is continued to the extremity of an artery."

"The muscular power of an artery acts chiefly in a transverse direction;.... the elastic power exists almost entirely in the external coat; the internal coat must be the seat of the muscular power..... Arteries are the conductors and disposers of the blood.....The elastic (power of reaction) is best fitted for sustaining a force applied to it, (such as the motion of the blood given by the heart), and propelling it along the vessel; the muscular power, most probably, is required to assist in continuing that motion, the force of the heart being partly spent, but certainly was intended to dispose of the blood when arrived at its place of destination."[2]

B. *Veins.* The six coats already mentioned include all that are found in the blood-vessels; and the distinctions of the vessels of the several orders depend on the proportional quantities of these coats present in each. The veins, according to Henle, have, 1st, a lining of epithelium, like that of the arteries; 2d, a striated or fenestrated coat, similar to the second in the arteries; 3d, a longitudinally fibrous coat, analogous to that in the arteries, but, in the large veins, formed of several strata, and often morbidly thickened; 4th, a layer, occupying the place of the contractile circular-fibred coat of the arteries, but much thinner than it, and chiefly or entirely composed of fasciculi of cellular tissue, which like that of the skin and dartos may be regarded as contractile; and 5th, the external cellular coat, with longitudinal fasciculi. The true elastic coat is absent.

The valves exist in veins of less than a line in diameter, wherever their office is to be fulfilled.[3] They are covered by the epithelium, and consist of tissue like that of fibrous membranes, which, as Hunter observed, proves that they are not duplicatures of the lining membrane. In the larger valves this tissue is mixed with some like that of the striated membrane of the vein.

Very few conclusions can yet be drawn from these facts, respecting the active functions of the veins. Such as they are, however, they might also be quoted from Hunter, who says, that the veins have nearly the same elasticity with the arteries; that their muscular (contractile) power is very considerable; that the former in some degree preserves them in a middle state; and that the latter adapts them to the various circumstances which require the area to be within that state.

[1] See especially Räuschel, (Diss. inaug. de arteriarum et venarum structurâ. Vratisl. 1838;) and Schwann, (Encyclop. Worterb. der med. Wissench. art. Gefässe.)

[2] Treatise on the Blood, &c. [3] Henle, *l. c.*

c. *Capillaries.* However little the miscroscope may have contributed to the knowledge of the foregoing part of the circulatory system, it has taught all that is known of this, the more important portion of it. It may indeed be regarded as one of its chief honours that it was the means of obtaining the knowledge of the last fact essential to the full proof of the circulation of the blood. Harvey could only prove that the arteries carry blood from the heart, and that the veins bring it back; of the passage from one set of vessels to the other at their distal extremities he knew nothing, and only in the later part of his researches, decided that it was not by the wide channels, which the older writers called *anastomoses*, but probably through a *parenchyma*, in which the blood was infiltrated.[1] The real mode of transit was first proved by Malpighi,[2] in 1661, by a microscopic examination of the circulation in the distended urinary bladder of a frog[3]. His facts were soon confirmed by many others, and especially by Leeuwenhoeck.

Form and arrangement. It was not, however, till long after this time that the general existence of capillaries was admitted; and when it was granted, volumes of hypotheses were written about their arrangement, and their various relations to the parts around them.[4] Of late years the microscope has established the truth in far greater simplicity than the imagination had pictured it; proving that, to whatever part the blood is sent, it either passes directly from arteries to veins (both of very small size) or flows from one to the other through a network of minute canals;[5] that it never, at least in the healthy state, passes from the blood-vessels into any other canals or cavities, or into the tissues around them; and that the only mode of communication between the cavity of the vessels, and any other part of the body, is through the invisibly-minute pores, which exist as well in the walls of the capillaries and small vessels as in all organized tissues.[6]

But, though these facts have cleared the way for truth, they have not afforded a deeper insight into the real nature of those processes in which the contents of the capillaries come into immediate relation with the surrounding parts. Marvellous as are the structures revealed by the beautiful art of injection, one cannot yet trace the particular purposes that are served by any of the numerous varieties of vascular arrangement; from Swammerdam,[7] who first employed it as a means of preparation in 1667, to the present day, it has shown increasing wonders of form, but has scarcely afforded a glimpse of the intimate nature of any process.[8]

It will therefore be unnecessary to enter into all the details of the arrangements of the capillaries and small vessels in the several organs and tissues. The

[1] Compare his Exerc. de Motu Cordis, p. 56, and p. 60, (edit. 1766,) with passages in the Epist. Prim. ad J. Riolanum, (p. 105, ej. ed.,) and in the Epist. Secund.

[2] Epist. Secunda ad J. Borellum, (De Pulmonibus, p. 143,) where he describes both capillaries and the circulation in them. The proof by artificial injection seems to have been first obtained by Harvey's friend, George Ent, (Apol. pro circul. sang.) but he could not trace the connexion whose existence he had proved.

[3] The frequent mention of these creatures in physiology will prove how well, in recompense for their sufferings, they deserved the honour of a late essay by M. Dumeril, "Sur les découvertes faites dans les sciences par l'étude de l'organization des grenouilles, (Bull. de l'Acad. de Méd. 1840.)

[4] See especially Haller, (Elementa Physiologiæ, tom. ii.)

[5] Berres calls them, not capillaries, but *intermediate vessels.*

[6] The acute Prochaska was among the first to establish and appreciate the great value of these facts; see his Disq. Anat.-Phys. Organismi Corp. Hum., p. 94, &c.

[7] See his life by Boerhaave, and a communication to the Royal Society in 1672.

[8] The most beautiful delineations of the minute vessels are those by Mascagni, (Prodromo della grande Anatomia;) Berres, (Anat. Microsc. Corp. Hum.;) and Arnold in his recent great work on Anatomy. The differences of arrangement are much less in the capillaries themselves than in the small vessels preceding and following them; it is to these alone that the descriptions which authors give of arborescent, plumose, tufted, and other forms refer.

general facts are these—that the capillaries compose networks permeating the interspaces of the proper elements of each organ and tissue; that the diameters of their canals, (which are all of nearly equal size in the same part,) vary from $\frac{1}{1000}$ to $\frac{1}{3000}$ of an inch, the most common size being about $\frac{1}{3000}$[1]; that the meshes generally bear a close relation in form to the predominant disposition of the proper elements of the tissue, and are in some parts (as the lungs, the choroid, and some mucous membranes,) even narrower than the vessels around them, but more commonly are three or four times wider; and that, as a general rule, the more active the functions of a part, (especially if it be an organ of secretion,) the closer is its network of capillaries.

2. *Structure.* The capillaries have distinct walls, and are not mere channels drilled in the tissues around them. In some parts they seem to constitute the main tissue, as in the pia mater, which is an irregular vascular network with a few cells scattered in its meshes, and the smallest possible quantity of cellular tissue, the vessels in the pulpy membrane of the cochlea of birds, and, as Mr. Bowman has lately discovered,[2] the corpora Malpighiana; in others they are separable from the soft surrounding tissues, as in the choroid, iris, and retina; and in all parts of which the tissues around them are well distinguished by colour and compactness, the walls of the capillaries are plainly discernible. The only question now is concerning the tissues which compose them.

According to Henle,[3] the finest vessels are composed of a completely structureless membrane, in which no fibres or striæ are ever discernible, but which bears minute oval corpuscles, the persistent nuclei of the cells from which the capillaries are formed; they are placed longitudinally upon the vessels, and are arranged in one, or two, or alternate, rows. This may be named the *primary vascular membrane*, because it appears to be the direct product of the primary cell, from which the capillary vessel is formed, and because, in various development, it exists in the vessels of every kind. In vessels of a size just larger than the capillaries, the nuclei of the primary membrane are considerably elongated; and there are added an inner layer of epithelium-cells and an outer layer of pellucid membrane, bearing elongated, transverse cell-nuclei. The latter represents an early stage of the circularly-fibrous coat of the larger arteries. It is from these elongated, longitudinal, and transverse nuclei, that vessels of this size acquire the appearance from which Schwann,[4] and Valentin[5] deduced that they had transverse fibres, and the latter that they possessed both elastic and cellular tissue. Dr. Martin Barry, probably in the same structures, discerns compound double-spiral filaments wound spirally around the vessels.[6]

3. *Functions.* The knowledge of the mode of circulation in the capillaries is entirely due to the microscope, but it must be admitted, that except in Dr. Barry's account of their structure, there is no anatomical confirmation of that which other modes of observation have shown, namely, that they and the small arteries and veins are not merely passive tubes, but may exercise a power of regulating the flow of blood through them.

Under ordinary circumstances the blood moves through the systemic capillaries in an even stream, at an average rate of an inch in a minute and a half, and through the pulmonic system, at the rate of about five inches in the same time.[7] But many circumstances influence the diameter of the capillaries, and

[1] Krause, (Vermischte Beobacht. Mull. Arch. 1837, and B. and F. M. R. Vol. VI.) says there are some much smaller, not more than from $\frac{1}{10000}$ to $\frac{1}{15000}$, and others varying from $\frac{1}{6000}$ to $\frac{1}{9000}$ in the retina, villi, and organic muscle; but his observations have not been generally confirmed. On the whole, however, there is sufficient evidence that in some parts, such as the brain, there are a few vessels not large enough to transmit the blood-globules, (see Henle, *l. c.* 471; and Wagner, Lehrbuch, p. 186.)

[2] On the Malpighian Bodies, &c., Philos. Mag. June, 1842.

[3] Allg. Anat. 491.

[4] Encyclop. Worterb. art. Gefässe.

[5] Repertorium, bd. ii.

[6] Proceedings of the Royal Society, Jan. 6, 1842.

[7] These calculations were made by Hales, (Staticks, vol. ii.) Those by E. H. Weber and his brother, (Müller's Archiv, 1838, p. 450,) make the velocity equal to about $1\frac{1}{4}$ inch per minute. In either case the result seems inconsistent with the rate at which poisons

the motion of the blood in them. Besides the pathological changes which they undergo, the small arteries and capillaries are seen to be contracted by cold,[1] and by warmth to be slightly enlarged; under the influence of certain irritants also, such as capsicum, or an essential oil, they contract, and again, immediately after, dilate,[2] which fully confirms what general observations had made probable: namely, that during life a power is exerted, by which the small vessels, changing their diameter, can control the passage of their contents. And, in like manner, some confirmation has been afforded to the evidence from experiment, that this power is exerted under the influence of the nerves; for an anatomical connexion between the latter and the small vessels has been proved by Purkinje, Valentin,[3] Remak,[4] Henle,[5] and others, who have seen the finest nervous filaments on the wall of the cerebral and other blood-vessels, of less than $\frac{1}{50}$ of an inch in diameter.

M. Poiseuille has greatly added to the knowledge of the current in the capillaries, by watching the *motionless layer* in it. The existence of this layer was observed by Haller, Spallanzani, and others, but its importance was not appreciated by them. From M. Poiseuille's[6] observations, confirmed and extended by E. H. Weber,[7] Gluge,[8] Wagner,[9] and Ascherson,[10] (who has seen the same appearance in mammalia,) it appears that the stream of blood flows most rapidly in the axis of the capillary vessel, and that its velocity gradually diminishes towards the circumference, till, in immediate contact with the walls, there is a layer which is perfectly still. The breadth of this layer, which is the simple result of the adhesion of the blood to the walls of the vessels, is usually from $\frac{1}{8}$ to $\frac{1}{10}$ of that of the whole stream, and is the greater the slower the general current is. Its existence and the different relations of the other parts of the stream are discerned by the observation of the blood-corpuscles. The perfect ones commonly occupy the middle of the stream, surrounded by the lymph-corpuscles, which move ten or more times slower than those in the axis stream. These corpuscles again are surrounded by the motionless layer, into which if any globules are forced, they move very slowly, and, if they come near to the wall, remain for a time quite stationary.[11]

and other substances are proved, by the experiments of Hering and Mr. J. Blake, to be carried with the blood; but the length of capillary tube through which each globule has to pass is extremely small, and in the larger tubes the current is much more rapid. Perhaps also the globules move more slowly than the plasma; if so the conclusions from the microscope must be deceptive.

[1] Schwann and others, confirming Hastings.

[2] See especially Dr. C. J. B. Williams, (Gulstonian Lectures, Med. Gaz. 1841.) Most of the previous observations by Kaltenbrunner, Wedemeyer, Hastings, &c. are nearly valueless, since it is probable that the substances they employed might chemically alter the physical condition of the blood-vessels and the adjacent tissues.

[3] Ueber den Verlauf der Nerven, p. 12.

[4] Obs. anat. et micros. de system. nervosi structurâ. [5] Allg. Anat. p. 511.

[6] Rech. sur les causes du mouvement du sang dans les vaisseaux capillaires, (Ann. des Sc. Nat. 1836.)

[7] Müller's Archiv, 1837; and B. and F. M. R. Vol. IV.

[8] Sur la couche inerte des vaisseaux capillaires, (Ann. des Sc. Nat. Jan. 1839.)

[9] Nachtrage zur vergl. Phys. des Blutes.

[10] Müller's Archiv, 1837, and B. and F. M. R. Vol. VI. p. 219. Among many deductions which may be drawn from this fact of the blood being at rest near the walls of the vessels, is that the capillaries do not exercise a constant force in propelling it; if they did the part next the wall should move most rapidly.

[11] It is probable that the local influence of cold in making the still layer wider, and retarding the stream of blood, is due as much to some physical influence as to the contraction of the vessels, for M. P. could never discern the latter: he therefore refers the retardation to the well-known rule that the quantity of the same fluid transmitted in a given time by a capillary tube is, within certain limits, directly proportioned to its temperature. He has proved also that the influence of cold is not merely local; but that when it is applied to any part, the capillary circulation is slightly retarded in all; perhaps through the reflex influence of the cold upon the heart.

The fact that the purpose to which the capillaries are habitually subservient is only the passive one of conveying blood close to those parts of the body which either grow or secrete, renders the vascularity or non-vascularity of a tissue a matter of less interest than it used to be; for it is proved that if a part be only able to imbibe the fluid portion of the blood from an adjacent vessel, it nourishes itself as completely, and after the same method, as one whose substance is traversed by numerous capillaries. The extra-vascular tissues, as they are usually called, that is, those in whose substance neither injection nor the microscope has yet revealed any blood-vessels, and which derive their nutritive materials from the blood flowing in adjacent tissues, are the crystalline lens, epidermis, epithelium, and all forms of cuticle, hair, nails, enamel and dentine of teeth, and the analogous structures of feathers, hoofs, &c. To the list of vascular tissues the microscope and the improved art of injecting have added the cornea,[1] the anterior part of the capsule of the lens,[2] the membrane of the aqueous humour,[3] the hyaloid membrane,[4] the articular[5] and other cartilages,[6] the tendons,[7] the elastic tissue,[8] and even the densest bones.

XVI. Lymphatics and Lacteals. 1. *General Structure and Arrangement of the Vessels.* The researches of Panizza,[9] confirming those of Cruickshank, Mascagni, Fohmann, and others, have established that in the tissues generally the lymphatic vessels arise from closely-meshed networks which are interspersed among the proper elements of each part, and which, like those of the capillary blood-vessels, vary in the size both of the canals and of the spaces which they inclose. The difficulty, however, of learning the exact size of these canals is even greater than with the blood-vessels, because of the remarkable yielding of their walls.

There are still several organs and tissues in which no lymphatics have been discovered: such are the brain and spinal cord,[10] the bones,[11] the cartilages, the dense tendons, the eye,[12] the placenta, the umbilical cord,[13] and the membranes of the ovum, and all those into which blood-vessels have not been traced.

The structure of the larger lacteals and lymphatics is very similar to that of the veins. They are lined by an epithelium like that in the blood-vessels; next externally to this is a layer of nearly longitudinal fibres of a character intermediate between those of cellular tissue and the granular fibres of the arterial contractile coat; and around this is a layer of fibres of cellular tissue, which have a circular arrangement, and are connected with those of the next adjacent tissue.[14] The minutest lymphatics seem to be destitute of valves; but they are

[1] Romer, in B. and F. M. R. Vol. II. 235, &c.; Berres, *l. c.*, t. xii. f. 5.

[2] Schroeder van der Kolk, Over choroiditis als oorzaak van Glaucoma, in the Verhand. van het Genootschap . . . te Amsterdam, 1841.

[3] Schroeder van der Kolk, *l. c.*

[4] Berres, (*l. c.* Pl. xiv.;) Dalrymple, (in Tyrrell on Diseases of the Eye.) The most perfect injection is described by S. van der Kolk, *l. c.* See, however, for evidence against these injections, Mr. Toynbee's paper in the Philos. Trans. 1841, p. 159, "Researches tending," &c.

[5] Liston, Medico-Chirurgical Trans. v. 23.

[6] Fremery, Müller's Physiologie, i. 283, &c.

[7] J. P. Medical Gazette, 1839.

[8] Berres, Dœllinger, &c. But this is rather doubtful; the vessels were perhaps in the proper contractile coat of the arteries.

[9] Osservazioni antropo-zootomico-fisiologiche.

[10] Arnold, (Icones Anatom. pars i.) gives admirable figures of the lymphatics of the coverings of these organs; the results of his attempts to inject others in their substance were doubtful.

[11] Cruickshank and Brugmanns believed they had injected lymphatics in bone; but their success is doubtful.

[12] Mascagni (Prodromo) describes some in the eye; but he was too apt to regard every thing as a lymphatic.

[13] See Müller's Physiologie, i. p. 250, on Fohmann's supposed injections.

[14] Henle, Allgem. Anat. p. 551.

discernible in those of less than one third of a line in diameter, and have the same structure as those of the veins.[1] The minutest lacteals in the villi consist of a single membrane with elongated cell-nuclei, corresponding to the longitudinal fibrous membrane of the veins, but not lined by epithelium.[2]

2. *Absorbent Glands.* In the lymphatic and lacteal glands, the walls of the large vessels of which they are mainly composed are traversed by a dense network of capillary blood-vessels; a circumstance which affords some confirmation to the belief that something passes by a kind of secretion from the blood to the lymph and chyle in them, by which the latter becomes more charged with fibrine, and by which the development of the corpuscles is forwarded.

The question whether, in man, the lymphatics and small veins anastomose either in the glands or near their origins, still exists. They certainly anastomose in the latter situation in amphibia and fish,[3] but the microscope has added no evidence to that of Fohmann and the others[4] who hold that they similarly communicate in mammals and birds.

3. *Villi.* The bodies to which this name should be exclusively applied are seated in the small intestines only, and are peculiarly the organs for the absorption of the chyle. They are delicate vascular processes of the mucous membrane, from a quarter of a line to a line and a half in length,[5] of which about twenty-five are set on every square line of surface.[6] They vary in form according as the vessels they contain are empty or full of chyle: in the former case they are flat, and pointed at their summits; in the latter they are cylindrical or clavate.[7] Into the base of each there enters a single lacteal vessel, which, after passing along the middle, ends either in a blind, slightly swollen extremity, or, as Krause[8] and Valentin[9] think, in a network. In some villi, also, there are two such vessels, which pass along opposite borders and terminate without anastomosing.[10] The walls of each are traversed by a very delicate network of blood-vessels formed of from three to five minute arteries, which, after variously dividing and anastomosing, are continued into one or two veins which descend along the villus to the vessels of the submucous tissue.[11] Each villus is farther invested by a very delicate sheath of epithelium, which is frequently, perhaps after each completed digestion, shed.

These facts regarding the structure of the parts of the absorbent system do not at all illustrate their mode of operation. The lacteals have not open orifices in the villi; they probably derive their appearance of terminal apertures from the cells which compose the epithelium over them; they probably, therefore, act by imbibition through their porous walls. But it is to be observed that they do not lie next to the fluid which they absorb; they are covered by a layer of very vascular mucous membrane and by a sheath of epithelium. Valentin[12] has hence been led to suggest that since the blood-vessels in a villus hold nearly the same relation to the lacteal as in a secernent gland they do to the extremity of the duct, and since in absorption the material from the intestines must pass by the blood-vessels to enter the lacteal, it is therefore probable that the chyle is not immediately transferred from the intestine to the lacteal, but is, as it were, secreted into it through the medium of the blood-vessels. Whether this be true or not, the selection of peculiar substances to pass into the lacteals proves that they do not act by so simple a force of imbibition as is exercised by the other porous tissues.

[1] Valentin, Repertorium, 1837. [2] Henle, *l. c.*

[3] See especially Hyrtl. Med. Jahrbucher des Oesterreich. Staates, bd. xxxi.

[4] See Müller, *l. c.* p. 256. [5] Ibid. *l. c.*

[6] Lieberkuhn, Diss. de Fabricâ et Actione Villorum, 1782.

[7] They present many other forms in animals; but these seem to be the only ones that occur in man.

[8] Vermischte Beobachtungen, (Müller's Archiv, 1837.)

[9] Müller's Archiv, 1839. [10] Henle, Symbolæ ad anat. villorum.

[11] Lieberkuhn, (*l. c.*;) Doellinger, (De vasis sanguineis quæ villis ineunt.)

[12] De functione nervorum, &c. p. 112.

Probably the lymphatics of the peripheral network act similarly. Dr. Carpenter[1] has made it highly probable that these do not imbibe all fluids indifferently, but that their office is to absorb the nutritious products of the *secondary digestion;* that sort of digestion which, as Dr. Prout says, is carried on in all parts of the body, and by means of which substances are appropriated for nutrition both from the dead and decomposed elements of all the tissues, and from materials deposited in store for reabsorption, as the fat of hybernating animals.

Experiments have rendered it, on the whole, probable that the coats of the larger lymphatics have vital contractility; and their microscopic structure is favorable to this view. Without such a power the motion of fluid in them is inexplicable; with it, no other is necessary; for the valves, extending as they do into the minutest branches, must render the whole force of the contraction of each segment efficient to the propulsion of fluid into the segment above it.

XVII. Secernent Glands. Many of the general rules of glandular structure laid down by Malpighi[2] and Müller[3] are deduced from microscopic observation, or, at least, they are settled by it, for the objects illustrative of them do not all lie beyond the field of ordinary vision. Of these general rules, the chief are: 1st, That a general unity of plan prevails in the seemingly manifold varieties of glandular structure in the different organs and classes of animals: 2d, That all secretory glands are composed of tubes opening on a free surface, and either simple, or variously ramified so as to present in a small solid space a very great extent of surface for secretion: 3d, That, while the excretory end of the gland-duct opens on a free surface, its opposite or secretory end is always closed: 4th, That aggregations of these blind ends of a ramified gland-duct form the *acini* which were long supposed to be the proper agents of secretion: 5th, That there is no open communication between gland-ducts and other vessels, and that the blood-vessels do not open into the ducts or acini, but ramify in a capillary network in their walls and interspaces, and there supply the materials of the secretion.

Recent observations by Henle, Krause, and others, render it very probable that some of these general rules, though true as far as they go, require to be modified or added to. There are organs which may be strictly called glands, yet have not tubes opening constantly on a free surface, but open thus only at particular times, by a kind of dehiscence. Such are the Peyer's and solitary glands of the small intestines, first well described by Böhm,[4] and since by Krause[5] and Henle;[6] and these may be taken as a type of a numerous class of similar bodies which occur constantly, or at particular times, in the substance of all mucous membranes.[7] Böhm described the Peyer's and solitary bodies as simple sacculi beneath the mucous membrane, without external orifices, containing a fluid rendered opaque by a number of minute white granules and cells, and surrounded by what he called a *corona* of tubules, which had no communication with their cavities. Krause, however, believed he succeeded in injecting some of the sacculi through these tubules; and thus supplied the first step to that which Henle has generalized with other facts of the like kind, in the very probable theory, that all the sacculi of this kind are secretory organs,

[1] Principles of Human Physiology, p. 377.

[2] Epistola de glandularum consimiliumque partium structurâ.

[3] De glandularum structurâ penitiori.

[4] De glandularum intestinalium structurâ penitiori, 1835; see also B. and F. M. R., Vol. I. p. 521.

[5] Vermischte Beobachtungen, (Müller's Archiv, 1837.)

[6] Müller's Archiv, 1833, hft. iv. note; and Allg. Anat.

[7] Lelut, who is quoted by Henle with just praise for his excellent investigations of epithelia, describes numerous glands without ducts in the pharynx and œsophagus, (Des glandes muqueuses, &c. in Journ. Hebdomadaire, 1833, t. 18.) To this class also must be referred the so-called mucous or lenticular glands occasionally met with in the stomach, urinary bladder, &c.

which are closed till the secretion within them is matured, and then, by an absorption or bursting of their finely-membranous walls, open a communication with the surface of the membrane over them, and thus discharge their contents.

If it be admitted that any organ should be regarded as a gland which abstracts materials from the blood, and instead of appropriating them to its own nutrition, discharges them externally, or into some cavity, then, many organs not hitherto regarded as secernent glands may take their place in this class; such as the ovaries, and the so-called vascular glands, which probably elaborate some fluid and add it to the blood in their vessels, or peculiarly alter the blood as it circulates through them. The ovaries, indeed, may be taken as a type of the glands which have not permanently open ducts; for they contain numerous cells, the Graafian vesicles, imbedded in their stroma, which, at the time of conception, bursting through their enveloping membranes, escape into the oviduct, and leave behind them empty sacs. Just so it is also with the Peyer's glands, and all the others of that class; in the place of which we often find fossæ, the remains of the sacculi or vesicles that have recently burst through the mucous membrane, with whose surface their interior thus becomes continuous.

The proper *morphological element* of a gland, then, seems to be a sacculus or vesicle, elaborating within itself or attracting into its cavity the material of secretion, and discharging it through either an occasional or a permanent orifice or duct. These *primary vesicles* (as Henle[1] names them) are probably primary cells more or less metamorphosed. The walls of the smallest among them are formed of a translucent, structureless membrane; those of the largest consist of several layers of elongated nuclei and filaments of cellular tissue, arranged in concentric circles around them, and are sometimes lined by an epithelium. These additions, it is presumed, are effected by the development of nuclei and cells from the primary cell-wall; as in the blood-vessels the epithelium and the striated and fibrous coats are produced from the primary vascular membrane. The structure of the larger permanent gland-ducts also, when any can be separated from the vesicles, is very analogous to that of the blood-vessels, and is perhaps the result of similar phases of development. They consist of an internal layer of epithelium-cells (generally like those of the adjacent membrane), surrounded, first, by a layer of longitudinal fibres similar to those of organic muscle, then by a much thinner, and not always discernible, layer of circular fibres of the same kind, and lastly by a layer of cellular tissue.

According to the mode in which the primary gland-vesicles arrange themselves, three different forms of glands with permanent orifices may, according to Henle,[2] be produced. 1st, The *closed tubular glands*, which are formed of a single elongated vesicle, or in which a number of vesicles may be supposed to have arranged themselves in one line, and then to have all opened into one another by their apposed portions, except the lowest, which has remained closed at one end, and the one nearest the surface which has opened externally. Such are the Lieberkuhnian and tubular glands of the intestines, and the simple tubular glands of the stomach, mere tubes, like single gland-vesicles elongated to different depths, with walls, composed of a tube of structureless membrane, opening permanently on the mucous surface, and closed at their opposite ends. Such also, though rendered more complex by the attachment of vesicles along their sides, are the Meibomian glands; and such, elongated and grown tortuous, are the perspiratory and ceruminous glands. 2d, The *aggregated* glands, in which a number of vesicles arranged in groups have become so connected by a kind of fusion of their adjacent walls, that only a small portion of the membrane of each remains, and they form one cavity, with numerous recesses from its inner circumference. Such are all those commonly called mucous glands (as those of the lips, trachea, vagina, &c.); and the tonsils, lachrymal, Brunnian, salivary, mammary, and Cowper's glands, the pancreas and prostate; which differ only in secondary points of structure, such as the arrangement of their excretory

[1] Allgemeine Anatomie, p. 821, e. s. [2] *L. c.*, p. 906.

ducts, and the mode in which the *primary lobules* or simplest groups of gland-vesicles are connected together by fibro-cellular tissue, and supplied by blood-vessels. The smallest branches of the gland-ducts sometimes run into the central cavity of the group of vesicles, which thus all open into it: sometimes the groups, or primary lobules, are set upon the extremities or by the sides of the ducts: but whatever secondary arrangement there may be, all have the same essential character of rounded groups of gland-vesicles opening by a common central cavity into minute ducts. 3d, The *reticulated* glands, such as the kidneys and testicles, which consist of tubes of a transparent and structureless homogeneous membrane, the *membrana propria*, probably formed by the elongation and anastomosis of cells, like the blood-vessels, or the Haversian canals in bone, and which, like them, are connected in a network, and seldom or never end in a cul-de-sac.

Whether the gland be a vesicle which only once or occasionally opens, or one which, perhaps after sundry metamorphoses, has a permanent communication by ducts with the external surface, the mode of secretion seems to be the same. It is, however, still a question (and one which the microscope will not decide,) whether the fluid separated by the vescicles be always already formed in the blood, or whether it be not in some cases elaborated and transformed in them. Probably there is not one rule for all glands; for in certain secretions the microscope detects constituents which could not have been separated as such from the blood, namely, globules and corpuscles of different kinds, which indicate that the fluid separated had the character rather of a cytoblastema than of a dead matter.

In those secreted fluids which serve especially or solely for the purification of the blood, and are therefore more peculiarly excretions, such as the bile and the urine, there appear to be, during health, no corpuscles. But in those which after their discharge have to serve some special office in the economy it is very usual to find cells either perfect or in various stages of development. Henle has treated at some length of these endogenous cells. At present it may suffice to say of them, that they sometimes constitute the essential part of the secretion, as in the testicles, where they either become or generate within themselves the seminal filaments or animalcules;[2] the ovaries, where they become the germinal vesicles; the mammary glands, where they are the peculiar milk-corpuscles; the gastric glands, where they contain digestive fluid. In other cases they form an epithelium to line the gland-vesicles and tubules: in others, without any evident purpose, they are discharged from the gland-ducts or fill their extremities in the form of mucus-corpuscles; in others they appear as globules or cells of fat.

From the preceding account it is plainly not possible to draw a strict line between secretion and nutrition. In both alike the fluid abstracted from the blood may work in itself changes characteristic of life. Neither can expulsion from the seat of their production be regarded as characteristic of secreted substances; for the elements of many epithelia remain attached only a little longer than the corpuscles of some secretions, and the attachment of some of them is perhaps even shorter, as, for instance, of the epithelium-cells of the stomach in comparison with the animalcule-generating cells of the semen.

The ducts of glands which have permanent external orifices and through which the secretion formed in the gland-vesicles is discharged, have a minute structure very similar to that of blood-vessels. They are lined by a delicate layer of epithelium-cells, continuous with those of the surface on which they open, and continued to their finest branches. The forms of its elements in

[1] The *basement membrane* of Mr. Bowman, (Cycl. of Anat. *art.* Mucous Membrane,) who suspects that a similar membrane also lies beneath all epithelia.

[2] See some observations illustrative of this by M. Lallemand, in the Comptes Rendus 1841; and Edinb. Med. and Surg. Journ., 1841. See also Martin Barry, *l. c.* and Kolliker Ueb. das Wesen der Saamenthiere. Fror. N. Notiz. Juli, 1841.

different glands have been already mentioned. (p. 245.) Outside it there is in most instances a layer of fine, longitudinal fibres exactly resembling those of the contractile coats of the arteries and veins, and of the other lower muscles of organic life; and surrounding this a second thinner layer of similar fibres arranged in circles.[1] Lastly, external to these there is a layer of fine fibro-cellular tissue, but, considering their volume, there is scarcely any class of organs which contain so little of this tissue as the secernent glands.

XVII. VASCULAR GLANDS. Under this name may be classed the thymus and thyroid glands, the renal capsules and the spleen, which, as Henle[2] well says, "agree chiefly in this, that both their minute structure and their physiological import are at present totally unknown." It may suffice, therefore, to refer to the chief works in which there are original descriptions of their minute structure.[3]

XVIII. ERECTILE TISSUE. This might perhaps justly have had its place with the description of the organs of circulation since the only function which it discharges seems to be chiefly due to the peculiar arrangement of its blood-vessels. The parts in which it is found, however, are sufficiently similar in their characters to form a separate class: they are the penis, the clitoris, and, perhaps, the nipples. The first has been so much more examined than the others that the description of its structure is taken as the type of theirs.[4]

Independently of its blood-vessels, the erectile tissue of the penis is composed of a network of cords and bands, which form a multitude of freely-communicating cells, of various shapes. They are chiefly derived from the fibrous envelope of the penis, from which they pass on either side inwards, connecting the septum with the outer sheath. The substance of these cords and bands is traversed by the arteries of the erectile tissue, which usually run very tortuously within them: the spaces or cells between the cords and bands are occupied by the veins. Each cord or band has in its interior an artery of a size proportionate to its own: where one seems to branch, the artery within it also divides, and sends a branch to its division; and where the bands and cords unite, the arteries

[1] The descriptions of the muscular fibres are drawn chiefly from the gland-ducts of large animals; such as the ureters and bile-ducts of sheep and horses. (See especially Meyer, De musculis in ductibus eff. gland., diss. inaug., Berol. 1837; and in Froriep's N. Notizen Marz 1838; and Henle, Allg. Anat. p. 934.) The same structures are presumed to exist in man rather from analogy and from pathological facts than from actual observation. Tourtual has discovered true muscular fibres in an hypertrophied human ureter, (Muskelfasern im erweiterten Harnleiter, &c., Muller's Archiv, 1840;) but there are no detailed accounts of the muscular coats of other human gland-ducts.

[2] Allg. Anat. p. 996.

[3] *On all of them*, Berres, (Anat. Microsc.;) Müller, (Physiologie, bd. i. and Archiv, 1840, p. 101;) Burdach (Physiology, bd. v.;) Henle, (Allg. Anat. p. 996:) and Gulliver, (App. to Gerber's General Anat.) *On the spleen:* Giesker, (Splenologie, Zurich, 1835, quoted by Henle;) Müller, (Archiv, 1834;) Valentin, (Ueber der Verlauf der Blutgef. in dem Penis, &c. Müller's Archiv, 1838;) Bischoff, (Müller's Archiv, 1838;) Marcus, (Diss. de funct. lienis, 1838;) and the earlier works of Hewson, Heusinger, Schmidt, and Meckel. *On the renal capsules:* Nagel, (Ueber die Structur der Nebennieren, Müller's Archiv, 1836;) Bergmann, (Diss. de glandulis suprarenalibus, 1839;) Pappenheim, (Vermischte Beobacht. Muller's Archiv, 1840;) and Berres, (Ueber den zarten Bau der Drusen, Oester. Med. Jahr. 1840.) *On the thyroid and thymus glands:* A. Cooper, (The Anatomy of the Thymus-gland;) Berres, (*l. c.* in Oester. Jahrbuch;) Haugsted, (Thymi in homine, &c. quoted by Henle.)

[4] The only two complete descriptions are those of Müller, (Entdeckung der bei der Erection . . . wirksamen Arterien, Archiv, 1835;) and Valentin, (Ueber den Verlauf der Blutgefässe in dem Penis, Müller's Archiv, 1839,) with a subsequent note by Müller. This account is so far drawn from both equally that they will be referred to only when they differ.

within them also unite and anastomose. The terminations of the arteries open in an uncertain manner from the cords which contain them into the veins which are placed in the interspaces of the cords and bands, and of which the latter, since they are covered by the lining venous membrane, may be said to form the walls.

These bands and cords which form, as it were, the skeleton of the erectile tissues are dense structures, composed of several different tissues. From its surface inwards each consists of the following layers: 1st, a portion of the common lining membrane of the venous system, which is here, as elsewhere, in contact with the venous blood; 2d a mixed layer of elastic and fibro-cellular tissue, which corresponds to the outer wall of the vein or venous space; 3d, a layer of fasciculated fibres, exactly resembling those of organic muscle, and usually directed in the longitudinal axis of the penis; 4th, one of bundles of fibres, like those of tendon; 5th, the artery which occupies the middle of the band or cord, and on the other side of which the same succession of layers is usually repeated, though less regularly, because on one side or other of each band there are generally branches given off.[1]

Thus far there is little question as to the structure of the true erectile tissue: the chief doubt is in regard to the mode in which the arteries pour their blood into the veins. Müller believes[2] that they have two distinct modes of termination; that some of them—the nutritive branches—form a common capillary network, which leads in the usual manner to the minute veins; but that, besides these, there are others, which he names *helicine arteries.* These, he says, form branches about a line long and $\frac{1}{150}$ of an inch thick, which proceed, usually at right angles from the branches of the arteries within the bands and cords, and hang loose, with blunt and curved or twisted, but not open, extremities in the venous spaces. Through these the blood is, in erection, supposed to be poured into the veins, which are thus distended. Valentin,[3] on the other hand, maintains that the appearance of the helicine arteries is artificially produced; that, in cutting across a collection of cellular spaces like those of the corpus cavernosum, many of the bands and cords which bound and traverse them must be divided; and that since each of these contains in its interior a tortuous artery, it must, when cut away from one of its connexions and floated out in water, present the appearance of the end of an artery hanging loosely in a venous space. He believes, therefore, that the minutest branches of the arteries of the penis, after various anastomoses, dilate, penetrate the fibrous tissue in which they are inclosed, and pass into the smallest venous spaces, or (as it may be better expressed, in accordance with Berres' injections,) into a dense network of comparatively large veins, whose diameter is four or five times greater than that of the arteries, and the interspaces between which are formed by the intersecting fibrous bands and cords, in the substance of which the arteries run.

APPENDIX.

A. An observation which the writer, in common with many others, had overlooked, but which seems to confirm in some measure Dr. Barry's account of the blood-corpuscles, is made by Mayer, of Bonn.[4] He says that he has often found floating in the blood of men, mammalia, and birds numerous pellucid,

[1] This description is drawn from the corp. cavern. of the ass; the same structures exist, but are less discernible in that of man.

[2] And he is, in general, confirmed by Krause, (Vermischte Beobacht, Müll. Archiv, 1837;) Hyrtl, (Oester. Jahr. 1838, bd. xix.;) and Erdl, (Muller's Archiv, 1841.) But see on their observations, Valentin, Repertorium, 1841.

[3] His observations are confirmed by Berres, (Ueber den zarten Bau der Drusen, Oester. Jahrb. bd. xxxi.)

[4] Ueber freie Primitivfasern im Blute. (Froriep's N. Notizen, April, 1841.)

clear, straight, smooth, or somewhat granulated fibres, varying from $\frac{1}{1100}$ to $\frac{1}{350}$ of an inch in length, (but sometimes much longer,) and about $\frac{1}{20000}$ of an inch broad. In a pea-fowl which had died of acute abdominal inflammation they were as numerous as the blood-globules themselves, appeared notched, or granulated, and had independent motion. He believes also that similar fibres exist in several parts of the body: e. g. in the parenchyma of the spleen and kidney.

B. While the Report was being printed, the observations of Volkmann and Bidder, of which a part are inserted, arrived in the March number of Froriep's Notizen. They seem to prove anatomically the important fact, that the sympathetic nervous fibres do form an independent system, whose centres are the ganglia. At the points of junction of the two systems some of the sympathetic fibres (traceable by the characters described at p. 262,) run towards the spinal cord, others towards the periphery. The proportionate numbers passing in each direction vary in the different places of connexion. From the branches connecting the sympathetic with the eighth and ninth spinal nerves of the frog, for instance, scarcely any sympathetic fibres proceed centrally; yet these are the largest of the branches which are commonly described as the *origins* of the sympathetic system from the spinal cord. On the whole, the sympathetic at these points of junction always gives more fibres that it receives. It must therefore have some source of fibres of its own; and this source is found to be chiefly in the ganglia, both spinal and sympathetic, (specially so called,) and, in a less degree, in the cord itself.

If, it is argued, the sympathetic derived at these parts all its fibres from the spinal cord, such fibres ought to be found in duly proportionate number in the roots of the spinal nerves. But it is not so. In the case of the fourth spinal nerve of the frog, for instance, the branch connecting the sympathetic with it, is larger than *it* is. Therefore, since all the fibres of the connecting branch run centrally, they ought, if they have their origin from the spinal cord, to be found in great numbers in the roots; but there are fifty times more cerebro-spinal than sympathetic fibres in the roots of this fourth nerve. The sympathetic fibres which pass into its trunk cannot be traced further than to the ganglion on the posterior root. From this ganglion, therefore, they probably have their origin; and they are destined, as those in the other trunks are, chiefly to the posterior branches of the nerve. The anterior branches also contain sympathetic fibres, but they are derived, not from the spinal ganglia, but from those of the sympathetic itself.

June 11, 1842.

Medical Reform.

It being now generally understood that Sir James Graham's bill for remodelling some parts of the profession is likely to be *introduced* into Parliament this session, we earnestly call on all our readers—especially on those of the class of general practitioners—to be early on the watch to learn and to study its provisions. As it cannot *pass* this session, there will be time for examining and, if need be, for resisting it. In the meantime, we also strongly recommend to their notice the scheme of rational and practical reform, proposed in an admirable pamphlet just published by Sir James Clark.* Unless as much is granted to *the great body of the profession*—that is, to the GENERAL PRACTITIONERS—as is contended for in this pamphlet, the reform of the CORPORATIONS, said to be contemplated by the bill, will be, to say the least, utterly nugatory as a means of healing existing discontents and discords, and of raising the profession, generally, in the estimation of the public and their own.

* Remarks on Medical Reform, in a Letter addressed to Sir James Graham, Bart., &c. &c. By Sir Jas. Clark, Bart., M.D., F.R.S., Physician to the Queen, &c.—Lond. Murray.

Report on the present state of our Knowledge respecting Respiration and Animal Heat, and on Professor Liebig's new Chemico-Physiological Theory. By Robert Dundas Thomson, M.D.

Before stating Liebig's views it seems necessary to say a few words on those previously entertained. We shall suppose our readers acquainted with the earlier history of the progressive discoveries in this department of chemistry. If they are, they must be as much surprised as we are at some late pretensions of M. Dumas to originality. "The fact," he says, "of the absorption of oxygen in the act of respiration has been long known, as well as its transformation into water and carbonic acid; but a more recent idea is that the oxygen does not burn the hydrogen and carbon at one effort. It passes before the completion of this simple result, through a series of successive combinations, of which the carbonic acid and water are only the termination and not the point of departure, as had been previously thought," (Dumas, Cours de 1839.) In another passage he fairly claims the theory of Lagrange and Hassenfratz; and to prevent mistakes we quote his own words: "Nous avons professé que le sang s'arterialise dans le poumon, sans produire de chaleur, et qu'il respire réellement dans les capillaires du corps tout entier en produisant l'acide carbonique, l'eau et la chaleur. Cette théorie est exposée dans une thèse du 15 Juillet, 1839. Il serait facile de prouver qu'elle a été professée en 1838." (Ann. de Chim. Jan. 1842.) Now this is nothing more than a brief epitome of the theory of Lagrange and Hassenfratz; and how such a distinguished chemist as Dumas could have claimed it as peculiar to himself, is one of those mysteries which require explanation.

Dumas has asked "to whom belongs the true theory of animal heat?" "This theory belongs incontestably," he answers "to Laplace and Lavoisier." Unquestionably the theory received much of its development from the labours of these French philosophers. But it should never be forgotten that Dr. Black was the first to point out the source of the heat. It is the more necessary to attend to this position, because neither Lavoisier nor his successor Dumas ever alludes to the name of our illustrious countryman. Even Despretz has been undervalued by Dumas in estimating the claim of different chemists to the development of the theory of respiration. The following results are deducible from the experiments of Despretz:

1. It appears that during the combustion of an avoirdupois pound of carbon the quantity of heat evolved is sufficient to melt 104·2 lbs. of ice. Now if the latent heat of water be 140°, 104·2 lbs. of ice will require 14588° of heat to melt it; or in other words the heat evolved during the combustion of 1 lb. of carbon would heat 1 lb. of water 14·588°. 2. The oxygen required to consume 1 lb. of carbon amounts to $2\frac{2}{3}$ lbs., which is equivalent to 55 082 cubic inches, at the temperature of 60°. This oxygen combines with carbon, and is converted into its own volume of carbonic acid gas. 3. The 55·082 cubic inches of oxygen when converted into carbonic acid give out 14·588° of heat. Hence every $3\frac{3}{4}$ c. in. of oxygen when converted into carbonic acid give out 1° of heat. 4. Despretz farther showed that when 1 lb. of hydrogen is burned a quantity of heat is evolved capable of melting 315·2 lbs. of ice, or the heat evolved would heat 1 lb. of water 44·128°. But for this combustion 8 lbs. of oxygen are required. Now 8 lbs. of oxygen are equivalent to 165·246 c. i. Hence every $3\frac{3}{4}$ c. i. of oxygen, when they combine with hydrogen, evolve 1° of heat.

From these results we draw the conclusion that the heat evolved during the combustion of carbon and hydrogen is proportional to the quantity of oxygen gas consumed. But a somewhat different result takes place in the combustion of pulmonary respiration, according to the researches of the same laborious philosopher. For if we reckon the animal heat evolved in his experiments at 100° then the portion of it due to the combination of the oxygen of the atmosphere with carbon and hydrogen during the circulation of the blood through the body will be 82°. Consequently 18° or about $\frac{1}{5}$ must be owing to other processes not yet detected.

Dumas objects to the experiments of Despretz, that an animal when surrounded by a cold fluid has its temperature lowered, and that therefore results obtained under such circumstances cannot be relied upon as strict expressions of truth.

Admitting then that the oxygen inspired by the lungs combines gradually with carbon in its progress through the system, and that carbonic acid is the result of this chemical union, it is natural to inquire if the carbonic acid is simply absorbed by the blood, or if it is transferred in any other form through the circulation. Mitscherlich, Gmelin, and Tiedemann have proposed a theory which they think is adequate to account for the disposal of the carbonic acid. The grounds of their theory are that acetic and lactic acids exist in a free or combined form in most of the secretions, and also in the blood. These acids they suppose to be generated in the body. They have determined likewise, that venous blood contains 12·3 per cent. of alkaline carbonates, while arterial blood contains only 8·3 per cent. They conceive that the air acts upon the blood during respiration so as to produce acetic acid, which, meeting with the carbonate of the venous blood, sets free the carbonic acid and combines with the alkaline base; and that the oxygen of the inspired air unites in part directly with carbon and hydrogen, producing carbonic acid and water, and in part enters into combination with the organic compounds contained in the blood, (Müller's Phys. by Baly, p. 343.)

After such a perspicuous announcement of this ingenious theory, which has now been before the public of this country for five years, it is rather startling to meet with the following statement brought forward by Dumas as the enunciation of a theory peculiar to himself. "The oxygen absorbed serves to burn lactate of soda, and in general salts of soda, which it changes into carbonate of soda. The lactic acid transforms the latter into lactate, and disengages carbonic acid. This lactic acid proceeds from the saccharine or amylaceous aliment." (Ann. de Chim. iv. 3d ser. 142.)

The only difference in the two statements is with respect to the origin of the lactic acid; and this could not have been otherwise, because it is only since the announcement of the German philosophers, that the derivation of lactic acid from sugar and starch has been clearly demonstrated. We confess that we have some difficulty in accounting for the conduct of Dumas. In general he is better acquainted with the history of his science than any of his countrymen. So it might be said was Lavoisier, and yet he never acknowledged in his theory of combustion, the obligations he lay under to the discoveries of Black. Nay, he never noticed the discovery of oxygen by Priestley, (who had, in the simplicity of his heart, communicated the discovery to him,) but actually spoke of the new element as if it had been discriminated by himself alone.

We now proceed to notice some of the recent labours of Professor Liebig in this most interesting department of physiology; we are happy in being enabled to give an accurate outline of his new chemico-physiological theory, derived from the most authentic sources. The professor is now publishing a work on the subject, from which we anticipate an impulse calculated to lead to the most momentous discoveries in medical science. This work will soon appear in English as well as in German.

The source of animal heat. Absorption of nourishment and of oxygen are the first conditions for the sustenance of animal life. During every moment of his existence man imbibes oxygen by his respiratory organs. According to Menzies, a human adult takes up from the atmosphere 850 lbs. of oxygen during the year; and yet at the end of that period his weight remains perfectly unchanged, or only differs perhaps by a few ounces. What becomes of this enormous quantity of oxygen which is thus consumed, is a natural subject of inquiry. No part of this oxygen remains in the body, but it is again discharged under the form of a compound with carbon or hydrogen. The carbon and hydrogen of certain constituents of the body have united with the oxygen absorbed through the lungs and skin, and are expelled in the shape of carbonic acid and water. If we consider, with Lavoisier and Seguin, the quantity of oxygen consumed by an adult

daily to be 30¼ oz =15,661 Fr. grains, and reckon the quantity of blood 24 lbs. of which 80 per cent. is water, it follows that to turn the whole of the carbon and hydrogen of the blood into carbonic acid and water would require 66040 Fr. grs. or upwards of 120 oz. of oxygen; and this operation would be completed in 4 days and 5 hours. The food supplies the carbon and hydrogen required in this process. From a carefully conducted set of experiments made upon 856 soldiers, Liebig infers that an adult takes up daily 13 oz. of carbon. This was determined by weighing the food and fæces daily for a month. The fæces amounted to 7 oz. daily: they contained 75 per cent. of water, and the dry residue 42¼ per cent. carbon, 13·15 per cent. of ashes; 100 parts of fresh fæces, therefore, contain 11·31 carbon; or very nearly as much as an equal quantity of fresh meat. The 13 oz. of carbon which are daily taken into the system are discharged by the skin and lungs in the form of carbonic acid. For their conversion into carbonic acid these 13 oz. require 34⅝ oz. of oxygen. According to the experiments of Boussingault, a horse takes up, in 24 hours, 74½ oz. of carbon, and a milch cow 66¼. To convert these into carbonic acid the horse requires from 13 to 14 lbs.; and the cow from 11 to 12 lbs. of oxygen. Now as none of the oxygen is thrown off from the system in any other form than that of carbonic acid and water, and as the carbon and water are derived from the food, it follows that the quantity of nourishment required for the support of the system is in direct proportion to the quantity of oxygen taken up. Two animals which consume in equal periods of time unequal quantities of oxygen by the skin and lungs, require in the same proportion an unequal weight of food. In equal periods the consumption of oxygen depending on the number of respirations, it is clear that in one and the same animal the quantity of food digested varies according to the strength and number of the respirations. A child whose respirations are more frequent must require proportionally more nourishment than an adult, and can less easily bear hunger. A bird dies from want of food on the third day. The serpent which when placed for an hour under a receiver consumes scarcely so much oxygen as to enable the resulting carbonic acid to be detected, lives for three months and even longer without food. In a state of rest the number of respirations is less than when the body is actively employed. The quantity of food required in both circumstances must bear the same proportion. An excess of food and a deficiency of inhaled oxygen (or exercise), as well as great exercise (which enforces a greater imbibition of nourishment) and weak digestive organs, are incompatible with each other.

The quantity of oxygen, according to the view of Liebig, which an animal absorbs in the lungs is not altogether dependent on the number of respirations, but it is closely connected with the temperature of the inspired air. The cavity of the chest of an animal remains always possessed of the same capacity; at each respiration the same volume of gas enters. But the weight of this volume, and therefore the weight of the oxygen contained in it, is not always equal. When an adult breathes 46,037 cubic inches of oxygen of the temperature 77°, the weight of the oxygen amounts to 30½ oz. But if the same volume of oxygen be breathed at the temperature of 32°, the weight of oxygen will be 32¾ oz. In summer and in winter, at the Pole and at the Equator, we breathe the same bulk of air; and while in summer we inhale in an equal number of inspirations 29½ ounces, the quantity of oxygen inhaled at 32° is 32¾ ounces; in Sicily (at 95°) 26¾ ounces; at 14°, 33¾. At a lower temperature we expire more carbon than at a higher temperature, and we must in the same proportion employ more or less carbon in our food; in Sweden more than in Sicily; and in Germany an eighth part more in winter than in summer. Even in comparing equal quantities of food in cold and warm countries, we find the quantity of carbon to be very unequal. The fruits which the inhabitants of the tropics employ contain in the fresh state 12 per cent. of carbon; while the fat and oil of the Esquimaux contain from 66 to 80 per cent. of carbon. There is no great difficulty in practising moderation in warm countries or in enduring hunger for a considerable period under the equator; but cold and hunger soon

produce death. According to Liebig, therefore, the reciprocal action of the constituents of the food and of the oxygen disseminated through the circulation in the body is the source of animal heat.

All living beings whose existence depends upon their absorption of oxygen, are dependent for one source of their animal heat on the atmosphere which surrounds them. This truth applies to all animals. It extends to the germinating seed, to the flowers of plants, and to fruits which are attaining maturity. Heat is only evolved in those parts of animals to which the arterial blood and the oxygen imbibed by respiration are distributed. Hair, wool, and feathers possess no peculiar temperature. The higher temperature of animals, or in other words the greater extrication of heat in animals, is always the consequence of a combination between a combustible substance and oxygen. For in whatever form the carbon combines with the oxygen, the act of combination cannot take place without the evolution of heat. Whether the union is effected slowly or with rapidity, the resulting heat is ultimately exactly the same. The carbon of the food which is converted into carbonic acid in the bodies of animals must evolve the same amount of heat as if it were burned in the air or in oxygen gas. Animals which breathe rapidly, and therefore consume much oxygen, possess a higher temperature than those which breathe more slowly. The temperature of a child is $102\frac{1}{2}$°, that of an adult 98 to 100°. The heat of a bird is 106 to 109°, and is greater than that of mammiferous animals, which have a temperature varying 99 to 106°; or than that of fishes, whose heat exceeds that of the surrounding water in which it is immersed by only a degree or two. All animals are warm-blooded, but it is only in those which respire by lungs that we find their peculiar temperature completely independent of the temperature of the surrounding media. An animal is a heated body which is acted on by the surrounding atmosphere, as all heated bodies are; it imbibes heat when the surrounding atmosphere is hotter than itself, and it gives out heat when the atmosphere is colder. It hence follows that at the Pole, with the temperature below zero, the loss of heat must be much more rapid than at the equator. Yet the blood of the Esquimaux and that of the inhabitants of the tropics possesses the same temperature—a clear proof that the heat must be renewed more quickly in winter than in summer, and more rapidly at the Pole than at the Equator. In different climates it is obvious that the quantity of oxygen consumed in respiration must depend on the temperature of the external air. With the loss of heat by cooling, the quantity of respired oxygen increases. The carbon and hydrogen necessary for combination with the oxygen must vary in a similar proportion. It is clear that the compensating heat will be produced by the reciprocal action of the constituents of the food which combine with the inspired oxygen.

The animal body may in some respects be compared to an oven which we supply with combustible materials. In like manner, whatever forms the food gradually assumes in the body, and whatever transformations it may undergo, the last alteration is a conversion of its carbon into carbonic acid, and its hydrogen into water; while the azote and unburned carbon are excreted in the urine and fæces. To retain the temperature of an oven constant, it is necessary to supply combustible materials in unequal quantities, according to the changes of the exterior temperature. In relation to animals the food is the combustible matter; by the due access of oxygen and the oxidation of the food, heat is evolved. In winter, when by movement in colder air, the quantity of the inspired oxygen increases, the necessity for nutriment rich in carbon and hydrogen varies in the same proportion; and in compensation for this necessity we obtain the most perfect protection against the severest cold. A hungry man shivers; and the animals of prey of northern climates are more voracious than those of southern countries. Our clothing is merely an equivalent for food; in proportion to the warmth of our clothing the necessity for eating diminishes. The quantity of food, therefore, which is used depends on the number of respirations, on the temperature of the air which we breathe, and on the quantity of heat which we

give out to the air. No isolated influence can alter this law. The European, when residing in tropical countries, endeavours in vain to stimulate his organs with powerful condiments to imitate the appetite which he indulged in at home. English patients, whose digestive organs are out of order, are sent to southern countries, where the quantity of inspired oxygen is diminished and the nourishment of the body proceeds with less labour to the organs of assimilation. In summer the most prevalent complaints in Germany are liver-diseases (carbonaceous diseases;) in winter lung-diseases, (oxygenous diseases.)

The whole process of respiration is clearly exhibited when we take a view of the condition of a man or animal under abstinence from all food. There will be, as before, oxygen abstracted from the air, and carbonic acid and water expired, because the number of respirations remain unaltered. We know with precision from whence the carbon and hydrogen emanate; and with the continuance of the abstinence we see the carbon and hydrogen of the body diminishing. The first effect of hunger is the disappearance of the fat. This fat can be detected neither in the scanty fæces nor in the urine; its carbon and hydrogen are thrown off by the skin and lungs in the form of a compound with oxygen. It is obvious that these constituents have served for the purposes of respiration. Every day $32\frac{1}{2}$ oz. of oxygen are inspired, and these must remove their equivalents of carbon to form carbonic acid. When this combination ceases to go on respiration terminates—death has taken place. The time required for starving an animal to death depends on its fatness, on the state of its activity, on the temperature of the air, and lastly on the presence or absence of water.

In all chronic diseases death occurs from the same cause—from the action of the atmosphere. When the materials fail which are destined for the sustenance of respiration in the organism; when the organs of the sick refuse to perform their functions; when they lose their capacity to transfer the food into that state necessary for its combination with the oxygen of the air—then their own substance, the fat, brain, muscles, and nerves will be attacked. The peculiar cause of death is, in this case, the process of respiration, the influence of the atmosphere.

Oxidation, not the nerves, the cause of animal heat. None will deny the importance of the nervous system in the process of respiration, for no change of state can occur in the animal economy without the influence of the nerves. By their action the intestines bring the combustible materials into a condition fit for their combination with oxygen; and in the absence of their functions the whole act of the imbibition of oxygen must assume another form. Yet it cannot be doubted that the influence of the nerves in respiration, and in the production of animal heat, has been much overrated.* Liebig even goes so far as to declare that the idea of the evolution of animal heat by the action of the nerves is an absurdity; for if we exclude chemical action, or changes in the arrangement of the elementary particles as a condition of nervous agency, it means nothing else than to derive the presence of motion, the manifestation of a power, from nothing. But no power can come of nothing.†

That the quantity of heat evolved by the combustion of 13·8 oz. of carbon is amply sufficient to account for the temperature of the human body, may be readily gathered from the employment of numbers. An oz. of carbon burned would evolve 14207°, and 13·8 oz. would therefore give out 197477·3°. This would suffice to boil 136·8 lbs. of water at 32°, or to convert 24 lbs. of water at 98° into vapour. If we consider then the quantity of water vaporized through the skin to be in 24 hours 48 oz. (3lbs.), there will remain 146380° of heat

* Some have endeavoured to discover a source of animal heat in the contraction of the muscles; forgetting that in their expansion an equal quantity of heat was absorbed. All genuine sources of heat in the body must probably be referred to chemical action.

† The consideration of power is most important. Heat, it is said, may be produced by *friction*. This mode of expression keeps out of view the cause of friction; friction being only an effect of power.

which are dissipated by radiation, by heating the expired air, and by the excrementitious matters. Liebig considers that experiments made upon the quantity of carbonic acid expired by the usual tests are of no value; because so much depends upon the density and temperature of the air, and other circumstances, that it is impossible to calculate accurately. The degree of motion, labour, or exercise, the amount and quality of the food, the comparative warmth of the clothing, and also the time when the food is taken, are important elements in this mode of investigation. Liebig prefers the method already referred to, by determining the composition of the food and that of the excretions. Prisoners in the house of correction at Marienschloss, where labour is enforced, consume $10\frac{1}{2}$ oz. of carbon daily, while in the house of arrest at Giessen, close by the Hessian laboratory, the consumption of carbon is only $8\frac{1}{2}$ oz. The quantity consumed by soldiers engaged in healthy exercise, we have already stated to be 13·8 oz., while in a family of five adults and four children, the average daily consumption of carbon for each was $9\frac{1}{2}$ ounces.

The constituents of the blood exist in plants. From the unwearied researches of Liebig and his pupils, the important fact has forced itself upon our acceptance, that where vegetable life ends, animal life begins. Plants prepare vegetable fibrin, vegetable albumen, and vegetable casein for the nourishment of animals. These are swallowed by animals as food, and are conveyed into the blood without even modification. The albumen and fibrin of the blood existed in plants under the denomination of vegetable fibrin and albumen; they are absolutely identical. Hence Liebig observes, "we may say that the animal organism merely gives to blood its form, and that it is incapable of creating blood out of other substances, which do not already contain the chief constituents of that fluid." The three bodies, which we have described as serving the purpose of supplying substance to the body, all contain azote. What then, it may be asked, is the use of starch, sugar, gum, pectin, &c., which contain no azote, and do not enter into the formation of the solids of the system? Graminivorous animals must be supplied with one or other of these substances. If they are withheld, death rapidly ensues. The young of carnivorous animals are nourished in a somewhat similar manner to graminivorous animals; for milk contains only one azotized principle, *casein*, while its other constituents are butter and sugar; but casein is identical in its composition with the albumen of peas, beans, and lentils. These vegetables are capable of producing animal blood; from them, therefore, the mother's blood may be formed, and from the blood is secreted the milk which serves as nourishment to the young. The utility then of the casein and similar compounds is sufficiently obvious; but that of the butter and sugar is not so easily detected. Liebig considers that they supply a certain amount of carbon and hydrogen to the azotized constituents of the food, or, in other words, that they afford a supply of carbon and hydrogen in excess, which is expended on the production of animal heat, and serves to protect the organism from the action of the oxygen of the atmosphere. He adduces several familiar instances in support of his position: the boa-constrictor, when a bird or rabbit is given to it, retains its original weight, consuming the flesh, blood, bones, and nerves of its victim, and, expelling a pure white substance like chalk, urate of ammonia, which contains for every equivalent of azote two of carbon; but the food which it had swallowed contained eight equivalents of carbon to one of azote. This immense consumption of carbon can only have taken place through the skin and lungs in the form of carbonic acid gas. Again, the excrement of lions consists chiefly of bone earth, with mere traces of compounds of carbon; their urine contains not urate of ammonia, but urea, in which the azote is to the carbon as one to one; but their food contained one of azote to eight of carbon. The whole of the excess of carbon has disappeared, in the process of respiration, as carbonic acid; and its accompanying hydrogen in the form of water. Now, if the flesh which constituted the food of the lion, had been burned in a furnace, we should have ob-

tained nearly the same results; the only difference would have been in the form of the azotic compound. The azote would have appeared in the state of carbonate of ammonia, while the remaining carbon and hydrogen would have been converted into carbonic acid and water; precisely as in the process of respiration, the combustible parts would have assumed the form of ashes, and the unconsumed carbon would have been given out as soot or lampblack. Now, in the animal the solid fæces are only the incombustible or imperfectly burned parts of the food.

The bile supplies carbon to combine with oxygen. Although the oxidized products given off by respiration correspond with the carbon and hydrogen taken into the system in the form of food, and with the nitrogen in the shape of a compound containing the same elements as carbonate of ammonia, we are not to conclude that the only use of the food is to produce carbonic acid, water, urea, and uric acid; on the contrary, Liebig infers that the tissues are continually undergoing metamorphosis, that their transformation is the source of the excretions, while the blood deposits new matter in their place. The nitrogen of the exhausted or transformed organs appears in the bladder in the form of urine; the carbon of the transformed tissues appears in the gall-bladder in the shape of bile, a compound of soda (choleate of soda), and these transformations produce animal heat. But the bile is not an excretion, neither is it merely a stimulant to the intestinal canal. What is it, then? Chemical research shows us that it is taken up by the absorbents even of the rectum; when injected by the rectum, the whole of the bile disappears with the injected fluid; none whatever can be detected in the fæces. Bile, therefore, differs from urine in this respect, that the urea and uric acid of the urine are expelled for ever as useless to the system, while the carbon of the bile serves as an ulterior combustible material. The food of the carnivora is converted into blood which is destined for the reproduction of organized tissues; and by means of the circulation, a current of oxygen is conveyed to every part of the body. The globules of the blood, it can be demonstrated, have no share in supplying nutrition to the system; but they serve to transport oxygen through the sanguineous canals, and give it up in order to combine with carbon and hydrogen in the capillary vessels; for here the current of oxygen meets with the compounds produced by the transformation of the tissues, and combines with their carbon to form carbonic acid, and with their hydrogen to form water. Every portion of these substances which escapes this oxidating process is sent back into the circulation in the form of the bile, which by degrees gradually disappears. In carnivorous animals, all the food is capable of forming blood. Their excrements, with the exception of the urine, contain only inorganic substances; no bile and no soda are present; but although soda is an important constituent of the bile, we do not find it in the fæces except in the shape of common salt and sulphate of soda, which exist in all the animal fluids. The soda of the bile must, therefore, have returned from the alimentary canal into the organism. The bile affords a plentiful source of carbon, much more fruitful perhaps than might appear at a superficial glance: it is estimated that a man secretes daily from 17 to 24 oz. of bile, a large dog 36 oz., a horse 37 lbs., or 592 oz., of which 59·2 oz. are solid matter; but no portion whatever of the excrement is bile. During the digestive process, therefore, the whole of the bile is returned into the blood, the soda reappears in the newly-formed blood, and, lastly, we find it in the urine in the form of phosphate, carbonate, and hippurate of soda. A similar observation applies to human bile. Man secretes from 9640 to 11520 grs. of bile daily; but only $\frac{1}{80}$ of a substance in the slightest degree resembling bile can be detected in the fæces.

From these data and the considerations previously stated, the remarkable conclusion is necessarily drawn, that all the carbon of the food is ultimately given out in the form of carbonic acid; it further appears that the carbon of the carbonic acid given off with that of the urine, the nitrogen of the urine and the hydrogen given off as ammonia and water; these elements, taken toge-

ther, must be exactly equal in weight to the carbon, nitrogen, and hydrogen, of the transformed tissues, and also to the carbon, nitrogen, and hydrogen of the food. If this were not the case; the animal would alter in its size, and so it happens with the young of animals. They increase appreciably every day—and how do they increase? In the young animal, the assimilative process is more energetic, more intense, than the process of transformation: the circulation is more rapid, the respirations are more frequent, and for equal bulks the oxygen consumed must be greater in the young than in the adult animal; but as the metamorphosis of organized parts proceeds more slowly, there would obviously be a deficiency of carbon and hydrogen to combine with the oxygen inspired. To obviate this difficulty, Infinite Wisdom has supplied to the young the carbon and hydrogen of the butter of milk, and the carbon of sugar, for the support of the respiratory system at an age when a greater resistance is opposed to the metamorphosis of existing tissues, or, in other terms, to the production of compounds which, in the adult, are formed in quantity amply sufficient for the purposes of respiration. The young animal receives the constituents of its blood in the casein of the milk, a transformation of existing tissues goes on, for bile and urine are secreted; the matter of the metamorphosed parts is given off in the form of urine of carbonic acid and water; but the butter and sugar of milk also disappear, and cannot be detected in the fæces. The butter and sugar of milk are given out in the form of carbonic acid and water; and their conversion into oxidized products furnishes the clearest proof, that far more oxygen is absorbed than is required to convert the carbon and hydrogen of the metamorphosed tissues into carbonic acid and water. These considerations, Liebig conceives, leave no room for doubt, that nature has added to the food of the young of carnivorous animals substances destitute of azote, which their organism cannot employ for nutrition, strictly so called, that is, for the production of blood; substances which may be entirely dispensed with in their adult state. In the young of carnivorous birds, the want of all motion is an obvious reason why there is a diminished waste in the organized parts; hence milk is not provided for them.

Sugar, starch, and gum supply carbon to burn the excess of oxygen inspired. In graminivorous animals, we find that substances possessing an identical composition with sugar of milk, are required for their existence. Starch, sugar, and gum, are all similar in their composition, as the following table shows.

Starch	12 atoms carbon	and 10 atoms water.
Cane sugar	„	1
Gum	„	4
Sugar of milk	„	2
Grape sugar	„	4

Starch is readily converted into sugar, and the change takes place on the ripening of fruit; unripe apples contain starch, but not a trace of it can be detected in their ripened state. Gum resembles starch in not being fermentible. Sugar of milk is in the same condition, but it may be fermented by exposing it to heat in contact with putrefying cheese: in this case it is, however, first converted into grape sugar. The function performed on the vital processes of graminivorous animals by these substances, is pointed out in a clear and convincing manner, when we consider the very small relative amount of carbon consumed by them in the azotized constituents of their food, which bears no proportion whatever to the oxygen absorbed by the skin and lungs. A horse, for example, in Germany may be kept in good condition by feeding him daily with 15 lbs. of hay and $4\frac{1}{2}$ lbs. of oats: now, as hay contains $1\frac{1}{2}$ per cent and oats 2·2 per cent. of azote, the horse receives no more daily than $4\frac{1}{2}$ oz. of azote, corresponding to 8 lbs. of blood; but along with this azote that is combined with it in the form of fibrin and albumen, the animal receives only about $14\frac{1}{2}$ oz. of carbon; only 8 oz. of this can be employed to support respiration: for with the nitrogen expelled in the urine, there are combined in the form of urea 3 oz., and as

hippuric acid 3½ oz. of carbon. But man consumes daily about 14 oz. of carbon, and the horse, according to Boussingault, 79 oz. daily; hence it appears that in the azotized constituents of its food, the horse receives rather less than the fifth of the carbon necessary for its respiration. The remaining four-fifths must inevitably, therefore, be supplied by the starch, sugar, or gum, which constitute the complementary elements of nourishment.

It appears now obvious that in graminivorous animals, whose food contains such a small proportion relatively of the constituents of blood, the process of the transformation of existing tissues must proceed much less rapidly than in carnivorous animals, otherwise the vegetation of the globe would scarce be sufficient to maintain them. A nation of hunters eat large quantities of animal food and little else. When they are kept within certain limits, its members must increase but sparingly, because the carbon which they consume is supplied mostly by animals. These animals must reside within the boundaries of their nation, and must be supplied with their carbon from the plants growing within these limits. Can we wonder then at the depopulation of the Indian parts of North America? Fifteen lbs. of flesh contain no more carbon than 4 lbs. of starch. If the savage, therefore, with *one* animal and an *equal* amount of starch, could maintain life for a certain number of days, he would be compelled if confined to flesh alone, in order to procure the carbon necessary for respiration, to consume during the same period *five* such animals. How close then is the connexion between agriculture and the increase of the human species! The cultivation of our crops has ultimately no other object than the production, on the smallest possible space, of a maximum of those substances which are adapted for assimilation and respiration. When man is restricted to animal food, he respires like the carnivorous animals, at the expense of the matters produced by the transformation of the organized tissues, and in order to effect this he requires, like the lion and other beasts of prey, to undergo a vast amount of muscular exercise; he is compelled to expend a given amount of *power* merely to supply matter for respiration. Cultivation is the economy of power. Science teaches us the simplest method of obtaining the greatest effect at the smallest expenditure of power, and to produce with given means a maximum of power.

When we compare the capacity for increase of mass in the assimilative power of carnivorous and graminivorous animals, a most marked difference is at once observable. A spider sucks with great voracity the blood of the first fly, but is not excited or disturbed by a second or third. A cat will eat the first, or perhaps the second mouse presented to her, but even if she kills a third she does not devour it. But, on the contrary, a cow or sheep in the meadow continues to eat with little interruption from sunrise till sunset. Their systems possess not merely the power of supplying the waste created by the metamorphosis of the tissues, but of converting into organized tissue all the food they devour. The excess of blood produced is converted into cellular and muscular tissue; the graminivorous animal becomes fleshy and plump, while the flesh of the carnivorous animal is always tough and sinewy. The animal in this state eats and reposes merely for digestion. Want of exercise is however equivalent to a diminution in the consumption of oxygen, and the excess of carbon is deposited in the cellular tissue in the form of *fat*.

Fat originates from starch, &c. When the horse and ox are in their normal condition, their urine contains benzoic acid (with 14 equivalents of carbon), but when the animal is kept in the stall, hippuric acid appears (with 18 equivalents of carbon). Wild animals are never fat; while stall-fed animals are covered with fat. But when the latter are permitted to move freely in the air, or compelled to draw heavy loads, that is to consume more oxygen, the fat again disappears. It is hence evident that the formation of fat is the result of a want of due proportion between the food swallowed and the oxygen absorbed by the lungs and skin. A pig when fed with highly azotized food increases in flesh, but when fed with potatoes it becomes fat. From these and other facts it appears logical to infer that fat is derived from vegetable food. If we compare the composition of sugar of milk with starch, sugar, mutton, beef, and human

fat, we find that in all of them the proportion of carbon to hydrogen is the same; that is, 45 to 6, and that they only differ in that of oxygen. From which it follows, that by the mere separation of oxygen, it is possible that sugar, starch, and gum, may pass into fat. The following formulæ exhibit distinctly the differences between fat and starch:

Starch......	C_{12}	H_{10}	O_{10}
Fat	C_{11}	H_{10}	O

The only distinction therefore is in the absence from the latter of one atom carbonic acid (CO_2) and 7 atoms oxygen. If further we remove from 3 equivalents of milk sugar, 4 equivalents of water, and 31 of oxygen, we have remaining a formula which exactly represents the composition of cholesterine, the fat of the bile. But from whatever source fat may be formed, it is certain that its production can only take place in one way, viz., by a separation of oxygen from the elements of the food. The formation therefore of fat depends on a deficiency of oxygen, but in the production of fat a new source of oxygen is developed, *a new cause of animal heat.* The oxygen evolved when the fat is deposited must combine with carbon or hydrogen from some other source, and be discharged in the condition of carbonic acid or water, and this union must produce as much heat as if the carbon had been burned in oxygen gas. If we suppose that from 2 equivalents of starch 18 equivalents of oxygen are disengaged, and that these 18 equivalents combine with 9 of carbon from the bile, for example, then in this case as much heat must be developed as if the 9 equivalents had been directly burned. Such a phenomenon is analogous to that in the process of fermentation, when by the separation of the elements of sugar into carbonic acid and alcohol, as much heat is evolved as is sufficient to heat every pound of the fermenting liquid by 298°. In like manner in the formation of fat every pound of carbon which obtains the oxygen necessary to convert it into carbonic acid from substances which thereby pass into fat, must disengage as much heat as would raise the temperature of 200 lbs. of water by 70°, or from 32° to 102°. When animals are fattened on food destitute of azote, only certain parts of their structure increase in size; thus, in a goose, the liver enlarges and becomes soft and spongy from the deposition of fat in the cells of that organ. In some diseases the starch, sugar, &c. of the food, do not undergo the changes necessary to fit them for respiration, and consequently to be converted into fat. Thus, in diabetes, the starch is only converted into starch sugar, which is expelled without further change; and in diseases of the liver we find that organ loaded with fat and oil, probably derived from the maltransformation of the bile.

Food divisible into nutritive and respiratory. From the previous considerations Liebig infers, that human food consists of two kinds, azotized and non-azotized. The former is adapted to form blood—the latter cannot produce blood. *Nutritive* or *azotized* food consists of vegetable fibrin, vegetable albumen, and casein, animal flesh, and animal blood. In the *respiratory* or *non-azotized* food, are included,—fat, starch, gum, the different kinds of sugar, pectin, wine, beer, and spirits. Now it appears to be a fact well established by recent experiments, that the azotized constituents of food are identical in their composition with that of the solids of the blood; and no azotized body which differs in its constitution from fibrin, albumen, &c. so far as observation goes, is capable of sustaining life. This view of the subject of nutrition gives a most satisfactory explanation of the observations which have been more than once made, and which we recently in this Review took an opportunity of discussing (Brit. and For. Med. Rev., April, 1842—On Food) in reference to the inefficiency of gelatine for the purposes of nutrition. Dogs, when fed on gelatine alone, died from starvation. But when animal flesh was given in conjunction with gelatine, or when vegetable azotized substances were united with gelatine, dogs were sufficiently and properly nourished. It is rather remarkable that no such simple explanation as that presented by Liebig should have offered itself to Magendie, who was sacrificing the inferior animals in multitudes, to satisfy himself of the accuracy of a fact which had been previously settled beyond all question by several indi-

viduals, and particularly by Donne. Magendie did not observe the fact, or at least made no use of it, that when gelatine is devoured by dogs, although they are not nourished, yet this entirely disappears, while bone earth alone is found in the excrement. The same observation applies to man when fed on strong gelatinous soup,—not a trace of the gelatine can be detected either in the urine or fæces; it must, consequently, have been consumed for some ultimate purpose in the economy; but what that destination is does not appear so clearly; Liebig conceives it possible that gelatine in the dissolved state may again be converted into cellular tissue, membrane, and cartilage, and may thus serve for the reproduction of such parts of these tissues as have been consumed. In this way he explains the effect of animal jelly in invalids; the organic power by which the constituents of the blood are converted into cellular tissue and membranes, must of necessity be weakened by sickness, and under these circumstances gelatine dissolved, that is, in a form adapted for assimilation, may contribute to strengthen the vital power, as may be done in the case of the stomach by due preparation of general food.

Changes of the tissues. It is now satisfactorily ascertained that vegetables produce a substance, termed by chemists *protein,* which constitutes the basis of vegetable albumen; and it appears that out of the protein the various parts of the animal are produced by the vital power. Albumen is that form of protein which seems to constitute the matters from which we may deduce the resulting compounds. All azotized vegetable matters digested by animals must be converted into albumen before they can be endowed with nourishing power. This transformation is produced by chymification in the first instance, or a process which may be aptly enough compared to fermentation, inasmuch as both are changes produced in matter by the contact of another substance. By the action of muriatic acid upon the mucous coat of the stomach, a substance (pepsin) is formed which, by contact with the food, renders it soluble. This phenomenon is quite independent of the vital power, as is proved by the success which attends the experiment when made in vessels out of the body. Lactic acid does not appear to be generated on the healthy human stomach. In the action, therefore, of the fluid of the stomach on food, no other element appears to take a part except the oxygen of the atmosphere and the elements of water. Saliva affords an important supply of water, and also of oxygen, according to Liebig. No fluid possesses in such sufficiency the power of entangling air as saliva. Liebig considers that a large quantity of air gets access to the food on the stomach through the medium of the saliva; and there the oxygen combines with the food, and the nitrogen is given out by the skin and lungs. Rumination in the inferior animals may very possibly have for its object a renewed introduction of oxygen; physiologists have found that nitrogen is given out by the lungs in variable quantities; and Liebig accounts for this by the various proportions introduced into the stomach by the saliva. As a proof that gases can be absorbed into the system from the stomach, he quotes the examples of poisoning by feather white wine, where wine in a state of fermentation has its decomposing condition increased by the temperature of the stomach. The carbonic acid generated is absorbed and penetrates to the pulmonary air-cells, and the patient dies with all the symptoms of asphyxia; the best antidote has been found to be in these cases ammonia. Liebig has proposed some still more ingenious explanations of the source of the excretions. These he finds, by calculation from analyses, to contain all the elements of the blood. In order to form correct views on this subject, he has had the blood analyzed by his well-known plan; not the separate constituents, but a portion of the whole mass: the composition thus deduced he finds to correspond with half the formula of choleic acid (that is, the bile), 1 atom of uric acid, and 1 of ammonia, which is equivalent to saying that the blood ultimately appears in the excretions in the form of bile and urine. This important view is of the utmost consequence to the practice of medicine. In the higher classes of animals, uric acid disappears and is replaced by urea. This substitution obviously depends on the amount

of oxygen absorbed in respiration, and also the quantity of water consumed by different animals in a given time. When uric acid is acted on by oxygen, it is converted into alloxan and urea. A new supply of oxygen, acting on the alloxan, changes it either into oxalic acid and urea, into oxaluric acid, or into carbonic acid and urea. The mulberry or oxalate of lime calculi are usually found in those in whom, from want of exercise, the supply of oxygen has been deficient. Calculi, containing uric acid, or oxalic acid, are never found in consumptive patients; and it has been remarked in France that when patients, affected with uric acid calculi, are removed for exercise to the country, they become subject to the mulberry calculus. It is erroneous to suppose that the mode of cooking food can have any influence on these diseases. Flesh, in whatever way prepared, is at once converted into blood, while the uric acid and urea are derived from the transformed tissues. If this azotized food is properly supplied, and if an equivalent amount of oxygen is afforded to the system, none of these concretions can occur; in other words, if the liver and kidneys are capable of transforming the tissues which are continually undergoing change into urea, uric acid, and bile, none of these concretions should occur. Uric acid calculi have never been observed in carnivorous animals in the wild state; and it is believed that gravel and calculus occur in persons who use very little animal food. In reference to the bile, Liebig considers it to be derived from the decomposition of protein and starch; and he infers that, if the elements of protein and starch, oxygen and water being also present, undergo transformation together, and mutually affect each other, we procure, as the results of this transformation, urea, choleic acid (or bile), ammonia, and carbonic acid, and no other product besides these whatever. The metamorphosis of the compounds of protein present in the body is effected by means of the oxygen carried by the arterial blood, and of the elements of starch rendered soluble in the stomach, and carried to every part to enter into the newly-formed compounds. We have presented to us, the principal constituents of the animal excretions and secretions; carbonic acid, the excretion of the lungs,—urea and carbonate of ammonia,—expelled by the kidneys, and choleic acid separated by the liver.

Some of the speculations of Professor Liebig, relative to the action of vegetable alkalies on the system, are highly curious: all the azotized alkalies, with the exception of three or four, are poisonous. Caffein, the alkaloid of tea and coffee, is not poisonous; and this substance, by the addition of 9 atoms of water and 9 atoms of oxygen, may be resolved into 2 atoms of taurine, a substance procured from choleic acid (the bile) by the action of muriatic acid. This substance, like the medicinal alkaloids, may, from the similarity of its composition to the brain and nerves, combine with them and supply their waste. There is nothing so absurd in this as some might be inclined to infer; for we must remember that the animal organism has produced the brain and nerves out of compounds furnished by vegetables. If then we grant that the brain and nerves are formed from vegetable albumen, is it unreasonable to conclude that substances (vegetable alkalies), intermediate in composition between the fats and the compounds of protein, may be employed in the organism for the same purpose?

In the foregoing report, we have confined ourselves to a scanty analysis of some of the most important of the new views of Liebig. We do not affirm that all these views are demonstrably correct; but we believe that he has assumed the proper ground for physiological research; that in short all his speculations are in the proper direction. His book when it appears will present a rich mine of ideas, which we have no doubt will be purloined and diluted after various fashions. We trust that the present outline will preserve some of the property to its proper owner; and that those who build other works, by assuming some of his ideas, and surrounding them by a multitude of their own common-places will, unlike the plagiarists who have borrowed from his work on *agriculture*, have the honesty to acknowledge the source of their materials. At another opportunity, we shall present to our readers Liebig's views respecting the mechanical movements.

GIESSEN: *June* 1, 1842.

THE

BRITISH AND FOREIGN
MEDICAL REVIEW,

FOR OCTOBER, 1842.

PART FIRST.

Analytical and Critical Reviews.

ART. I.

1. *The Spas of Germany*. By A. B. GRANVILLE, M.D. Second Edition. —*London*, 1838. 8vo.
2. *The Spas of England*. By A. B. GRANVILLE, M.D. Three Volumes. —*London*, 1841. 8vo.
3. *Pilgrimages to the Spas*. By JAMES JOHNSON, M.D. — *London*, 1841. 8vo.
4. *The Mineral Springs of England*. By EDWIN LEE, Esq., Surgeon. —*London*, 1841. 12mo.
5. *Memoranda on France, Italy, and Germany*. By E. LEE, Esq. Surgeon.—*London*, 1841. 8vo.
6. *A Practical Treatise on the Efficacy of Mineral Waters in the Cure of Chronic Diseases*. By SIR A. M. DOWNIE, M.D.—*Frankfort*, 1841. 12mo.
7. *The Sanative Influence of Climate*. By SIR J. CLARK, Bart. Third Edition.—*London*, 1841.
8. *Rapport sur l'Emploi des Eaux Minérales de Vichy dans le traitement de la Goutte, lu à l'Académie de Médecine*. Par M. PATISSIER.—*Paris*, 1840.
9. *Rapport fait à l'Académie Royale de Médecine sur les Eaux Minérales de France pendant les années* 1834-5-6. Par M. F. V. MÉRAT, rapporteur.—*Paris*.
10. *Theoretisch-praktisches Handbuch der Heilquellenlehre*. Von AUGUST VETTER. Two Vols.—*Berlin*, 1838.
11. *Marienbad, et ses différents moyens curatifs dans les Maladies Chroniques*. By C. J. HEIDLER, M.D. Second Edition.—*Prague*, 1841.
12. *Ueber den Gebrauch der Mineralquellen, im besondere derer zu Ems*. Von Dr. J. VOGLER.—*Frankfort*, 1840.
13. *Kissingen ses Eaux Minérales et ses Bains*. Par F. A. BALLING, M.D.—*Frankfort*, 1839.

14. *Ueber den Kurort Ischl.* Von Dr. E. KUNDT.—*Vienna*, 1841.

15. *The Baths of Creuznach.* By CHARLES ENGELMANN, M.D.—*Heidelburg*, 1841. 8vo.

16. *A Description of the Mineral Springs of Aix-la-Chapelle and Borcette.* By L. WETZLAR, M.D.—*London*, 1842. 8vo.

17. *The Spas of Homburg.* By Sir A. M. DOWNIE, M.D.—*London*, 1842. 12mo.

THE books whose titles we have transcribed, (and we could have added some dozens more,) and all of which relate to one subject—MINERAL WATERS AND BATHS—are of very various degrees of merit. Between the scientific treatise on the whole subject by the laborious German, who, with inexhaustible and untiring perseverance, seems to make himself master of all that every one has written on his favorite theme, for the purpose of laboriously framing it into a system; and the thin pamphlet of the superficial spa-doctor, who extols with indiscriminating and excessive laudation, and with the clearest and most barefaced intentions, the virtues of the spring from which he derives his own means of subsistence, there are numerous intermediate books of very varied worth and purpose. Amongst them are not a few goodly-sized, handsome volumes, whose smooth, white paper, ample margin, and clear type, wood and stone and copper illustrations, and curiously-figured, many-coloured calico bindings, are one of the proofs that many opulent people in this country are bent on the endeavour to regain health, or to replace bodily uneasiness with more agreeable sensations, by change of climate, travelling, and mineral waters, instead of trusting exclusively to the ordinary medical treatment to which the majority are, with various success, most commonly submitted for the same purposes. As there is an increasing disposition, among those who are able to follow their own inclinations, to seek health by such remedies, these books are obviously well suited to the tastes and habits of society.

I. An author of the seventeenth century writes:

> "Oh England! full of sin, but most of sloth,
> Spit out thy phlegm!"

Whatever our faults may now be, sloth is certainly not one: we have exchanged it at least for some others probably of the most opposite kind. Our natural characteristic is rather intense activity of the intellect, with corresponding energy in action, leading men to strive, at schools, for prizes; at colleges, for exhibitions, fellowships, first classes; in political life, for place and power; in professional life, for distinguished station, founding families, great wealth; in commerce and in trade, to outstrip rival nations and rival shopkeepers, and, if possible, to monopolize the commerce and manufactures of the whole town, county, province, or the world. The Americans include all this in the strong, unmistakable expression, "going ahead;" and as a nation they illustrate much of the mingled good and evil belonging to this state of things: where life seems a race in which those who do not ever keep up with the rest, are run over by the hurrying, precipitating crowd, and are crushed. Our own profession strongly indicates this state.

Physiologically speaking, it is by the active intellect, by the mind—

or one part of it, rather—acting through the nervous system, that this general condition of things is worked out. The supremacy of the muscular system has long passed. "It is not (now) the thews and sinews of a man, but the spirit." In the patriarchal times, with the Greeks, the Romans, and with those they named barbarians, and amongst all whom we now call savages, with the barons of the middle ages, as well as their retainers, every man (except the inspired man—the poet and the priest) was, until the pen superseded the sword, greatly estimated according to his bodily strength, his mere animal man. But gunpowder, printing, and steam have strangely changed this. Muscular strength is alone estimated as so much mere machinery, and valuable only in the "hewers of wood and drawers of water," as worth so many pence a day. The men of strength are the men of intelligence, not the men of muscle. The acute, pallid, meager driver who directs the railroad engine whilst rushing on at its appalling speed, by a slight handle that can be moved by a child, is a type of this curious change. The pleasures of the day are similarly modified. Simple, unexciting enjoyments belong to a different state of society. "The child is father to the man," the early striving for rewards at schools is the first edition of the work of life—of the subsequent real struggle when the business of living begins: and this country is so thickly peopled, and sharpened, acute, practical heads are so numerous, that it is not to be wondered at, when the prizes are much greater and the combatants much more agile, that the contest should be severe. The man whose mind is always active in business requires stimulating pleasures. He can no longer be "pleased with a rattle, tickled with a straw;" but (like the Anglo-Indian who has been so stimulated by a tropical sun, that he cannot eat his rice and chicken without curry) his enjoyments must be highly seasoned, piquant, exciting. Not to speak of the more sensual animal excitements, such a man gets in his newspaper (full of those topics which are moving the whole world) a daily dose of the highest kind of stimulus; and in the succession of weekly, monthly, and quarterly journals, (which embody much of the literary ability of the day,) the more intellectual man finds the gin and beer of his jaded brain. To great and increasing numbers, politics are the condiments of their daily bread:—the news-room, the club, the liberal or the conservative association, holding their stated pothouse meetings, the inn-bar, a beershop bench, the town council, or the county townhall, are all arenas in which the affairs of the nation are discussed and settled with greater violence and much stronger personal feeling than by the chief actors themselves. The pleasures depending on the muscular system are comparatively few, and are still decreasing—practised almost exclusively by a few of the highest and, as extremes meet, of the lowest orders of society. By the former, hunting, shooting, and other field-sports are carried on with great perseverance: the same ardour which in the middle ages would have sent the same man to fight the Saracens in the Holy Land, to win the honours of ladies in tournaments or in perilous adventures abroad, to defend his castle against another chieftain, or to make vindictive or predatory excursions in the neighbouring domains of his foe, is now expended in carrying him over five-barred gates, through or over thick hedges, across forests, bogs, fields, on fleet horses, in pursuit of foxes or deer. Instead of books of chivalry, in which the adventures of armed knights in distant lands, and

their combats, and various perils in pursuit of some imaginative idea are depicted, we have books of sports describing the leaps of the noble huntsman; the head of game slaughtered by the peer; the privations and hardships the patrician deer-stalker undergoes in wading streams, and creeping along the heather upon his belly, and watching for days his prey; or the arms and accoutrements, the boots and the coats with which some gallant colonel provides himself whilst waiting night after night in the depths of winter for the flocks of wild ducks which are to be the victims of his pleasure.

Amongst the poor there are a few muscular delights remaining: cricket, hockey, quoits, skittles, make up the poor catalogue of their out-of-door amusements. And by the multitudes whose bodies are fatigued with overwork, and whose intellects have had no opportunities for becoming expanded, is it surprising that the quiet luxuries of gin, beer, and tobacco should be so highly estimated? But the great bulk of the people—the middle classes, in its most extended sense, (including almost all who labour for their bread, except as servants, labourers, and mechanics,)—have no amusements depending on muscular exertion. In Scotland alone does it seem to be thought consistent with the gravity of years, with "respectability," with learning, for any one of this class to use their muscles for any other purpose than that of riding or walking. An Englishman sees with surprise on the ice of Duddington Loch, the principal of the university, the professor of moral philosophy, with perhaps a poet, an historian, a physician, a divine, silver or grizzle-haired men, employed with juvenile earnestness in rolling blocks of stone great distances, and with great art and strength ("curling," it is called:) an amusement which in his own division of Britain would appertain alone to boys or unoccupied young men. The Scotch, however, are in this respect much wiser, and, to a far greater extent than the English, combine active amusements with the strict performance of the more sedentary duties of life.

It cannot be denied that the chief pleasures of the day are of a similar character to its duties—mental, not corporeal. And as the brain and nervous system are the material organs through which all their objects are accomplished, it is not surprising that they suffer from over exhaustion, whether from over work or excessive pleasure. One of the most certain results of this activity being wealth, the luxury which is its attendant becomes in its turn a fruitful source of disease. The sensual pleasures which luxury supplies are often most keenly enjoyed by those whose nervous systems are stimulated by incessant exertion, and it is precisely to such persons that they are the most injurious. He who takes active exercise in the open air carries off many of the effects of a diet which under other circumstances would load his body with superfluous nourishment or stimulate him to excess; but the man who is engaged in a sedentary occupation, especially one requiring exercise of the active powers of the mind, eats and drinks too much with far less impunity: he is gradually and, to himself, imperceptibly sowing disease, and does not discover his mistake until his health is seriously impaired, and his habits become so fixed that often he cannot break through them. What has been said in a moral sense is equally applicable in a dietetic one:

> " At thirty man suspects himself a fool—
> Knows it at forty, and reforms his plan;
> At fifty chides his infamous delay,
> Resolves, and re-resolves, and dies the same."

The candle cannot be burned at both ends without double-quick consumption. The expenditure of nervous power required in active business—professional, political, commercial, or literary—renders the body less able to bear the labours of digestion, and to stand the depression which invariably follows the stimulus of too-exciting food and drinks. "Men dig their graves with their teeth" all the faster when, in addition to full diet, they neglect exercise by overtasking their brains. The consequence of such neglect of obvious rules is twofold. On the one hand, confirmed indigestion, and on the other, every form of nervous disease, both primarily arising from all those causes which exhaust directly nervous power, and secondarily from this exhausted nervous system sympathizing with an impaired digestion. The stomach, which even in the healthy state is the centre of sympathies, is now evidently more susceptible; so that whatever irritates it irritates the brain, and vice versâ. The stomach, which has been well but quaintly called "the conscience of a man's body," is now, like a conscience ill at ease, his torment. Our bodies being so framed that whatsoever they have been long accustomed to do or to bear, whatsoever has long acted upon them, either from without or from within, becomes almost necessary to them, it follows that our remedies in disease will be modified and changed in some degree according to the general habits and modes of life of the time. The epicure, the gourmand, the mere man of pleasure, the over-stimulated over-cultivated woman, the anxious care-worn professional man, the toiling, striving, struggling man of business, sooner or later feels that some "screw is loose," that the body has been too long trifled with, and that what is sown is about to be reaped. Whether it is called indigestion, liver-disease, hypochondriasis, abdominal congestion, exhausted nervous power, chronic rheumatism or gout, whatever local habitation the disease may have taken, or whatever theory may be adopted to explain it, the causes have been much the same—a neglect of organic laws of long continuance, daily irregularities in small omissions or commissions, bringing the body into such a condition that some more active, more apparent, and greater cause brings on disease. The patient himself is well aware that he has passed the Rubicon which separates him from health: he is conscious from his own inner sensations that he has an organized system, that he has a stomach, a head, or a heart, and that those vital functions which, like growth and life, had before been performed silently, unconsciously, and unerringly, go on so no longer. Perhaps the patient eats and drinks and looks well; it may be he is stout, portly, and fresh-coloured, and is often in fair spirits; and friends (healthy people, ignorant of what illness is) talk of imagination, fancy, nervousness, as if this theoretical explanation, this giving a thing a name, got over the difficulty. For what, after all, is more real than imagination, what more difficult to cure than its diseases, and what diseases (every madhouse testifies) more extremely obscure? In some cases the digestive organs are manifestly deranged; in others there are obscure symptoms of gout; in some deranged cerebral circulation; in some

hysteria, convulsions, spasms, local pains, depressed spirits;—in all the patient is out of condition, not perhaps confined to his room, but uncomfortable in his feelings, and either quite unable to perform as he was wont the duties of life or only able to get through them with difficulty; or, if there are no urgent calls to exertion, is in a fretful, complaining condition.

To numbers who are in this state of health, victims, and, to certain extent, self-sacrificed victims of a highly-artificial and unnatural state of society, such means of cure as foreign and domestic SPAS, with the travelling and change they demand, offer a chance of relief which is eagerly sought after. Apart from the acknowledged virtue of mineral waters, and the advantages and pleasures which attend their use and increase their beneficial effects on the body, these spas are singularly well adapted to the wants, habits, and tastes of those who frequent them. For whilst the water acts medicinally on the body, producing healthiness and cleanliness of the skin, and giving moderate assistance to the secretions, and the change supplies the lungs with pure, fresher, and newer air, the watering-place at the same time affords EMPLOYMENT to the mind and body, and prevents or diminishes those sufferings from *ennui* which are almost certain to accompany the indispositions of those who have been used to an active life either of business or of pleasure. On this account also they are such agreeable remedies, so much sought after, for they supply a succession of stimuli of a healthy kind to those to whom from habit constant stimulus has become almost a necessity. In travelling, particularly in a foreign country, the mind is pleasantly excited to observation and reflection by a strange people, a new country, different styles of architecture, by works of art, or the beauties and sublimities of nature, by the novelties of manners, customs, costumes so widely varying from those to which it has been accustomed; the diet, the wines, the dishes, the hours are new, affording to the digestive organs a succession of new stimuli; and the air, instead of traversing the ocean, comes probably fresh from sweeping over vast continents, changing its character and its effects on the body exposed to its influence. To the jaded man of pleasure, of business, or of intellect, a remedy which substitutes healthy excitements for the morbid ones to which he has been accustomed, must have great charms. At these mineral spas or watering-places constant employments are supplied him; his time is regularly divided, so that it slips away rapidly; and what with exercise to which he is compelled by the various daily water-drinkings and bathings at a distance from his hotel, his public meals, the interest in new faces and new manners, together with the various amusements which are at his disposal—all consistent with early and regular hours—he is, although an idle man (or woman), yet still a somewhat busy one. In such places in England, of course there is less stimulus to the mind; none of the pleasant varieties of foreign faces, costumes, cities; but still there is a similar effort to furnish employment and to turn remedies, as far as possible, into amusements. People are beguiled into exercise under the plea of promenading; they are compelled by fashion to rise early and to go early to bed; and in gay pump-rooms surrounded with all kinds of pretty decorations, do hosts of fashionable men and women daily for weeks with uncomplaining patience and unfailing assiduity, drink large tumblers of very nau-

seous water—doing in flocks what they would neglect to do alone, still chained by their habits, induced by fashion and companionship to take neutral salts, gregarious even in their diseases. Perhaps a fashionable physician is one of the attractions of the place; and it would probably be found that the assiduous way in which he employs his patients' time is an element in his success; mapping out for them their day's work, compelling them to live rationally and to take much exercise in the open air, and by water-drinking, walking, riding, driving, shower-bathing, and early rising, to fill up their time and drive away ennui. For the *dolce far niente* may be possible to an enervated Italian in his own luxurious climate, but its synonym is the Roman poet's "atra cura" to a people reared under colder and more changeable skies, to a life of active employments, or to one of as strenuous pleasures. No reader of Celsus but must be struck with the number of things which he gave his patients *to do*. It was no doubt for the wealthy and luxurious that he wrote, and the "gestatio in rheda," and the "clara lectio," and the hot and cold and vapour bathing, protracted and interchanged, must have taken up a good deal of time. So much bathing also with all its loiterings in the "sudarium" and other apartments of the bath, must have stood in the place, relatively to the skin, of a good deal of exercise, besides giving the invalid patrician occupation.

II. The medical evidence of the value of mineral waters in the cure of diseases is abundant even to profuseness in quantity, but its quality is indifferent. Indeed here, as in medical evidence generally, the quantity is in inverse ratio of the quality; for where little is known very much is usually said and written about it, and those diseases which are most obscure are most voluminously descanted upon. The books before us may be comprehended in three classes: the productions of casual medical visitors; the essays of the resident physicians; and learned and laborious systematic treatises.

1. The first class of writers are the offspring of the last few years, physicians who write for the general reader, and who combine with much popular medical matter more amusing details interesting generally. Such a writer in the few weeks in which he can escape from his private practice in London or elsewhere, instead of rusticating with his wife and family, as the more usual plan is, combines pleasure and business; with note-book and such writing gear crosses the channel and, by means of railroads, steam-boats, and the most rapid horse vehicles he can command, visits with British haste and activity every spa he can reach from Belgium to Bohemia, from the valleys of Nassau to the Swiss or the Noric Alps, and returns laden with his materials for a book. In this short time he has accomplished much: he has travelled rapidly many hundreds of miles (the exact number is usually told), he has studied all the natural scenery—rocks, woods, cataracts; "meadow, grove, and stream" have so fixed themselves in his memory as to enable him on his return to reproduce them with dioramic truth. He has examined with the eye of a connoisseur all works of art. Statues, pictures, cathedrals, monuments, ruined castles, battlemented towers, he has so justly valued as to decide on the exact place they are to hold in the estimation of mankind. As "the proper study of mankind is man," he has noted in his rapid course the physiognomies, manners, habits of the numerous coun-

tries he has passed through, and has been enabled to draw from his observations correct inferences as to the general character—moral as well as intellectual—of the various people he has seen, and to explain on the best principles of philosophy or political economy (as the case may be) the exact influence of laws, government, liberty or despotism; hot or cold weather, clear or foggy air, damp or dry seasons, in producing just the kind of people, and no other, as are found there. Or he may affect a somewhat lower range: he may examine the dress and manners of the "aristocratic," (a favorite term, although now so generally exploded, of one of these writers "qui stupet in titulis,") and decide on the exact scale of eminence which the spa holds by the visitors who frequent it, whose rank and dignity, disguised though it may be by green-sickness, gravel, gout, skin-disease, scrofula, or sciatica, is as clear as the day to his practised vision. Indeed, did he not appear so anxious to prove it, we should believe the travelling doctor was actually one of this favoured class, so entirely intimate does he seem with an "illustrious foreign prince," with "the Duke of This" and the "Marquis of That," with "amiable and most interesting noble ladies." At each medicinal bath and spring our informant, with a thirst for knowledge worthy of all praise, drinks the required quantity of fluid, whether impregnated with foul sulphuretted hydrogen, or holding in solution purgative salts, or fortified with iron; of all and of each—for the same watering-place may have waters of all kinds—he quaffs tumbler after tumbler, (with the quarter of an hour interval,) and then, with careful, discriminating self-examination, minutely registers in his diary the exhiliration and depression, the headach or intoxication, the purging, sweating, or diuretic flow which results from this self-sacrifice. He submits his body as well as his stomach for the good of science; and after soaking his fabric in every bath—hot, tepid, cold, in impure water, and in very filthy mud—he good humouredly and most enthusiastically dwells upon the delicious smoothness of his skin, its marble whiteness, the exquisite titillations of the small bubbles of carbonic acid gas which "just effleuraient the surface of the body," as they rise and burst by millions through "the lucid, genially warm, and gently murmuring" waters! The effects on his mind, the psychological as well as the physiological peculiarities are commented on with equal care, precision, and accuracy. One gentleman lying down in a tepid bath describes his feelings thus—"It is the human tempest lulled into all the delicious playings of the ocean's after-waves;" and we are told of water baths producing "the ecstatic state of a devotee blended with the repose of an opium eater;" passages of such poetic beauty and truthfulness as could have alone originated in some very peculiar source of inspiration, as if the "sacred brook of Helicon" were an aerated chalybeate water.

But besides this personal experience the observer introduces himself to the physician of the place, and often many pages are the result. Or else he masters the various pamphlets which their resident physicians have written and he prints the analysis and his own comments. The whole work is much enlivened by piquant anecdotes, personal remarks, quotations, classical allusions, French slang and phrases, and often the minute and highly-interesting details of the journey. Quarrels with rail-road clerks, ire at over-charged luggage, heartburn from over-eating at

a cheap and well-furnished table d'hôte, invitations to evening parties, (reprinting the card,) give a life and reality to the journey, beguiling the reader to proceed, whilst also they afford an insight into the author's own mind, such as, were he not his own chronicler, we should have been ignorant and unsuspicious of. In one of the books of this class the author has been irritable and mightily irate with the editor and contributors to this Review; but as he, giving way to unreflecting impulse, expressed his momentary feelings of annoyance at the treatment he received at the Birmingham railway station, and in the next volume having grown cool, and seeing his error and his hasty conclusions, qualified his charges, so we will hope that in this instance time will also have made him wiser. At any rate, he cannot make us angry.

Among these popular works, the most remarkable is the small volume, by Sir Francis Head, which brought the spas of Germany into fashion. A man of genius is overworked, and sent by his physician to an insignificant spring in a small German principality; like any other traveller he wraps his cloak round him on the deck of a steam-boat, crosses the channel, ascends the Rhine, takes up his quarters at the Spa boarding-house, eats his meals at the tâble d'hote, bathes, and drinks the waters, talks with the bathman, and observes the visitors, wanders through the adjoining woods, climbs the hills, watches the swine, goes to church, and like the common herd of travellers he passes on and is unobserved; but not unobservant. Nothing has escaped his quiet eye: he has noted it all in his memory, and has found food for fresh and original thought in the commonest every-day occurrence. He returns, and writes a small volume, gives it a quaint title, ironically calling it a *Bubble*. Every one reads it; it is the theme of common talk, of universal praise. Every one has been in Nassau with the writer, has admired the fine English lads in the steam-boat, and has felt shame at the contrary impression the dandy gives of the national character. The baths, the snakes, the forked street of Swalbach, the swine, evening coming on, and the village suddenly lighted up, the solemn sermon and the serious hearers, are scenes fixed in all our minds by a power which by means of mere words, places before us pictures as vivid and real, and more exacting than a painter's. And a mere sportive *jeu d'esprit*, in which it has pleased the writer "ridentem dicere verum," sends hundreds, nay thousands, to an out-of-the-way village in Germany, to compare their own impressions with his descriptions, to see those things which without his aid they could have never seen, or like him to endeavour to renew their youth. And the dull and prosaic go there—and see nothing, and in vapid phrases accuse the "old man" of exaggerations and misstatements, priding themselves on their own foot-rule accuracy, ignorant perhaps that "the eye only sees what it brings with it, the faculty of seeing," forgetful how "different was the effect of the same objects on the retina of Newton, and Newton's dog Dash."

When such a vein has been opened, the workmen who press forward, endeavouring to quarry in the same direction are not few. Dr. Granville visited all the spas of Germany, and by a work written for the general reader, but containing much that interests the medical man, increased the common interest excited by the subject. Uniting the information of a book of travels, a "Hand-book of Germany," with much medical mat-

ter as to the chemical nature of the springs, and their curative action collected from books, conversation, and personal inspection, Dr. Granville's work has been serviceable in attracting the attention of the public to the valuable mineral waters to be found in that part of Europe.

"Pilgrimages to the Spas," by Dr. Johnson, followed Dr. Granville's two volumes. It is written with the same two-fold object, being both a book of travels and a book of science; the medical part, however, being much less adapted for the general reader than that of Dr. Granville's book, as it consists principally of brief and condensed analyses of the essays written on the springs that were visited, with critical comments. The work is characterized by that activity of mind, disposition to reflection, great industry, power of compressing the thoughts of others into a short compass, judicious practical knowledge of medicine, love of quotation of verses, and partiality to Apollo, Cupid, Minerva, Morpheus, Bacchus and other heathen divinities, which mark the productions of the same untiring, never-failing pen.

Mr. Lee, who resided some time at Wiesbaden, and has visited most of the German spas, has published a smaller work upon them, marked by good sense, reflection, and an acquaintance both with the action of these waters and what has been written upon them.

The favour with which the public received the "Spas of Germany" induced Dr. Granville to visit rapidly the spas of England, and to write three volumes about them. They are even more adapted to general readers than his former work; a table at the end of each volume, containing the analyses of the various springs, is the part which chiefly interests the medical reader.

2. The second class of books are the productions of the physicians resident at the springs. When Touchstone excused his love for the homely wench he had married, with "A poor thing, sir, but mine own," he uttered an aphorism embodying one of the causes which makes us doubt in admitting without hesitation the evidence of all very interested parties. It is the expression of that tendency of the human mind which leads the individual (for very wise purposes) to put an infinitely higher value on whatever belongs to himself, and that makes him attach so much more importance to the one subject on which his thoughts are occupied, than comparatively it deserves. Added to this actual bias the spa-physician has also his own temporal interests so closely bound up with the number of visitors who may seek benefit under his direction at these sources of health that his judgment may be somewhat influenced. Indeed, there seems to be something of that sort of anxiety in many of these gentlemen to include as many diseases as possible in their list of curable ones, as advertising quacks evince in newspapers and pamphlets to set forth the unity if not of disease, at least, the singleness of the remedy. On these accounts, books in general on mineral springs are among the most unsatisfactory, puzzling, and confusing of all medical or pseudo-medical productions. Infinitely tedious are the lists of diseases, the tape-worm prolixity with which they are discussed, the theoretical explanations of their action, and the discordant testimonies of each observer. But besides these empirical puffs, there are essays written by men of science and character, the books themselves testifying to the sense, judgment, and candour of the writer; and as these alone supply us with valuable practical matter, we shall chiefly use them.

Amongst these monographs, one of high reputation is the classic work of Marcard, on the waters of Pyrmont, written about 1780. He was a physician of Hanover, a severe sufferer himself from a nervous disease, which was cured at Pyrmont, but was again brought on by writing his book. The diligence with which of late years pathological anatomy has been studied by directing attention exclusively to changes in individual organs and structures, has led to the comparative neglect of those general conditions of the whole frame on which some of these local disorders depend, and on a knowledge of which is often founded a judicious plan of treatment. The study of the symptoms of general debility of the whole body; of weakness of the solids; of a depraved state of the fluids, showing itself constantly in eruptions, deposits in the urine, &c., of morbid irritability of the whole nervous system as testified by increased mobility, and sensibility, and mental sensitiveness; of accumulations of blood in the vessels of the abdomen, &c., are some of those circumstances which, although recognized by men who are successful practitioners, are insufficiently treated of in systematic and elementary books, but form the subject of many of the chapters of M. Marcard's volumes. In a main point these volumes differ from the whole class. The Pyrmont water is a strongly aerated chalybeate, and (as Marcard has written on the treatment of chronic diseases in general,) instead of showing how many may be beneficially subjected to this tonic and exciting treatment, he has shown, on the contrary, in how few cases tonics and strengthening remedies can be at first employed with safety, although there are many to be benefited by such means after having been subjected to a cooling, aperient, or alterative course. Marcard being himself an invalid, a practical physician, and one well acquainted with prevalent medical opinions, seems to have embodied the general sense of the ablest men of his day, confirmed by his own observation; and the same views have descended with some modifications, and are to be traced in the various volumes and pamphlets, and even in the showy semi-medical guide and tour books now so abundant. For those who are at all acquainted with the literary history of any subject, medical or general, well know that some single observer and thinker has furnished the staple matter that has been diluted, mangled, and deformed, or that has been illustrated and expanded by a host of succeeding scribes who have supplied books on the same subject. It was no small pleasure to discover among such as these a treatise of Dr. Heidler on the waters of Marienbad, a really practical book, full of judicious observations, and of the results of the careful study of the action of these waters on a large number of persons affected with different forms of disease.

Dr. Heidler is one of the best known resident physicians of any of the German spas, and is highly esteemed for his sound sense by all who are acquainted with him. His industry is untiring; he is frequently bringing out new works on this one subject, and his attention to it, his enthusiasm, and his common sense are conspicuous in all of them. That he may be somewhat partial to his own spa is an inevitable consequence in one who has resided at it a large portion of his life, and has watched its increase from an inconsiderable village to one of the most renowned watering places of Bohemia. With a due allowance for this bias we shall make much use of his work, as owing to the springs of Marienbad being

of various kinds—aperients, alteratives, and chalybeates, Dr. Heidler's experience is very diversified; and as the two great varieties of waters, the saline and the iron, can be obtained at his spa, he is less exclusively wedded to either, and his results are applicable to waters of other spas which belong to the same classes.

The report on the waters of Vichy, made by the Royal Academy of Medicine of Paris, furnishes valuable information as to the action of hot alkaline waters on gout; and M. Bertrand's work on the waters of Mont d'Or in France affords materials for estimating the influence of the same remedy in chronic bronchitis.

Sir A. M. Downie, M.D. has written two little books. The first is to recommend Wiesbaden, and the second Homburg, a new spa. The plan of the first is good, and it is throughout sensible and judicious, but contains nothing very new. There is such a strange forgetfulness of opinions that our faith in the first was much shaken by reading the second. In the first, published in 1841, a description is given of the constitutional disturbance which is produced by the writers in much the same language as the native physicians employ; but instead of calling it a crisis, he prefers the term "point of saturation." The fact, however, is admitted that fever, discomfort, eruptions, and piles are not unfrequently produced by mineral waters after they have been taken for some little time, and that after these symptoms subside, "the full benefit of the course will be felt." In the second pamphlet, dated 1842, the same writer says, "during eight years that I have practised at the German spas, I have not seen one case of what is called crisis or 'bad sturm,' but admits he has seen symptoms of discomfort produced by imprudence in diet, exposure, and such causes during the course, or when the water has disagreed. Dr. James Johnson has the merit of converting our author. Between the publication of the two books, it seems that Dr. Downie had read Dr. Johnson's work, who theoretically imagines that a purge given before the course would obviate the disturbance which is thought to be "critical" and salutary.

The monograhs of Dr. Engelmann on the baths of Creuznach; of Dr. E. Kundt on Ischl, and of Dr. Wetzlar on the springs of Aix-la-Chapelle, have afforded us valuable information on these subjects. The latter book was needed; and will be found very useful to the visitors of the spa where the author so judiciously exercises his talents.

3. The third class are systematic works embracing the whole subject, in a chemical, theoretical, and practical point of view—compilations from the works of all other writers. Some of these are essential to those who thoroughly study the whole subject, as they are compiled with care, minuteness, and German diligence, by thoughtful, judicious men, who have themselves watched the effect of waters on disease.

Our object in this article is, to glean from these books the practical matter they contain, rather than to criticise very curiously their merits. The spas, particularly of Germany, are of so much interest at the present time, and the medical man is so frequently appealed to for information on their merits, and knows so little about them, that we offer no apology to those of our readers who may have more deeply studied their chemical history, for endeavouring, in a clear, simple, and comprehensive manner, to treat the whole subject so that the reader who has neither the time, in-

clination, nor books to enable him to investigate for himself, may have a general view of the various spas most frequented, the characteristic peculiarities of their waters, and the diseases in which they have been found most serviceable.

III. It cannot be denied that much scepticism exists among medical practitioners in this country, as to the practical efficacy of mineral waters in the cure of disease, and that patients themselves often take at least the initiative step in determining to try their effects. Such a scepticism is likely to be encouraged by the exaggerated accounts of the efficacy of these agents in so many different diseases, and by the difficulty of arriving at the exact truth from this host of partial and interested witnesses; and the medical man who is thus prejudiced, is the less likely to believe in the share which the mineral waters really may have in relieving his patients, when he thinks he can account for the improvement by the change of air and scene, freedom from cares of business or from the dissipations of pleasure, early hours, simple diet, and attention to the more obvious rules of health. But although these causes of improvement will explain much, and will sufficiently account for many cures, yet no one who will carefully read and reflect upon the chemical composition, and known action of those waters, (especially in Germany,) which are most active, but must admit that they are powerful agents capable of producing a decided impression on the secretions, excretions, vital and morbid actions of the body. The scepticism in this country must have been fostered by the fact that many of our waters whose virtues were most lauded and run after, differed very little from common water, and yet to their use was attributed most marvellous cures. Thus the hot wells of Clifton, the springs of Malvern, Matlock and Buxton, were found on chemical examination to be remarkably free from any solid matter whatever, and to be only very pure water rather warmer (except Malvern, which is cold,) than common water. And yet they were to cure consumption, scrofula, rheumatism and gout. A dislike, amounting to contempt of the marvellous, is a characteristic of the practical British mind; and in the case of these mineral waters the mere fact of the rich and the titled flocking to them, rather prejudiced the medical observer against them, as he knew it was that class who were the most easily seduced by anything new or wonderful. Their decline in public regard favoured the same unbelieving disposition. But on the other hand, Cheltenham, Leamington, Harrowgate in England, and more particularly many of the German waters, hold in solution such decided doses of medicines we are daily in the habit of prescribing, and their action upon the secretions is so palpable, that even the most sceptical must waver. Some contain active aperient neutral salts in such quantities as to act immediately on the bowels or kidneys; or alkalies which are sufficient to keep the fluids of the body in a constant state of alkalization: or such a combination of salts in small quantities, and gas, as to act immediately upon the kidneys or skin; or they are so highly impregnated with gases, such as carbonic acid, or sulphuretted hydrogen, whose action is felt by any one who will submit his body to the test; or they hold in solution the most satisfactory mineral tonic which we possess (iron), and in that condition, the state of carbonate, which all admit to be its most active form, and one which is retained in any medicinal preparation with

extreme difficulty. Besides, many of the springs are hot, and the action of heat is no fanciful, hypothetical notion, but is one of the most obvious stimulants we possess. Its effects upon the cutaneous capillaries are marked; stimulating the minute vessels, and filling them with red blood, and thus tending to equalize the circulation, and to relieve internal conjestions, or irregular distributions of blood. The action of hot-water baths in producing perspiration need not be insisted on; those only who have seen the effects of the long-continued use of hot baths, even of the simplest volcanic waters, pursued *ad sudorem*, in reducing corpulency, are perhaps fully aware of the amount of visible power they possess. In addition to this effect, the body immersed in a hot bath at the time absorbs a considerable quantity of the fluid; thus a single bath of the hot waters of Vichy, which contain a considerable portion of carbonate of soda and carbonic acid, renders the fluids of the body alkaline; even of gouty persons whose fluids were previously acid. When therefore, in addition to the heat of these waters, we find they are impregnated with various active neutral salts, alkalies, iron, carbonic or sulphuretted hydrogen gases, &c., and they are applied daily and for many weeks to the whole surface of the body, which is capable of absorbing some of them, their therapeutic power must appear still more credible. It should also be recollected that these active medicinal agents are mixed in such proportion, that they act as aperients, diuretics, or tonics, (when they are suitable,) without any pain, uneasiness, or other discomfort, and that they are taken perseveringly, daily, for one or two months; the patient thus giving himself that chance of benefit from a remedy which he will rarely submit to when undergoing a course of medicine, for such a chronic complaint as does not entirely compel him to submission. How few patients, for instance, with any form of indigestion, or with chronic rheumatic pains, will second his medical adviser in the same way as he would do at a German spa? where he willingly assists the curative action of the medicinal water, by following the prescribed regimen of diet, habits, and hours, and giving up everything at the time for the sake of gaining health.

To doubt the power of mineral waters because they are not alone relied on, but diet, regular and early hours, exercise, &c., insisted upon in conjunction with them, is unwise, because in the cure of chronic diseases, (even by medicine,) the minute attention to these causes influencing the general health is essential. The conjunction of suitable medicines with hygiènic rules is the best treatment, but if either of them be adopted singly, medicine had better be altogether thrown aside, and care be alone paid to those agents, (air, food, clothing, habits, exercise, and cleanliness,) which are constantly acting upon the body. Such a doubt therefore can never be solved, is not indeed capable of solution, as both of these agents must act in conjunction in order that each may produce its due effect.

We are perhaps too apt to confound in our mind our own suppositions as to the action of the chemical ingredients which are found in mineral waters, and the actual and visible effects they produce on the body; and as we cannot explain an action, overlook the fact that it does really take place. But it is surely unjust to disbelieve evidence, that a water which contains little besides carbonic acid gas and a minute quantity of iron acts on the kidneys, because its chemical ingredients are not in our

estimation diuretics. It is very fallacious to attempt to estimate the action of medicines on the body by reasoning prior to experience, however logical and clear the chain of reasoning may be. Occasionally analogy favours us, but the safest plan is to begin by experience, and then to reflect and reason on the facts which experience has furnished. Thus the virtue of some of our best remedies has been discovered either accidentally, or the medicine has been brought into use because the natives of the country (perhaps savages, villagers, unlettered and often unreasoning people,) have been long in the habit of employing them for the same complaints. Indeed, it is by far a safer way to seek information as to the virtue of any new remedy from the local tradition of the spot, than to apply it even according to analogy, which is the kind of reasoning that is most safe. By local tradition among the poorer class have the virtues of most springs been first discovered. It has been found that the people in the neighbourhood of springs have been well aware of the kind of diseases which were likely to be relieved. It is surely unreasonable to disbelieve this kind of experience, particularly when confirmed by the more educated and scientific who have subsequently confirmed it, merely because chemical analysis does not discover any active ingredient according to our notions to account for the effect satisfactorily; and particularly so when it is admitted that chemical analysis, though brought to such comparative perfection, is unable to detect the principles which produce contagious or epidemic diseases. If chemistry fail to detect the poison which in marshes produces ague, and in prisons typhus fever, in certain districts plague and yellow fever—poisons which we know to produce the same powerful effects in every human body exposed to them under certain circumstances, depressing the powers of life, setting up diseased actions, perhaps in a short time rendering diseased all our fluids, and often at once destroying life—we cannot wonder that it might also fail in discovering agents which in combination with water are capable of producing very salutary changes upon the body. We must go to experience first: we may then reason. When men who plume themselves on being practical men are prejudiced, they are the most confirmed theorists we know: they have been unused to reason, and when they attempt it they show themselves entirely incapable of estimating the weight of probable evidence—that evidence with which medicine has to do. The moment a mere practical man (so called) gets out of his narrow circle of individual observation he is lost; and such are those who are most disposed to doubt the efficacy of mineral waters: for here they leave experience, they have never had experience in their use; and they reason *à priori* from the effect of their own prescriptions; and as these waters do not contain the same doses, they doubt their power. The best chemists of France, celebrated as they are, have admitted that analysis has not enabled them to discover in many of the waters, whose effects on the body are well marked, the active principle; but instead of doubting the truth of these effects on the body, they have concluded that the failure only proved there were many bodies which escaped their means of examination. Chaptal said frequently and forcibly, that in the laboratory of the chemist, the dead body of the water was alone acted upon.* The

* Also Vauquelin, Annal. de Chimie et de Physique, xxviii., 105, and Robiquet, Journ. Pharm. 1835, p. 183.

commission appointed (under the direction of government) by the Académie Royale de Médecine, to report on the mineral waters of France, in 1836, declare their conviction that chemical analysis has not yet added in any considerable degree to that knowledge of the therapeutical effects of mineral waters which the experience of their effects had proved before chemistry was brought to their elucidation;* and they insist on the importance of taking into consideration the information which the inhabitants of the spot can furnish, "car il est remarquable combien les habitans sont instruits des véritables propriétés des sources qui les avoisinent, à tel point que parfois cette science populaire en dit plus que les livres à leur sujet."

We have said before that we must distinguish between our own notions as to the action on the body of the chemical ingredients, and the actual and visible effects which these waters possess. Now, although in many of these waters chemical analysis cannot detect the active principle, yet they do actually produce upon the body a visible effect: for instance, all the natural hot waters must produce perspiration; they must be absorbed; and they almost invariably act upon the kidneys: and therefore although their chemical composition may be simple, yet their action is marked, and in most cases sufficiently accounts for the relief. It is no objection against them to say that simple common warm baths would produce the same cures with the same attention to diet and regimen; they *possibly* would; but let the objector persuade his patients to make the same trial at home with his own baths, and furnish a series of cases to prove his point, and not be contented with a bare assertion, hitherto theoretical. As our object is to confine ourselves almost wholly to the action of these waters on the body in curing disease, we shall particularly insist upon their visible effects on the bowels, kidneys, skin, and on other secretions.

But in addition to the action of these waters as purgatives, diuretics, diaphoretics, and tonics, many physicians who have watched their actions are persuaded that they often act as alteratives, restoring healthy actions, and removing diseased ones, without any other effect very obvious to the senses; the improved health being the only visible change. But although many admit this alterative action yet they consider it is conjoined with some more visible one, and that usually during the course of treatment a crisis takes place; that there are symptoms of febrile irritation, uneasiness in the head, oppression in the breathing, strong and accelerated pulse, white tongue; and that these are the precursors of critical evacuations from the skin, bowels, &c. The knowledge of physicians of the action of alteratives is very limited: we recognize the effects only, but the means by which these effects are produced are unknown. In iritis, for instance, we see that lymph is gradually absorbed under the use of small doses of calomel and opium, which produce no sensible evacuation; bleeding and purgatives will produce the same effect very often, but at a greater expense to the constitution. Most of our alteratives, if given in larger doses, produce sensible excretions, (for instance, calomel, antimony, hydriodate of potash,) and it may happen that even in smaller doses they produce the same kind of action in a less

* Rapport fait à l'Académie Royale de Médecine sur les Eaux Minérales de France, 1834-5-6, p. 53.

evident manner, differing in degree only; both curing disease on the same principle,—aperients and other evacuants increasing the secretions more actively, and alteratives more gently. However this may be, we are certainly too ignorant of the *modus operandi* of some of the commonest medicines we daily prescribe, to have any good grounds for disbelieving, from analogical reasoning, the peculiar action of these waters. Analogy would lead us the other way: where the saline salts are in large quantities, they purge or act sensibly on the evacuations, and in smaller quantities when absorbed (as they are most readily into the system) there seems no grounds for disbelieving they can act as alteratives. Mineral waters may therefore be classed with those therapeutical agents, which act (1) as evacuants, producing secretions from the skin, kidneys, and intestines; (2) as alteratives; and (3) as tonics. The alterative action is, however, so generally conjoined with one of the others, that although we may recognize it, yet, for practical purposes, the twofold division, into evacuants and tonics is perhaps sufficient. Few practitioners of medicine but will acknowledge that they are most successful in curing chronic diseases, especially those in which the abdominal viscera are in any way involved, by producing visible secretions, and that tonics (notwithstanding their name) cannot be depended upon, and besides, are rarely prescribed, unless they have been preceded by aperients, or given in conjunction with them. In very many cases the judicious combination of aperients and tonics produce the most satisfactory results. It is in the same great class of diseases that mineral waters are useful, and in many nature has combined evacuants and tonics, so that whilst they produce secretions from the bowels there is no diminution of power.

IV. Regarded in a practical point of view, and according to their chief constituents, mineral waters may be divided into *Saline*, *Alkaline*, *Chalybeate*, and *Sulphurous*.

Saline Aperient Waters. The saline aperient waters contain many salts, which we are well aware exert a very marked and visible action upon the economy. Of these there are sulphate of soda (Glauber salt), sulphate of magnesia (Epsom salt), and muriate of soda (common salt); and the water which contains either one or more of these as its principal ingredient, when drunk freely, increases the secretions from the intestines, as well as their peristaltic actions, and acts as a diuretic. In some cases, the Germans say, no aperient effect is produced for some time; but after taking the waters for a fortnight, more or less, the body becomes saturated; general reaction, with feverish symptoms, comes on, which is relieved by some critical discharge. Waters containing one or more of these salts, and acting evidently on the bowels, are found at Carlsbad, Marienbad, Egra, Kissingen, Wiesbaden, and Baden-Baden, all in Germany; and at Cheltenham, Leamington, and Harrowgate in England. Of these the waters of Carlsbad, Marienbad,* Egra, and Cheltenham* owe their chief action to sulphate of soda, whilst the waters of Kissingen, Wiesbaden Baden-Baden, Harrowgate, and Leamington are strongly impregnated with muriate of soda. In torpid and phlegmatic habits of body, where such a course of aperients is necessary, the latter which contain the more stimulating muriates are preferable.

* The Kreuzbrunn of Marienbad, and the Old Well and Montpelier Spa of Cheltenham.

Sulphate of magnesia is the active ingredient in the water exported from Seidlitz and Seidschütz: and muriate of magnesia is in large quantity in the Püllna water.

Alkaline Waters. The salts of lime, in one or other of their combinations, are found in almost all mineral waters, though not in general in large quantities. In Germany a considerable portion of carbonate of lime exists in combination with the active aperients of Carlsbad, Marienbad, and Kissingen, as well as of Püllna and Seidschütz. Muriate of lime is in combination with muriate of soda, &c., in the Wiesbaden waters, but it is found much more frequently, and in greater quantity in the English springs, particularly at Harrowgate and Leamington. Sulphate of lime is a constituent of the waters of Bath and Scarborough. Bicarbonate of soda is the alkali most frequently met with. It is in considerable quantity, and is the principal ingredient in the waters of Ems, Töplitz, Mont-d'Or, and Vichy; it is also combined with saline aperients in the waters of Marienbad, Egra, and Carlsbad. In England it is found at Harrowgate, together with common salt, and also in small quantities in some of the less-known Yorkshire springs described by Dr. Granville. Carbonate of magnesia is found in the saline waters of Marienbad, and Kissingen, in the alkaline water of Ems, in the purgative waters of Püllna, Seidschütz, and Seidlitz, and in the chalybeate springs of Spa, Pyrmont, and Schwalbach,—in small quantities in all. It can hardly be said to exist in the English waters. There are traces of it only in some, and an appreciable quantity has been found in one spring at Cheltenham.

Chalybeate Waters. Iron exists in mineral waters in the most convenient form for acting on the body, dissolved in an excess of carbonic acid gas. It is combined in small quantities with all the aperient saline waters of Germany, thus giving them a mild tonic action; but when existing in greater simplicity in waters saturated with carbonic acid gas it forms a most agreeable beverage, which women of the most delicate constitutions, labouring under real debility of the body, in which the stomach partakes, can drink in considerable quantities, not only without nausea, (as is the effect of our medicinal chalybeates) but with pleasure and gratification to their tastes; the carbonic acid giving the water a sparkling briskness, a piquant flavour, and an exhilarating effect on the whole system, somewhat of the nature of intoxication. It is not unlikely that the carbonic acid with which the mineral springs of Germany abound, may be one of the agents on which their effects considerably depend, as the effect of carbonic acid in the stomach, or even on the skin is very marked.

It is a powerful stimulant of the nervous system. When breathed we well know it is rapidly fatal by producing spasmodic constriction of the glottis; but when taken into the stomach or applied to the skin it is a powerful stimulant. The immediate effects of champagne on the system differs from that of other wine. The sudden exhilaration, the immediate increase of spirits (so often indicated by the general flow of lively talk), which even a small quantity produces, is not precisely similar to the intoxication from other wines, which is a slower process and attended with subsequent depression: and the intoxication is not at all in proportion to the quantity of alcohol; for champagne contains but a small proportion

of alcohol in comparison to common dinner wines: whilst the ordinary French and German light wines, which are of the same character as champagne as far as regards alcohol, but which are not effervescent, do not produce the same instant exhilaration. The union of the carbonic acid with the spirit may be the cause of the difference of effect. Women with susceptible nervous systems and general delicacy of organization feel this sudden exhilarating intoxication at once, but by diluting the champagne with water it is much diminished, although the same quantity is drunk. A similar class of persons, who are unused to stimuli, feel an exhilaration and increase of spirits from soda-water; and at Pyrmont, in Westphalia, the chalybeate water which is highly charged with carbonic acid gas is drunk by the country people partly as a medicine, but partly on account of the kind of intoxication it produces. To the union of carbonic acid gas with so many of the German waters may perhaps be partly attributed the pleasure with which they are taken, the facility with which large quantities are drunk at a time, without any feeling of distension, the sensation as if the whole body was permeated and pleasantly acted on by the fluid, the increased appetite for breakfast, and the improved spirits. We shall not soon forget the pleasure of drinking at its source among the mountains, after fatiguing rambles, one of these highly aerated waters, which was otherwise so simple in its chemical composition that to its carbonic acid chiefly could be ascribed its effects. In pharmacy also the grateful stimulus of carbonic acid is constantly recognized, and saline aperients in an effervescing form are the most agreeable medicines that are prescribed. The effect of carbonic acid gas applied externally in paralysis is great; (we shall return to this subject when examining gas baths.)

Sulphureous, Ioduretted Waters, &c. Some other principles which we are in the habit of administering as active medicines are found in mineral waters, such as sulphur, iodine.

Sulphur, so much used for piles, cutaneous diseases, rheumatism, &c., is found in form of sulphuretted hydrogen in the cold waters of Harrowgate, and in the thermal springs of Aix-la-Chapelle, Barèges, and some of the other Pyrenean spas.

Iodine has been discovered by chemists in a few foreign springs, as well as English ones. At Woodhall, in the neighbourhood of Ashby-de-la-Zouche, are springs, the water of which contains more iodine than has yet been found in any British waters. In a gallon of the Woodhall water there is as much as half a grain of iodine, whereas no more than one tenth of a grain has been hitherto found in Britain in the same quantity. (Granville.) Local interest has been excited in these springs in scrofulous and rheumatic affections. Creuznach is the chief iodine spa of Germany.

V. We shall now proceed to give a general view of the diseases which have been benefited and cured by mineral waters, in order to convey clearer ideas of the whole subject, and to prevent repetitions when describing the virtues of the particular springs. Instead of going through the whole nosology and introducing every disease which by any possibility may improve whilst the patient is undergoing the strict diet and excellent regimen which are observed at these watering places, we shall confine our observations to those complaints which both theoretically

and practically, both from what we should judge *a priori*, from our own experience in the treatment of diseases by the ordinary method, and what we gather from the testimonies of those physicians who have had ample opportunities of closely watching the effects of these waters upon the human body,—appear to be relieved in a marked and indisputable way by this peculiar class of therapeutic agents.

As a general rule,—and all such rules in medicine must be construed generally, and never be supposed to be without exceptions,—mineral waters are inapplicable in all acute inflammations, in all the more active stages of chronic inflammations marked by pain and febrile action, in diseases of the heart, and in aneurisms, in epilepsy, in tendencies to apoplexy or great congestion of blood towards the head, and in dropsies. They are applicable to chronic diseases, and to such stages as are unattended with febrile action. From some observations at Vichy it would, however, seem that acute attacks of gout in the joints do not contraindicate their use.

It may be said generally that mineral waters are especially applicable to the diseases of those who by over indulgence and luxury, by a neglect (voluntary, or from circumstances,) of that temperance in all things, which is one of the secrets of well-being, or owing to these causes combined with a faulty organization entailed upon them by the sins of their ancestors, suffer from derangement and disorders of the general health, or from local maladies depending on the same general causes. Such are diseases of the digestive and abdominal organs with their numerous train of sympathetic disorders;—nervous complaints, particularly such as are symptomatic and dependent upon disordered digestion, or uterine irritation, as well as a rarer kind resulting from pure debility;—gout and rheumatism, generally connected with a faulty digestion; and gravel, stone, &c., the diseases of the same disordered condition; skin diseases and chronic diseases of mucous membranes attended with undue secretion, and often (together with enlarged glands) connected with a strumous habit of body.

This summary certainly embraces a large catalogue of diseases to be cured or relieved by the same remedy; but it must be remembered that mineral waters include saline aperients, diuretics, alkalies, and tonics, besides acting (as hot baths) very powerfully upon the skin, by stimulating it and promoting perspiration. Thus they comprehend such remedies as in practice we prescribe most commonly, and find most efficacious in the majority of chronic diseases which we have to treat.

Although not disposed to value a theory of disease as more than a generalization, not absolutely true, but as including and explaining the class of facts we are at present acquainted with, which a more exact knowledge will probably displace for a more comprehensive theory, yet we would not reject the attempt, for we are all (though we may be unconscious of it, as was M. Jourdain that he had been talking prose all his life,) inveterate theorists. Upwards, through the different degrees of practitioners of medicine, from the "old woman" to the man whose practice has been guided by the most reflective and steady reasoning, all try to explain, to give a reason for the belief that is in them; and as this is the tendency of the human mind, and a very useful one, we shall introduce here in as short and as comprehensive a form as we can, the general explanation of

disease which many of the most thoughtful and practical Germans now adopt, and which is, we think, a successful one as far at least as it points to a correct plan of treatment. We shall take for our text the view which is given by Vetter of the opinions of Puchelt, Kreysig, Sachs, and other German physicians, as well as his own; but not entering into it with the same minuteness, nor touching on the more speculative parts. The words which, in this country, are alone used by metaphysicians of the transcendental schools, and mysterious speculations on that most mysterious of all subjects,—life,—are commonly met with in German books of medicine, but are especially unsuited to the peculiar state of medical intellect in this country.—But to pass on to the theory.

The body comes into a plethoric state, the consequence of which is an irregular distribution of blood to some particular organ: an undue quantity is sent to it, overfilling it. Diminishing the quantity of blood by venesection will relieve this, but only temporarily; the same condition will return, requiring bleeding at regular and stated intervals, and, although the character of the disease may be changed, health is not restored. We must, therefore, look for the cause somewhat further on, and see whether or not the state of the digestive organs is not such as to supply imperfect blood. On very careful examination, the error will be found here, and, if discovered early, a strictly temperate diet, water-drinking, and the other hygienic remedies, will restore the threatened health. Few, however, are aware of the necessity, or are willing to submit to be deprived of present gratification with the expectation of future and distant benefit; and nature has many processes of her own to restore the lost balance. The activity of the liver is increased, and the flow of bile relieves the disordered condition of the blood, or slow dilatations of the vessels take place, forming diverticula for the plethoric condition, as varicose veins, or places for actual discharges, as piles. Every secerning tissue is ready to separate from the blood products of abnormal nutrition, according to its own powers. The cellular tissue often separates it copiously in the form of fat: hence unhealthy fatness so commonly precedes inveterate abdominal diseases. The disease now takes one form or another, according to the organ or tissue involved; partly as their actions are morbidly increased or diminished, partly as their substance is changed, enlarged, thickened, hardened, or filled with morbid products. This is the explanation of those diseases in which the blood is overloaded with fibrin. There are other classes in which the nervous system refuses to be an agent in carrying on the morbid changes, in which there is no harmony between the morbid processes and the wants of the economy;—or, we may state it thus,—that the vis medicatrix naturæ is altogether unable to cope with the disease, its regulating power is lost, and anomalous, irregular, incurable diseases follow.

If this theory is correct, and the steps of disease are, disordered nutrition, producing a blood abnormal in quantity and quality, from whence arises irregular distributions of blood to particular organs, and in time organic changes in those organs, it follows practically that we must look to the digestive organs as the great means of cure, and our endeavours must be to produce a more wholesome blood. In applying these views to practice, we necessarily come to consider the disorders of digestion in the first place, and first of the earlier stages of these.

In early dyspepsia, the tongue may be clean, the countenance healthy, the body neither ill-nourished nor unhealthy in appearance, neither the pulse, respiration, temperature, nor evacuations perceptibly wrong, still there is more or less loss of appetite, disinclination to food, inclination to nausea; or, on the contrary, too great desire for food, quick and unusual return of the feeling of hunger, the sensation of satiety takes place too soon, or is too long delayed. In other cases, together with these, there are more marked local symptoms: sensations of heat, heartburn, fulness, oppression both of the head and stomach, nausea, changes of taste, of smell. These symptoms may depend on the diet, and cease when it is changed; or they may be owing to irritation or to debility, as in commencing old age, and in pregnancy, &c.

When a disordered digestion has continued some length of time, other symptoms ensue, such as chronic vomiting or diarrhœa, acid secretions, heartburn, or diseases of the abdominal viscera from irritation and congestion. Sometimes mucous, gastric, or bilious fevers are developed under the immediate influence of atmospheric causes acting upon this unhealthy state of body. Or the primary symptoms of indigestion may subside, and the disease may be transferred to the lymphatic vessels and glands, or to the veins. The diseases in which the excitability of the lymphatic system preponderates over all others are the various forms of scrofula; eventually the venous system gradually predominates over the irritated lymphatic system; the veins are considerably increased and distended, whilst the lymphatic vessels and glands decrease in their relative size and activity.

The second group of diseases are those in which irritation of the nutritive functions is followed by a plethoric state of the veins. This condition of the venous system, although not sufficiently recognized in the English schools, was not overlooked by the older writers, who admitted "plethora ad venas." The Germans regard it as a condition with which many forms of disease are connected, such as local congestions of various organs, scirrhus, fungus hæmatodes, many varieties of nervous diseases, and hypochondriasis and hysteria, which depend on organic causes. Abdominal plethora, depending on congestion of the veins of the portal system, is, it is thought, a very common complaint, and one in which mineral waters of the aperient class are eminently useful. Forms of gout, stone, and venous dropsy are supposed to be in connexion with this same condition of the veins, which are imagined not only to be passively over-filled and dilated, but to be in a state of increased activity and irritation. It is not surprising that, having these views, the Germans should regard piles as so important an indication of the internal state of the abdominal viscera, and as often an efficient safety-valve for their diseases.

With such pathological principles, says Vetter, which are embraced by those physicians who have an eye to see the unity of idea behind the vast variety of appearances, a more extensive view is taken of the nature of disease and of the action of remedies; and the observer regards whole lists of names of diseases as pathological metamorphoses arising from one and the same cause. He ceases to wonder that the same remedies cure so many apparently different complaints. In fact, this is a comprehensive view of the constitutional origin of local diseases—a truth which Abernethy saw so clearly, whilst he was candid enough to confess

that he had not attended to medical cases with that degree of observation which would lead him to clearly develope the whole treatment. His one remedy—a mercurial and a black draught—was an unfortunate one; not in his hands, perhaps, but in those of others who signally abused it. Abernethy stated that he never had had experience in purging: his method was, to excite by means of medicine some copious and healthy secretions; but his scholars went (as people usually do) much beyond their master, and were inveterate prescribers of purgatives, introducing a practice which, indiscriminately adopted, has been attended with much mischief. The practical application of a theory which traces so many diseases to a faulty state of the digestive organs, and grounds the basis of their cure on improved nutrition, will be more obvious as we enter a little more into detail respecting the nature and character of these complaints. To this we next proceed.

A. *Diseases and disorders of the digestive organs.* We have alluded to the early symptoms of these diseases, but it is rather their consequences for which mineral waters are sought; and the class of waters which produce the most benefit are those which excite copious secretions from the mucous membrane of the intestinal canal; their efficacy chiefly depending on glauber, epsom, or common salt, in combination with carbonic acid, and small quantities of the salts of magnesia, and a little iron. The symptoms of chronic diseases of the abdominal organs or of their secretions are, according to Heidler, a yellow, pale, or cachectic complexion, want of appetite, bitter or pasty taste in the mouth, vomiting, oppression or cramp of the stomach, colic, constipation or diarrhœa, menstrual irregularities, sterility, leuchorrhœa, together with symptomatic disturbances of the nervous system, as hypochondriasis, hysteria, epilepsy, headaches, giddiness, dizziness, tinnitus, want of sleep, low spirits, palpitations, languid muscular power, cold hands and feet, small, feeble, hard, and sometimes intermittent pulse. Abdominal plethora, that is, obstructions or congestions of one or more of the abdominal organs, (running into organic changes of structure,) from a morbid excitement of the vital activity of the veins of the abdomen or of the whole body, (*venositas aucta, turgor venosus*,) is considered to be the very common cause of these symptoms. Stahl, Kaempf, Koch, Marcard, Kreysig, Hufeland, and other eminent physicians of Germany, who have recognized the frequency of this condition, have recommended aperients and deobstruents that do not weaken. This is the effect of the saline aperient mineral waters of Germany; such as those of Carlsbad, Kissingen, and Marienbad. In such cases, Dr. Heidler says, the Kreuzbrunn of Marienbad produces morbid evacuations of the bowels of every kind, green, black, gray, glassy, gelatinous, like pitch, or yelks of eggs, or ley, and coagulated blood; and this evacuation may be continued for weeks with a manifest improvement of strength, appetite, spirits, and a return to health. The brown and earthy tint of the complexions of many patients becomes clear, and their dispositions more gay and natural after having evacuated for many weeks or even months black excrements like pitch, by the use of these waters. Dr. Heidler adds, that the existence of this complexion should always fix the practitioner's attention in chronic disease; for even when it is the result of some preceding dis-

ease, it may, before its elimination, contribute to and aggravate the symptoms.

There can be little doubt that stools of this kind are rather excrementitious matters discharged from the system than mere accumulations in the intestines which are brought away—a fact recognized by the ancients who attributed many diseases to atra bilis, as well as by many moderns. Ramazzini considered that a general sallowness and darkness of skin, with a torpid condition of the skin, and faulty digestion, in workmen who led a sedentary life, was owing to the excrementitious parts of the fluids being imperfectly discharged. The most marked examples of abdominal plethora in patients at these spas are fat, abdominous, middle-aged Germans, who live sensual, sedentary lives, eating and drinking gluttonously and smoking incessantly; they resort annually to their spa, requiring without doubt to be cleared from that load of excrementitious matter which their habits have accumulated, and which Dr. Heidler so graphically describes as expelled from their bowels for months together.

But the same disease often exists in those whose external condition is very different, and who are suffering from various nervous symptoms which are often attributed to mere debility, and are treated accordingly. The sallow or pale colour of the face is not an invariable symptom. Our countrymen who are of fair complexion, who live on succulent meats, and are much exposed to the open air, often get into this plethoric condition and suffer severely both from the local symptoms of indigestion and hypochondriacal and other secondary nervous symptoms, although their fresh and high colour, their "fair round bellies," and their portly forms seem to indicate high health. It will be found, however, that gentle saline aperients will bring away from these, similar secretions.

Dr. Heidler's theory is, that the saline mineral waters which increase the secretions from the intestines differ in their action from purgatives, with which they are often confounded. Purgatives act by their irritating and often poisonous properties. Nature gets rid of them, as of any other morbid irritant, either by increased secretion or increased peristaltic action. But this water [he refers to the Kreuzbrunn of Marienbad, but his observations are applicable to all similar ones] is not an irritant. The rapidity with which it is digested, its rapid and complete absorption from the intestinal canal, the pleasure with which it is taken, the total absence of any disagreeable sensation, and the feeling of health which follows the evacuations, as well as the quality of the excrements, all prove this. They are critical evacuations, the effect of the increased healthy action of the organs from the roused vis medicatrix naturæ, and altogether different from the increased activity—evidently morbid—produced by purgatives. These are rather deobstruents (restoratives); and by confounding their action with purgatives, a fear is engendered lest they should weaken and injure the stomach. But how can a remedy which increases the appetite and digestion attack the stomach? how can a remedy diminish the vital powers which developes the impaired muscular power, renders the circulation more free, the pulse stronger, the mind more calm, the sleep more tranquil, the complexion clearer? The fatigue sometimes felt by those who drink this water is like the lassitude of the

plethoric, or that arising from a large meal, or from a glass more wine than usual. It is particularly felt by the sanguine and irritable, and arises from the slightly exciting nature of the water, and often from the simultaneous use of hot baths, or from too much exercise, or it is the forerunner of a crisis. But whilst we accept with satisfaction Dr. Heidler's facts as to the pleasant and agreeable way in which these waters act upon the body, so different, indeed, to our pills and senna draughts, yet we do not think there are sufficient grounds for removing them from the great class of aperients. Owing to Abernethy and Hamilton, the action of aperients has been more closely studied in this country than on the continent; and we well know that, as a general rule, aperients relieve most efficaciously when so combined as to produce the least pain and disturbance. Hippocrates in one of his aphorisms asserts the same thing, laying it down as an axiom, that a purgative has been given with judgment when its immediate effect is relief instead of discomfort. It is needless to state in this country, that jalap, colocynth, scammony, calomel, &c., may be so combined as to act without any griping feelings of irritation, and that they often, when judiciously persevered in, bring away large quantities of foul, fetid, unnatural excretions, with a feeling of improved strength, and ease, and with relief to very anomalous nervous symptoms. In diseases of children this is particularly the case. In such cases we have, like others, been long in the habit of regarding the stools as the result of the excretion of excrementitious matter from the body, promoted by the purgative through the channels of the liver and bowels, on account of the quantity, the long continuance, and the varied appearance, increasing often in unhealthy aspect as the purgatives are persevered in. It seems as if it were the common result of this class of mineral waters in cases where the general health is much impaired from long-continued errors in diet, and other bad habits resulting from various degrees of luxury, to bring away foul excretions with relief. But in practice we know that the same effects follow very frequently a well-adjusted course of saline aperients which are so combined as neither to gripe nor to debilitate. They may probably act less surely, and be less pleasant remedies than nature mixes in her vast internal laboratory, but on this account we would not remove the latter into a distinct class, but rather believe that sulphate of soda or sulphate of magnesia, when in natural solution, act very much in the same way as the same salts when dissolved by ourselves from crystals, or even like the judiciously-prescribed aperients from the vegetable or mineral kingdoms; which, however, are more dangerous tools. In cases, then, of diseases of the digestive and other abdominal organs of long standing, brought on by habits of indulgence in the pleasures of the table, or in long-continued errors as to quantity; in all persons the whole of whose fluids are disordered from such causes, and whose general health is deranged, the saline aperient waters are likely to be of use. Those which are most in vogue in Germany are the waters of Carlsbad, Kissingen, and Marienbad.

B. *Gout and rheumatism.* The theory of gout which explains most satisfactorily the symptoms and the effect of remedies seems to be, that it consists of some morbid principle or principles in the fluids of the body, generated either by hereditary predisposition or by a long-continued indulgence in stimulating food and drink, and in luxurious habits,

or most commonly by both combined; producing, whilst circulating through the system, various anomalous and distressing symptoms,—indigestion, flatulence, low spirits, various nervous pains, palpitations, inflammation, and depositions in the smaller joints—chiefly of the hands and feet, acid sweats, deposits in the urine, &c. The connexion between gout and stone and gravel is well known: the same causes often produce both—nutritive drinks, rich meats, and all highly-azotized food; and in both diseases there is a tendency to the deposition of lithic acid in the urine. Gouty concretions in the joints are composed of lithate of soda; the sweat of gouty persons is acid, and according to Chelius there is double the quantity of lithic acid in the urine of such persons. Colchicum, it has been chemically proved, increases the same principle in the urine;* and according to M. Donné, excess of animal food, and the abuse of tea and coffee, and even tobacco-smoking produces crystals of uric acid; hence some chemical physicians have been led to regard the morbid principle of gout as uric acid. We shall recur to this subject when referring to the effect of alkaline waters on gout.

The following observations of Dr. Heidler are in accordance with the experience of every physician:

"The cure of gout does not consist in curing the crisis, (the fit;) the operations of nature should be then only watched, so that they may be neither excessive nor impeded. The patient often imagines he loses his disease at the same time as the pain, and exposes himself again to the bad influences of diet and air as soon as he is temporarily relieved, forgetting that this is the time to effect the slow and the radical cure. But unless using moderate diet, and abstaining from irritants, spirits, and all nourishment too abundant or of difficult digestion, unless giving up all those habits which distinguish the mode of life of the rich inhabitant of a city from that of a healthy and laborious countryman, all attempts to cure rooted inveterate gout are useless. The basis of the cure is, strict attention to regimen and diet. The patients should never forget that they carry with them the latent germ of the disease which is developed from time to time, and shows itself by inflammation of some joint; and unless the functions of digestion, chylification, sanguification, and nutrition, and the fluids themselves are improved, medicine is useless."

Thermal waters in the form of baths are those most esteemed in the cure of gout. Some of them are impregnated with saline salts and alkalies, but all, from their heat and stimulating action on the skin, promote powerfully the perspiration, and are of advantage in proportion as the abdominal functions are regular, and when nature has chosen the skin, kidneys, or joints for the excretion of the gouty principles (Heidler). From the acid nature of the excretions of gout, alkalies have been proposed as a remedy. Common alkalies were found to be inconvenient, as they disordered the functions of the stomach, unless given in very small doses: this effect was, however, obviated by combining the alkali with an excess of carbonic acid, and the bicarbonate of soda and potash were preferred. There are several mineral waters which, besides being hot, hold in solution bicarbonate of soda, and also are charged with carbonic acid gas, so that they are adapted to neutralize this acid state of the fluids, and to agree with the stomach: such are the baths of Vichy, Mont-d'Or, Ems, and Töplitz. MM. D'Arcet, Chevalier, and Petit have shown, by many experiments, some of them on their own persons, the rapid effects of the Vichy waters in rendering the urine alkaline. Two

* Dict. Univ. de Mat. Med. Par Merat et de Lens. Vol. ii. p. 360.

glasses (containing about 46 grains of bicarbonate of soda) render the urine speedily alkaline, and the previous acidity does not return for eight or nine hours. A single bath has the same effect; and, finally, those who drink as many as five glasses of the water, and bathe daily, have their urine rendered alkaline during the whole time they submit to the treatment, which is from thirty to forty days. This alkalization is not confined to the urine, but extends to the cutaneous transpiration and all the other secretions. This effect in the fluids, produced by bicarbonate of soda, has before been described by Falconer and others, also by Ingen-Houz. (Rapport, &c., p. 23.) In addition, therefore, to their heat producing copious perspiration, these waters must combat one of the most marked symptoms in gout, the acidity of secretions; and when due attention is paid to regimen and diet it is not surprising that they effect many cures.

Gout, however, is often combined with an irritable and congested state of the mucous membrane of the stomach and bowels, and with abdominal or general plethora; and, in such cases, the use of those saline aperient waters which increase the secretions from the mucous membranes of the intestines and from the kidneys, either before the hot baths or simultaneously, is strongly indicated. At Marienbad, there are both kinds of remedies, aperient saline waters and hot baths; and as gouty patients resort there from all parts of Europe the effects of the combination of these means have been tried on a considerable scale. The saline aperient spring, called the Kreuzbrunn, is a water which manifestly increases the secretions from the intestines and kidneys, and it first came into vogue from its beneficial effects in arthritic diseases.

"If experience," says Dr Heidler, "is consulted as to those remedies which have had the highest and longest reputation for the cure of gout, they will be found to be hot baths externally, and mineral waters internally. Their action is to increase the activity of the organs of secretion, of the skin, kidneys, and intestines. Next to these come guaiacum, sulphur, antimony and other medicines, to which are attributed anti-arthritic properties, but these also, in small doses, increase transpiration, and all the secretions from the external surfaces, and in larger doses augment the secretions from the intestines and other internal organs. Further, experience teaches us, that among mineral waters the preference is due to those which may be employed without inconvenience in the greatest quantity, and for the longest time; that is to say, those which are of the easiest digestion, and pass most easily, increasing the appetite, and all the secretions and excretions moderately, and without disagreeable sensations."

Hot baths are often indicated in the more irregular forms of gout, in which "the gouty principle" is not expelled readily by the usual channels. It is well known that very ambiguous symptoms, such as hypochondriasis, irregularity in the actions of the heart, anomalous nervous symptoms, and various forms of indigestion, occasionally precede, for months, the first appearance of a fit of gout, or affect those who have previously had attacks of gout. In such cases our older physicians were in the habit of sending their patients to Bath to bring on a fit of gout, and if the waters produced this effect, the general symptoms were relieved.

"The following symptoms," says M. Petit, of Vichy, "indicate visceral or internal gout. When patients consult me with internal complaints of some standing, such as gastralgias, certain hepatic affections, sometimes accompa-

nied with piles; some symptoms of affections of the heart or lungs, and if these affections have often varied in intensity, changed their situation, or ceased entirely to return in the same place—if they have resisted various means—if, finally, they are anomalous, unusual, 'bizarre,' I always enquire if their parents or family are gouty, and if they have been subject to gout, and if the answer is in the affirmative, I consider the complaint may be of a gouty character, and I do not hesitate to try the hot alkaline baths and waters, (especially if these patients have ever had pains resembling gout.) In the majority of these cases much advantage has been derived."

In *chronic rheumatism*, natural hot waters used as baths have been always held in great estimation. In the report to the Academy of Medicine on the mineral waters of France, from 1834 to 1836, by M. Merat, it is estimated that 30,000 persons, affected with rheumatism, annually resort to the use of the mineral waters of France, and with very decided benefit. Rigidity of the joints, (not anchylosis,) not of very long standing, a frequent sequence of rheumatism, is much relieved. In the selection of waters the general principles which would guide us in gout would decide the choice. If there was manifest disorder of the digestive organs hot water alone should not be trusted in. Dr. Heidler finds that the number who are cured of rheumatic pains at Marienbad is very great. There hot baths are combined with the internal use of waters which increase the secretions from the bowels and kidneys.

The connexion of stone and gravel with the gouty diathesis is well known. Renal and visceral calculi often arise from the same causes, and often alternate with gout, and the calcareous concretions in the joints, lungs, heart, and other organs of gouty subjects, are of uric acid. In such cases the hot alkaline waters of Vichy, Ems, and Töplitz, as well as the saline aperient waters which contain carbonate of soda and magnesia in small quantities, in addition to neutral aperient salts, such as those of Marienbad, are indicated. In a recent number we noticed, at considerable length, the effects of the alkaline waters of Vichy in such cases. As these waters rapidly alkalize the fluids of the body, they may reasonably be expected to produce somewhat of the same effect on calculi within the body, and are therefore suitable remedies in these diseases. In all such cases, however, it need not be stated that due regard must be had to the state of the digestive organs, and to the diet and habits, as alkalies can only palliate an effect, not remove its cause.

The hot springs which are in the highest repute for the cure of gout and rheumatism, are the hot alkaline waters of Vichy, Ems, and Töplitz; the hot salt baths of Wiesbaden, and Baden-Baden; the aperient salines of Carlsbad and Marienbad; the hot baths of Bath; the warm baths of Schlangenbad, Wildbad, Pfeffers, Gastein, and the tepid waters of Buxton, and some similar ones in England.

c. *Nervous diseases.* Under this head we here include only such complaints as are independent of organic change; those, viz., which are the result of pure debility, of a weak watery state of the blood, or are symptomatic of disordered digestion, and of uterine irritation, &c.

The effect of mineral waters in many (so called) nervous diseases, throws some light on this class of complaints. For instance, patients whose disorders have been treated ineffectually on directly opposite plans: with stimulants and tonics, with quinine, wine, electricity, &c., on the theory that their so called nervous diseases arise from pure debility,

subject themselves subsequently to a six weeks' course of gentle saline aperients, which produce two or three loose feculent stools daily, and employ at the same time a simple unstimulating nutritious diet, with fit exercise and early hours, and with relief to all their symptoms. Such results must surely render it more than probable that the complaints of these persons did not arise from pure debility, from real loss of power; but were symptomatic of oppressed power, probably from a congested state of the gastro-intestinal mucous membrane from indigestion, congested liver, abdominal plethora, or some other faulty condition of the abdominal viscera.

In the few observations we have to make on this class of diseases, we shall begin with nervous diseases which really seem to depend on true debility, and shall then pass on to those in which the debility is only apparent. In so doing, we shall make much use of the observations of Dr. Heidler, who has evidently watched carefully and meditated correctly the effects, both of chalybeates and saline aperients in this large class of complaints. Indeed, such observations and experiences are of much use in the treatment of diseases by our usual remedies: they supply indications which, in prescribing, we often endeavour to fulfil, and with much benefit. In this point of view, the effects of mineral waters in disease are of much more general interest to the medical practitioners than is usually imagined. Patients, whose time and circumstances will allow them to visit German spas, are comparatively few; but those who have similar complaints to those relieved by these waters, are extremely numerous; and we all know how very rebellious to common remedies such complaints are, and what a source of anxiety they prove to the medical attendant; so that any hints as to a more satisfactory treatment cannot fail to be valuable. (We quote Dr. Heidler:)

"True nervous debility, depending on real exhaustion of the vital powers, arises from numerous causes, such as sensual excesses, hemorrhages, or losses of other fluids, severe diseases, miscarriages, or laborious labours, grief, long-continued or excessive mental application, want of exercise and pure air, too prolonged suckling, loss of rest, insufficient food, severe mercurialization. Such causes produce weakness of the mind and body, trembling of the limbs, impaired vision, hearing, taste, and smell, &c. The following are the symptoms of true debility when of long standing and uncomplicated: complexion pale, more or less sordid, thin and pasty; nose and upper lip often a little swollen, œdema below the eyes, but no other symptom of dropsy or scrofula; muscles flabby and soft, appetite unequal, thirst in proportion to the appetite, breathing quickened, palpitations with or without flushings in the head or chest at the slightest emotion or exercise; languor and general depression, pulse small, soft, and generally slower than in health, but sometimes quicker; frequent chills, cold hands and feet, secretions of kidneys and bowels often regular, sometimes diarrhœa and constipation, skin too much disposed to sweat or too dry, often general uneasiness, aversion to all exercise, as well as to mental occupation, temper sombre, pusillanimous, irritable."

For this condition the chalybeate waters, especially those charged with carbonic acid gas, are beneficial; that is, if there is still power of reaction and no dropsy, no latent inflammation, no organic visceral disease, no accumulations in the bowels, or congestion of the abdominal viscera, and particularly when it is evident that the causes before mentioned have produced the symptoms. Such chalybeates are those of Spa, Swalbach, and the Carolinen-brunnen, and Ambrosius-brunnen of Marienbad. But

it is the opinion of Dr. Heidler, after observing the different effects of chalybeates and saline aperients in nervous affections, that true debility is but rarely the cause of the symptoms, thus confirming the chief principles of treatment so clearly and judiciously laid down by Marcard in the last century.

"The feeling of lassitude and exhaustion, a slow and small pulse, cold extremities and cramps, are usually only symptoms of sluggish circulation, plethora of the portal veins, and morbid irritation of the nervous system of the abdomen. The effect of remedies in these cases, as well as the history of the disease proves this. The symptoms occur in those who eat succulent meats, drink beer and wine, sleep much, are idle bodily and mentally. Those who gain hunger, thirst, and sleep by the sweat of their brow, and live on bread, water, and a little soup, together with the meat from which it is made, are not so affected. False or apparent weakness is common to many patients suffering from disorder in the abdominal circulation; their contracted pulse seems feeble, but it is not conjoined with the other symptoms which characterize either excess or want of vital power. Such patients have these symptoms: sensation of lassitude, palpitations and spasms produced by the slightest exercise and emotion, sometimes even by remaining a short time in a hot room, by a cup of coffee, a glass of wine, or by hot soup; they have constantly cold extremities, shivering, difficult and slow digestion, and complexion earthy, pale or bilious; sometimes, however, good, and indeed too red. Such persons are particularly women suffering from piles, congestions, or a high degree of abdominal plethora. They are believed often to be without strength and feeble, and are treated most injudiciously with tonics and stimulants."

The use of the saline aperient waters in these cases is attended with the greatest advantage. It is often the best antispasmodic and calmative, removing the cause in the intestinal canal by increasing its secretions. Its efficacy in nervous diseases which, under the most varied forms of hypochondriasis, hysteria, debility of the nerves, weakness of the stomach and intestines, make up so large a number of those who resort to Marienbad, both illustrates their nature and the best treatment. In all such cases, Dr. Heidler never neglects the careful examination of the abdomen, even when the patient makes no complaint and the digestion is good, and her stools regular. A fixed pain, slight tenderness on making pressure over the liver, mesentery, ovaries, &c., often are the symptoms of chronic irritation of these parts, the hidden cause of anomalous nervous symptoms.

" Experience shows," continues Dr. Heidler, " that chalybeates, as well as all similar remedies, whose principal effect is exciting and tonic, are less beneficial in these diseases than those whose action is to increase the abdominal secretions. Prudent physicians did not lose sight of this, even at the time when the fashion was to give tone and strength, and it was commonly believed that nothing was more debilitating than a purge, or an emetic, and when those who attended as much to the fluids as to the solids were ridiculed. The passing theory of Brown and Roeschlaub banished the old reputation of Carlsbad and other similar waters. There were never a greater number of hypochondriacs, never more complaints of abdominal affections and weakness of the nerves than when wine, quinine, iron and opium were the universal remedies. Zimmerman, R. Whytt, Tissot and others dissipated some of these illusions, and showed how generally nervous affections were symptomatic rather than local diseases of the nerves."

Chalybeates, as is well known in medical practice, are of much more marked efficacy in the diseases of women than of men. We can rely on

them with more certainty in such cases than perhaps on any other medicine. The same must hold good in chalybeate waters, and therefore in chlorosis, amenorrhea, and in various hysterical or other nervous symptoms, which depend on a torpid state of the uterine functions, they are applicable. For impotence and sterility they are much resorted to on the continent. It is necessary, however, to investigate these cases carefully, for the debility may be more apparent than real. In many cases it may be advisable to employ both remedies, commencing with saline aperients, and finishing with chalybeates. At Marienbad, besides these two kinds, there is a third spring, (the Ferdinand's-brunnen) which contains less sulphate of soda than the Kreuz-brunnen, and more iron. This is often found useful in mixed cases, such as when the patients are phlegmatic and torpid, and, whilst requiring aperients can bear stimulants; or for the very sensitive and nervous, who are generally pale, and, although quick and sudden in their actions, are without real power. Such patients may have local debility of the digestive organs, such as are called cases of weak digestion and torpid bowels, the local condition being often produced by indigestible food, abuse of spirits, purgatives, emetics, and narcotics. Such debility is found by Dr. Heidler to be relieved by this intermediate water, in which a mild aperient and a tonic are combined.

D. *Paralysis.* This is a disease for which thermal baths are much used. Their benefit must depend on the causes, which are various and obscure. When the attack of paralysis is sudden and complete, rendering it probable that it is owing to an apoplectic effusion in the brain, benefit cannot be expected. Nature, we know, gradually absorbs or accommodates herself to the presence of the coagulum, and if the part of the brain in which it was effused, is not extensively disorganized and impaired, the patient slowly recovers the use of his limbs, until the occurrence perhaps of another effusion. In recent cases, the stimulus of a hot bath, by increasing the cerebral circulation, might be of great injury: when the disease is chronic, baths cautiously tried may perhaps be of some slight service in amusing the patient's mind, in affording him change, in supplying him with the most pleasant of all gentle stimuli, that of hope, while we subject him to a careful diet, and to a well-regulated hygienic plan; for in no other way, except in improving the general health, can we suppose benefit to arise. Dr. Heidler's experience of the effects of warm bathing in these cases is as follows:

"Paralyses, from apoplexy of many years' standing, which have not gradually improved, are incurable. The hope of cure is in proportion to the time and number of the relapses, so much the less as the intellect and muscles of the head are the more affected, as the abdominal functions are the more regular, and out of proportion with the paralysis, as the patient has been less bled at the commencement, and as he is more advanced in age."

Dr. Heidler has seen warm baths used in cases of from six months' standing to eight or ten years. Very rarely has he seen perfect restoration, and those who have not improved, have sought health in vain at other baths. But there are many cases of paralysis, even from apoplexy combined with congestions of the abdominal viscera, in which a course of saline aperient waters, combined with warm bathing, may be beneficial.

It should, however, not be forgotten that all mineral waters, when taken internally in any quantity, and also hot baths, have a tendency to

produce congestion of the head, and therefore that where there is a tendency to apoplexy their use may be attended with danger and even death. Sudden deaths from apoplexy at the German's spas are not uncommon.

Paralysis coming on gradually is the most obscure, both in its nature and treatment. There may be no pain, merely gradual loss of power or sensation, slight loss of strength and diminution of the fineness of touch, which is attributed to weakness, and yet some organic changes may produce it.

"It is easier," says Dr. Heidler, "to attribute paralysis to weakness than to examine diligently whether the functions of the nerves are embarrassed by congestions of blood, varicose dilatations, extravasations, tumefactions, &c. This supposed weakness is especially dangerous when a plethoric and robust patient, with strong or suppressed pulse, complains of the first paralytic symptoms. The pulse in the diagnosis of paralysis is often of the highest importance. Whenever it is quick and hard stimulants are wrong even at an advanced period. Notwithstanding the appearance of debility, we should carefully avoid seeking the cause of an incipient local paralysis in the part itself, and using exclusively local treatment, even when at first the functions of the brain appear to be regular, and when there is neither pain nor swelling in the vertebral column."

Dr. Heidler suspects that many cases of apoplexy, called nervous or serous, may be wholly symptomatic of abdominal diseases; for in these latter affections there are many symptoms of depressed nervous energy, such as languor and muscular weakness. Might not, he asks, this condition be pushed further with complete loss of power?

Paralysis which, in Germany, is thought to arise from metastasis, and which, in this country, is overlooked, or disbelieved in (from our contempt of the opinions of our medical ancestors), is considered the form of the disease which is most likely to be benefited by mineral waters. The chief causes are supposed to be the sudden suppression of a natural secretion, as perspiration or menstruation, or of a morbid one of long duration, as certain skin diseases, leucorrhea, piles, &c.; and cases of paralysis of an arthritic or rheumatic origin are believed to come under the same class. In such cases of paralysis (says Dr. Heidler,) Marienbad has acquired its reputation; but when the disease is of many years' standing, and has resulted in idiopathic diseases of the brain and spinal marrow, or of the nerves, or with hectic fever, or great exhaustion of the vital power, a cure cannot be expected.

Paralytics rosort to all the natural hot springs, from the sulphurous waters of Barèges and Aix-la-Chapelle, and the strong hot salt springs of Wiesbaden, to the pure thermal springs of Pfeffers and Gastein.

E. *Chronic discharges from mucous membranes.* When leucorrhea, gleets, and mucous secretions from the rectum are complicated or dependent upon a congested or irritable state of the digestive organs, or a symptom of a bad, flabby, mucous habit of body, (cachexia pituitosa, or a strumous condition of body, in which there is a great tendency to mucous discharges,) it has been found that those waters which sensibly increase the secretions from the intestinal canal are very beneficial.

Chronic bronchial discharges are sometimes benefited by hot baths, used ad sudorem, producing a strong determination of blood to the skin. The hot alkaline baths of Mont-d'Or in France, under the judicious directions of M. Bertrand, the resident physician, are employed with much

success in chronic pulmonary catarrh, and in chronic pneumonia, and in even hemoptysis in patients who are neither febrile, nervous, nor irritable, but whose habit is rather torpid.

Sir James Clark considers that a residence of one or two winters in Italy, and a course of mineral waters suited to the case during the summer, "afford the most effectual means we possess in the more obstinate and deeply-rooted cases of bronchial disease." The selection would depend upon the nature of the case, care being taken that the situation of the spa be suitable as well as the water, as it would not do to send a patient who could not bear a rarefied air to a spring in the Pyrenees, or one who was injuriously affected with damp to a warm close valley in Germany, although the waters might be peculiarly suitable to his case. When the patient's constitution is very delicate, and there are symptoms of abdominal congestion complicating the bronchial disease, Sir James Clark prefers the warm alkaline and slightly aperient baths of Ems.

"In cases of less delicacy, and those especially in which a mountain air promises benefit, or where the bronchial disease is complicated with chronic cutaneous eruptions, Bonnes or Cauterets (in the Pyrenees) will be more effectual. In cases where there exists a very torpid state of the system, and especially a languid or defective action of the skin, or where the occurrence of the bronchial disease has coincided with the disappearance of any cutaneous eruption, the system of bathing adopted at Mont-d'Or will, I believe, effect cures where other waters fail." (Clark, p. 349.)

F. *Chronic skin-diseases.* The obstinacy of this class of diseases to remedies, their liability to return, and to be aggravated by slight causes, need not be insisted upon; and, when they are of long continuance, the system seems to become so habituated to them that it is often a question whether it is not preferable to allow them to remain, than to run the risk that may be incurred by getting rid of them by active or powerful remedies. In medical practice a course of mild saline aperients, together with warm or tepid baths, and careful diet, are as successful in long-continued cases as any remedies we possess, and unlike arsenic, mercury, cantharides, and other powerful agents, can do no harm. We cannot doubt, therefore, that a course of one of these saline waters with warm bathing, change of air, and a good regimen is often serviceable. Dr. Heidler candidly says that his own views on these diseases are not clear. Many patients have been cured by a few baths; others have not been relieved by a large number. His experience makes him believe that where patients have chronic skin diseases of long standing, and are in good health, they gain no good, but when the affection is consequent on indigestion, scrofula, gout, &c., as is often the case, they are relieved by the waters which benefit these diseases.

Sulphurous waters have been especially esteemed in this class of complaints. The waters of Harrowgate owed their celebrity to their supposed efficacy, and they contain sulphur in union with neutral salts, in such a proportion as to act upon the bowels. Many of the patients who resort there have that red, pimply state of the face which is called "scorbutic," and which is often brought on by the sudden application of cold, or appears periodically, owing to a peculiar habit of body—but the waters also cure more obstinate and powerful cutaneous diseases, particularly when their internal use is conjoined with their external employment, as

hot baths. The natural hot sulphurous waters of Aix-la-Chapelle and Barèges are, however, more efficacious than the cold sulphurous waters of our own country.

G. *Scrofula.* Regarding scrofula as a peculiar morbid condition of the whole system, often an hereditary disease, but constantly connected with, or brought into activity by a faulty condition of the nutritive functions, and susceptible of relief chiefly by a scrupulous and habitual attention to the diet, exercise, climate, and other hygienic remedies, together with a medical treatment of the particular local symptoms of the case, we can understand that warm bathing, and the internal use of waters containing such remedies as muriate of soda and lime, iron and iodine, together with others which increase and rectify the secretions from the intestinal canal, with change of air or climate, and a careful diet may often be beneficial. This class of remedies are not specifics against scrofula, and when it is deeply rooted and hereditary they are useless: they must be selected according to the symptoms of the individual cases. On the continent mud baths, and poultices of mud to the parts locally affected, are considerably esteemed, as well as warm bathing generally. "When the patient looks cachectic, and his face is swollen, his belly enlarged and pasty, or he has worms, mucous stools, dyspepsia, or constipation, or any other chronic affection of the abdomen, the use of such waters as the Kreuzbrunn of Marienbad, brings away large mucous stools, sometimes for months together, without weakening the patient, but on the contrary with an improvement in the appearance, strength, and health." (Heidler.) Chalybeates may be taken subsequently.

Such then are the groups of diseases for which mineral waters are employed, very various indeed in symptoms, and in name, but all more or less dependent upon or connected with a faulty condition of the nutritive functions of the body, and thus having a common point of union and of resemblance.

VI. We now pass to the consideration of the particular mineral springs; and as they are exceedingly numerous we shall notice those only which are most frequented, and particularly those of Germany. Our object will be, as far as we can, to give such a sketch of each as will enable the reader to gain a general idea of the nature of the water, and the diseases for which it is suited; our space forbids our entering into minute details, either of chemical analysis or of description.

A. *Saline Aperient Waters.*—CARLSBAD. This celebrated spring lies in Bohemia, about 230 miles from Frankfort, and is, consequently, at the usual rate of travelling at five miles an hour, attainable only after a long and somewhat difficult journey. It is the most famous spa in Germany (the "king of the spas,") and is much resorted to by invalids from all parts of Europe. The situation of Carlsbad is very picturesque. There are several springs: the chief is the Sprudel. It is a hot saline water, a boiling spring (of the temperature of 167°), its chief active ingredient being sulphate of soda. It also contains common salt, a little carbonate of soda, and some iron, besides carbonate of lime; and it is charged with carbonic acid gas. It is employed almost wholly as a drink. Its action on the body is well marked, increasing the secretions from the mucous surfaces. From its heat, carbonic acid, and iron, it is stimulating, as

well as aperient; hence it is efficacious in chronic stomach, and other complaints of the digestive organs in persons of a lymphatic and torpid, or of a bilious and spare temperament, who are not easily excited; also in those of a like temperament who have led indolent luxurious lives, and by eating and drinking too much have laid the foundation of deep-rooted affections of the abdominal viscera. It is, of course, contra-indicated in the highly plethoric and sanguineous, as well as in the irritable and sensitive nervous temperament. The crisis, or "bath storm," is here very marked.

The opinions of the Germans as to the efficacy of the Carlsbad water is very high. Hufeland, when asked by Dr. Granville the reason of the undiminished celebrity of Carlsbad, said: "C'est qu'il guérit des maux rebelles à tout autre moyen curatif." There is not a single medical man of eminence in Germany, adds Dr. Granville, who does not entertain the like opinion. The consequence seems to be that diseases of all kinds are sent there, so that a medical eye cannot detect any prevailing class of complaints by the appearance of the water-drinkers.

Kissingen. This spring lies in Bavaria, two or three days' journey from Frankfort. It is much resorted to by the Germans, and is also attracting the notice of English invalids. There are several springs containing the same ingredients, but in different quantities. The Ragozi spring is the most efficacious, and the one which patients visit the Spa chiefly to drink. Kissingen is a cold water, the principal ingredient being common salt (of which there are 62 grains to a pint); it is highly impregnated with carbonic acid gas and contains some iron. It is thus gently purgative, stimulating, and tonic. It increases the secretions from the kidneys and intestines. It was first brought into repute for its alleged virtue of curing sterility, for which it is still celebrated in Germany. In chronic disorders of the digestive organs, in the torpid and phlegmatic, in those who have exhausted their digestive powers by hard living, in chronic enlargements of the liver from similar causes, in sterility and other uterine affections, where the general symptoms seem to require iron in combination with a stimulating aperient, these waters are indicated. In the plethoric and those disposed to inflammation, as well as in those of sensitive nervous organizations, it is contra-indicated. The water of the "Pandur," another spring, is heated and used as baths. From its being highly charged with carbonic acid gas, it stimulates the vessels and nerves of the skin, and is said to be efficacious in some cases of paralysis, but contra-indicated in the plethoric or in those disposed to determinations of blood to the head. These baths must be a useful adjuvant to the waters taken internally, particularly when the system is torpid. Living is excessively cheap at Kissingen: a dinner for two persons, consisting of several dishes and some excellent Bavarian beer, costs one shilling and eleven pence. Every other repast is proportionably cheap. (Granville.)

Many of the other spas are resorted to as places of amusement as well as of health; this at present is devoted to its right object, and so strictly that "the tables d'hôte are under the strict surveillance of the authorities, and nothing is allowed to be served up that is likely to disagree or to interfere with the beneficial action of the waters." (Lee.)

Could some such wholesome despotism be exerted by the medical attendant over the table of his patients, he would not, in a vast number

of cases, require to recommend a German spa; but men are now somewhat like what they were in the days of Naaman, the Syrian,—disdaining and getting wroth at the suggestion of a simple remedy near at hand.

MARIENBAD. This spring is also situated in Bohemia, about a five hours' drive from Carlsbad; it is comparatively a new spa, but much celebrated in Germany. Its situation is beautiful. It has several springs, both saline and chalybeate. The principal saline spring is the *Kreuzbrunn*, which is cold, containing a large proportion of sulphate of soda; it holds also in solution some common salt, carbonate of soda and magnesia, and iron, and is charged with carbonic acid gas. Hufeland regarded it as the Carlsbad water cooled, and on this account less exciting. It seems to be a water perhaps more generally applicable than any other. It increases the secretions from the intestines, kidneys, and skin; is easily digestible, and acts on the bowels mildly and without uneasiness, often bringing away for weeks together foul secretions with great relief. It is a combination of a gentle aperient, stimulant, and tonic. It is less purgative and exciting than Carlsbad, and less stimulating than the cold salt springs of Kissingen. It is, therefore, not contra-indicated like these in the sanguineous and plethoric who are suffering from disorders of the digestive and other abdominal organs; whilst it is often of great service to those who are apparently suffering from nervous debility, but in reality from a state of irritation of the gastro-intestinal mucous membrane, of which various nervous symptoms, as hypochondriasis, muscular debility, &c., are merely symptomatic. For the torpid and phlegmatic, Carlsbad and Kissingen are preferable; but in the sanguineous and the nervous, the Kreuzbrunn of Marienbad is the fit water. These three springs seem applicable to the same kind of diseases, but the selection should depend on the temperament of the patient, Carlsbad being the most stimulating, Kissingen next, and Marienbad the least. There are three other springs at Marienbad; two of these (the *Carolinenbrunnen* and the *Ambrosiusbrunnen*) are light chalybeates, highly charged with carbonic acid gas, like Swalbach, and often very useful after a course of the saline; or in cases of true debility. The third spring (*Ferdinandsbrunnen*) is intermediate, less aperient, and more tonic than the Kreuzbrunn, and not purely chalybeate like the two others. It is useful in mixed and doubtful cases.

FRANZENSBAD. This is the spa of Egra or Eger in Bohemia, and is a morning's drive from Marienbad. Its situation is dreary and it is going out of fashion, being superseded by the latter spa. It is a cold, highly-carbonated saline aperient; its chief salt is Glauber-salt, with some muriate and carbonate of soda and iron, and it is highly charged with carbonic acid. Franzensbad thus contains the same chemical ingredients as the Kreuzbrunn of Marienbad, but in less quantity; so that it seems to hold a middle place between the Kreuzbrunn, and the direct chalybeates, and may be more suitable to those of a delicate constitution, for whom more active waters may be injurious. The *Saltzequelle*, at the same place, is even less stimulating.

There are three aperient waters exported from Germany, and well imitated by Struve at Brighton. These are Püllna, Seidschütz, and

Seidlitz waters, in the neighbourhood of Carlsbad. *Püllna* water is a speedy and direct purgative, owing to its containing a large quantity of Glauber and Epsom salts. It also holds in solution muriate of magnesia. Wells are dug in the volcanic soil, and the water which soaks into them is bottled for exportation. Dr. Granville praises it highly; he had himself, like an Englishman, been addicted to the use of pills, but now, he says, it is only necessary to drink about two ounces of Püllna waters "to keep all straight."—*Seidschütz* water is of a similar kind, but, besides Glauber and Epsom salts, it contains nitrate of magnesia, which renders it even more bitter. It is an effectual purgative.—*Seidlitz*, the name of which is so well-known, is in the same district. The water owes its efficacy to Epsom salts, containing one drachm and two scruples in a pint. The Seidlitz powders have therefore nothing in common with real Seidlitz waters but the name and the effect.

In England the saline aperient waters are at Cheltenham, Leamington, Harrogate and Scarborough.

Cheltenham. The most active ingredients in the Cheltenham waters are Glauber and common salts; they also contain muriate of lime and magnesia, and a minute quantity of iron. The springs are numerous, and vary much in strength and in the relative proportion of the above-named ingredients. The common opinion is that the visitors are too numerous to be supplied with the real water, and that after a certain hour of the morning, an artificial water of the same character is substituted. The careful way in which the springs themselves are concealed does not tend to dissipate this common delusion, if it be one. The waters act upon the bowels considerably and regularly, without uneasiness or feeling of debility, and are thus of advantage in indigestion caused by full living, as well as in many complaints of the biliary system, brought on by the climate of India, a stimulating diet, and mercury. "The East Indian visits these springs, as a matter of course, upon his arrival in this country." (Scudamore.) "The gouty patient may drink the purely saline waters of Cheltenham," says the same authority, "with almost certain prospect of advantage." Indeed, our observations on the diseases relieved by the German waters of the same class, apply to those of Cheltenham. In one important particular the German waters differ from and excel the English, viz., in being impregnated with carbonic acid gas.

Leamington, in Warwickshire, is superseding Cheltenham as a fashionable spa; both places, however, are even more resorted to as residences for idle people than for the medicinal virtues of their springs. The Leamington waters are saline aperients: common salt, glauber salt, muriate of lime, and magnesia are the principal ingredients, varying much in the different springs; some contain iron and sulphuretted hydrogen gas. When taken in sufficient quantity the saline water acts as an aperient without pain or uneasiness. The diseases for which it is suited are similar to those of Cheltenham, and to the class of saline aperients generally.

Scarborough was once much resorted to for its mineral waters; the chief ingredients being sulphate of magnesia and a small quantity of iron. It acts on the bowels and kidneys.

With the exception of Carlsbad, the springs above described are cold; all of them are used internally, and all belong to the same class of mineral waters. They all act sensibly on the body by increasing the secretions from the intestines and kidneys; and they owe their active properties to saline salts—chiefly sulphate of soda or muriate of soda, combined with a small quantity of iron. We next arrive at the springs which are hot—thermal or volcanic waters, which are used chiefly as baths, but are also taken internally. The first we shall describe are very similar in their chemical contents to the above springs.

B. *Hot saline waters.* WIESBADEN, in Nassau, is more frequented by the English as a medicinal spa than any other. It may be reached in three or four days from England. The principal spring (the *Kochbrunnen*) is a hot, salt water; the principal ingredient being common salt, with which it is very strongly impregnated: temperature, 158° to 160°. It is principally frequented by the gouty and the rheumatic. "In gout of long standing, of the atonic kind, with or without deposition of calcareous matter in the joints, occurring in persons beyond the middle period of life, these baths are calculated to render the most eminent service." (Lee.) The stimulating nature of the water renders it more indicated in torpid and phlegmatic habits than for the more highly plethoric. Where there is much nervous irritability, with gout and rheumatism, the milder hot baths of Vichy, Ems, and Töplitz are preferable. The use of the douche in chronic local affections resulting from gout and rheumatism, adds much to the efficaciousness of the baths. Most chronic rheumatic affections are benefited. "Those depending upon long exposure to wet or cold, to which military men on duty are subject, are especially relieved by these baths." (Lee.) The water taken internally increases the secretions from the bowels and kidneys, and would be therefore useful in those cases where gout and rheumatism are combined with abdominal plethora, or congestion or irritation of the gastro-intestinal mucous membrane. It must be on this account that it relieves hypochondriasis, for the cure of which it is highly extolled.

BADEN-BADEN is in the neighbourhood of Weisbaden, and has hot salt springs of much the same quality. Both places are provided with all kinds of facilities for gambling. "A very few visits to the wells in the morning," says Dr. Johnson, "the hells in the evening, and the hotels in the middle of the day, will convince any observant traveller that three fourths of the sojourners at Baden go there to drink wine rather than water; and to lose money, rather than to regain health." (p. 93.)

C. *Hot Sulphurous Waters.* AIX-LA-CHAPELLE. The temperature of the hot sulphurous water is between 135° and 115° of Fahr. (Wetzlar), "the taste most nauseous, exactly resembling the washings of a gun-barrel, with a dash of rotten egg," (Dr. Johnson); but to this the palate soon becomes reconciled. Besides containing some sulphuretted hydrogen gas, it has nitrogen and carbonic acid gases in considerable quantities. It is highly impregnated with common salt, and also with carbonate of soda, besides some sulphuret of sodium. It is used externally as well as internally. The douche baths are admirably managed.

Its great reputation is for the cure of skin diseases, chronic rheumatism, secondary syphilitic and mercurial diseases, paralysis from lead and other mineral poisons. (Wetzlar.)

BARÈGES, on the French side of the central Pyrenees, is the most generally celebrated hot sulphurous spring, and very much frequented by the French. It is situated 4190 feet above the sea, and the village is only inhabited during the summer season; that is, from June to September. It is crowded, and lodgings are very difficult to be procured, and very expensive. Mr. Carmichael, of Dublin, who was at Barèges in 1829, was obliged to wait a fortnight before he could have the douche bath, and then to take it at eleven at night, as the only disengaged hour. The temperature is from 100° to 135°. The taste is nauseous, with the rotten-egg odour pertaining to sulphuretted hydrogen, but it is almost exclusively employed as baths. When taken internally it is stimulating and diuretic, but not aperient. When used as baths Mr. Carmichael found it a very powerful remedy. His disease was obstinate sciatica, connected with or dependent upon chronic indigestion, with probably gallstones. The douche bath in a quarter of an hour produced feverishness, followed by loss of flesh. It is therefore a remedy contra-indicated in the plethoric or inflammatory, or where there is great nervous irritability, and is alone suited to the chronic forms of disease in the more torpid and less excitable temperament. Great numbers of paralytic persons resort to it. "Partial paralysis depending on some affections of the spinal cord; various neuralgic affections; chronic lumbago and sciatica; chronic rheumatism, and gouty affections of the joints; various cutaneous diseases, such as lepra, psoriasis, &c., are the complaints which have a fair chance of receiving benefit from these powerful waters." (Carmichael.) Their great utility in old, exfoliating gun-shot wounds has been so well established, that it is many years since the French government founded a military hospital into which all appropriate cases are sent for three successive summers. But if the patient after this trial is not found fit for duty, he is either pensioned or discharged from service.*

The BAGNERES DE LUCHON, in the same district, are somewhat similar; and there are other springs among the Pyrenees which contain sulphur. The chemical composition, however, of the mineral waters of this district, as well as of the rest of France, has not been generally ascertained, and much ignorance consequently exists respecting their nature and use.

D. *Hot alkaline waters.* VICHY (Department de l'Allier,) has been very celebrated for its hot alkaline springs; the principal ingredients in which are bicarbonate of soda and carbonic acid gas. Struck with their utility in cases of gravel, and pondering on the connexion between gravel and gout, M. Petit, the resident physician at Vichy, has, in the last seven years, recommended them in gout with success. Previously to this time they were not employed for gout, and an older resident physician in the same place considered them to be injurious. In consequence of this difference of opinion the Royal Academy of Medicine in Paris appointed a commission to investigate the point. The reporter is M. Patissier, one of the members of the Academy, who had formerly expressed an opinion against the efficacy of these waters in the cure of gout. As much care seems to have been taken to arrive at the truth as is possible in such an investigation, and the report bears marks of that methodical arrangement and clearness of statement which belong so peculiarly to our clever

* R. Carmichael; Dublin Medical Journal, May, 1838.

clear-headed French neighbours. In this respect it is a contrast to some of the visionary, one-sided, obscure, exaggerated eulogies which are produced by spa-doctors generally, and by many of the German ones especially. But in saying this we should also add, that the report is an exception to the general carelessness of the French scientific men in regard to their own mineral waters. Very few of them have been analysed; and although the government and the Royal Academy of Medicine have drawn up excellent schemes for statistical returns, and the French system of centralization seems to offer the means of carrying their purpose into effect, yet the apathy and want of candour of the medical men who reside at the springs render all their plans abortive. As, however, this report contains important data for estimating the effects of hot alkaline waters on gout, and the inferences are applicable to all waters of a similar character, we shall take advantage of it.

M. Petit furnished the Academy in proof of his views with eighty cases of gout, not selected, but taken indiscriminately, being all of those who had taken the waters regularly, had followed up temperate habits, had persevered for some time in alkaline treatment, and had subsequently passed over two winters—the season in which attacks of gout are most frequent. Those cases which occurred only the year before the commission was issued were not included, as sufficient time had not elapsed to prove the efficacy of the waters; neither were those cases mentioned of patients who only imperfectly followed the treatment: for it is not probable that a constitutional disease such as gout could be radically cured by the temporary use of any mineral water whatever. To gain all the information possible, the reporter visited all those patients who were in Paris; and the secretaries of the Academy wrote to those who lived in the departments a series of questions: the greatest number answered them; and it is chiefly from the answers which are in accordance with the cases and observations of M. Petit that the cases have been prepared by the reporter, and the deductions drawn. Thus it will be evident that no pains have been spared to arrive at the truth in this report, and although perfect accuracy is impossible, yet that it must be a valuable document.

The deductions here drawn will of course apply not only to Vichy, but to all other warm alkaline springs, such as Ems, Töplitz, and even the less alkaline hot mineral waters which are charged with carbonic acid gas, as the Wildbad, Schlangenbad, &c. The differences in these waters, practically, is perhaps more in degree than in kind; and the variety of degrees is so great, that the medical adviser has a considerable range, and can accommodate the water to the peculiarity of the case. Thus, if circumstances render it doubtful whether the more powerful ones are suitable, the less active waters may be used without fear.

Of the 80 cases furnished by M. Petit, there were 19 in which the gout did not return (at least during two years) after the use of the Vichy baths and waters, and following them up with the constant employment of alkaline drinks, (℥j of bicarbonate of soda in a pint of water,) and a suitable diet, namely, by avoiding acids, highly azotized food, (such as the stronger meats,) and spirituous liquors. There were 51 cases in which the same treatment has rendered the fits of gout less frequent, shorter, and less painful; and 10 cases in which these mineral waters

seem to have been injurious. All were cases of articular gout, either acute or chronic. In 11 of the cases acute fits of gout came on whilst the patients were using the waters; but the attacks were less acute and shorter than under ordinary circumstances, and the feverish condition was considered to be no contra-indication. From a further analysis of the cases, the particulars of which it is unnecessary to give, the waters appear to have the same sanative influence on hereditary as on acquired gout, in long standing as in recent cases; and when it coexists with gravel it is more easily relieved. The power of these waters is very limited in removing the consequences of gout in the joints, such as contractions, nodosities, chalk-stones, or rigidities from anchylosis. Thus it seems that, of the whole 80 cases, 70 have been more or less benefited; the greatest number of the patients, who previously walked with some difficulty, experienced at least one or two attacks every year, and suffered almost constantly, have either had no return of gout during two years, or their attacks have been fewer, shorter, and less frequent: in all, the general health was improved, and they regained the strength, flesh, and cheerfulness they had lost for some time; and, finally, notwithstanding the disappearance of the gout, they have not experienced any consecutive disorder. The remaining ten patients suffered from symptoms which some of the physicians attributed to the alkaline treatment and others considered as independent of it. On this account we have read carefully the ten cases, in order to form our own opinion from them alone. Three of these (72, 73, 80,) are obviously inconclusive either way. In 3 the waters produced intestinal irritation, marked by indigestion (71,) diarrhœa (72,) and hemorrhage from the bowels; but one of these patients was a hard drinker, and the chronic diarrhœa produced by the waters was decidedly beneficial, whilst the other 2 drank the waters in immoderate quantities. In the 4 remaining cases the waters seemed decidedly to have disagreed. In one they produced (to use the patient's own words) a flow of blood to the head, and this repeatedly, so that he discontinued them. The reporter seems dissatisfied with the expression,—a proof of the strength of his own cerebral circulation. One of the most frequent primary effects of even the purest mineral waters is a sensation of fulness of the head and slight vertigo, particularly in the debilitated or the highly nervous. In the second case (a lawyer of sedentary habits and delicate constitution), these waters seem to have repeatedly produced severe headaches. In the third (a bad case of rheumatic gout, of long standing), they produced great debility; and in the fourth (a man of apoplectic make, of soft fibre, and great nervous susceptibility,) they seemed to have aggravated his gouty symptoms, which terminated eventually in apoplexy.

The reporter concludes with an opinion, that the number of cases are sufficient to prove that the waters of Vichy, followed up with alkalis, may be used with success in articular gout affecting the joints, and that they are preferable to other antiarthritic remedies, both from the facility of their administration and the little inconvenience which attends their employment. The 10 unsuccessful cases do not, we admit with the reporter, militate against this opinion; but they show that the effects of these waters should be carefully watched, and that the patients should not indulge in very large quantities to gratify their own tastes or views, as the water certainly exerts a powerful influence on the economy.

We shall quote a few of M. Petit's remarks, as they illustrate the action of this class of waters. The alkaline waters and baths of Vichy diminish the secretions from mucous membranes. Constipation is a frequent effect; chronic catarrhal discharges, chronic diarrhœa, and chronic catarrhus vesicæ (when no organic cause—as stone—exists,) are diminished, their character is changed, and they become natural. "Je ne sais que dire de l'influence des eaux de Vichy sur la *génération*. A Vichy, comme à presques toutes les eaux minérales, il vient de temps en temps des femmes avec la seule pensée d'y acquérir la faculté de devenir mères. Chez quelques unes, cela a réussi; chez la plupart, cela n'a rien fait. Les eaux y sont-elles pour quelque chose? Je l'ignore." (p. 168.) M. Petit considers that a purely vegetable diet is the most rational one in gout, but as few will submit to it, it is useless to order it. He does not forbid meat, but recommends its moderate use, and the avoidance of the stronger and more exciting meats, as well as dishes prepared with spices and vinegar, and all acid fruits. He attaches most importance to the almost constant use of alkaline drinks, temperance in all things, and abstinence from wine, (at least from acidulated wine,) liqueurs, and irritating drinks. Gouty inflammation of an internal organ is a contra-indication to the baths, but not inflammation of the joints. M. Petit never applies leeches; they diminish pain for the time, but render convalescence tedious.

Many more cases of acute than of chronic gout came to Vichy, and, if there is any difference, the waters succeed better in the acute form; in the chronic the cure is slower. Those with erratic, anormal, irregular, or visceral gout (often the cause of many anomalous, painful, and distressing symptoms,) have generally derived much advantage from these waters, with a suitable diet and alkaline drinks. Under the head, "Gout," (p. 333,) we have alluded to the symptoms which M. Petit considers as indicative of this form of the disease.

The Vichy waters are contra-indicated in phthisis, and in diseases of the heart of any standing; in all cases of hemoptysis and in dropsy, not confounding this, however, with the œdema of gout. The rheumatic and neuralgic do not frequent Vichy.

Mont-d'Or, in France, is another hot alkaline spa in great reputation. It was celebrated for the cure of phthisis, but the greater exactness of modern pathology has proved that the cases in which it is really efficacious are those of chronic pulmonary catarrh, or chronic pneumonia, and it seems to act by producing a vigorous determination of blood to the skin, and consequently copious perspirations. M. Bertrand, the resident physician, directs the treatment, often watching the effect thermometer in hand, feeling assured that he shall relieve the disease if he can efficiently stimulate the surface. Hæmoptysis does not even deter him, if the patient is not of an irritable habit of body. Indeed all the cases which are most benefited are those in which there is torpidity rather than irritability.

Ems, in Nassau, is a bathing place much frequented by the English. Its springs are of warm alkaline water; their temperature from 83° to 115°. They contain principally bicarbonate of soda and carbonic acid gas, but they are not simply alkaline, as they also hold in solution some common salt. Ems is situated in a narrow valley with little shade, so that in summer it is a hot and relaxing place. The baths stimulate the

skin and relax the nervous system. Their chief reputation is for bronchial affections. Sir James Clark recommends them in bronchial diseases affecting those whose constitutions are very delicate, where both the bronchial and abdominal mucous surfaces are in a state of irritation and congestion, or when the functions of the uterus are defective. The Ems waters may then be ordered as a short preparative to the more exciting springs of Carlsbad, Kissingen, and Marienbad. The situation as well as the somewhat relaxing effect of the waters are contra-indications in those affections where there is much relaxation of the system, and in persons who are better in cold bracing weather, or in dry exposed situations. "Ems," says Dr. Granville, "can never suit a hypochondriac, no matter from what functional disorder his unhappy state may arise. It never can suit such females as have suffered from repeated losses, have become blanched, weak, and shaky, and whose head feels heavy yet light, composed yet empty, always on the *qui vive*, yet oppressed."

Töplitz is a hot alkaline bathing spa, frequented by the gouty, rheumatic, and crippled. The temperature of the waters is from 114° to 122°, and they contain bicarbonate of soda and a very small quantity of carbonate of iron, enough to stain the baths. Carbonic acid gas is in very small quantities, and in this respect these waters differ considerably from those of Vichy and Ems. Töplitz is about sixty miles from Carlsbad, is frequented by the haut-ton of Germany, and is the cheapest watering place Dr. Granville ever visited.

There are two other springs much frequented, which are warm and very slightly alkaline, Schlangenbad and Wildbad.

Schlangenbad, in Nassau, is a slightly alkaline warm water, the temperature about 86°, containing a little carbonate of soda with some carbonic acid, giving a softness to the skin and a pleasant stimulus to the whole body. It was brought into fashion here by the celebrated "Bubbles from the Brunnens of Nassau."

Wildbad, which is in the neighbourhood, is a somewhat similar thermal water, its temperature from 88° to 99°. It contains a very small quantity of solid ingredients, of which muriate of soda is in the largest proportion; but it contains much more carbonic acid gas than the Schlangenbad.

The effect of these two baths seems to be to tranquillize the nervous system, and they are frequented by the gouty, rheumatic, and paralytic. Where, however, these diseases are connected with irritable dyspepsia, congestion of the gastro-intestinal mucous membrane, or congestion of the abdominal organs generally, these waters cannot be expected to produce much benefit.

E. *Simple unmineralized hot waters.* There is another class of hot and warm waters, whose efficacy is acknowledged but unexplained by their chemical composition, unless their virtue depends, as it may do, on their extreme purity and freedom from any foreign matter, together with their heat. They are purer waters than those commonly used as drink. Of this kind are the waters of Gastein and Pfeffers on the continent, and the Bristol Hot Wells, Buxton, Malvern, and others in England.

Pfeffers. In this place, situated in the Swiss Alps, (vividly described by Dr. Johnson,) are very pure waters, the temperature

about 100°. "They have neither taste, smell, nor colour. They will keep for ten years, without depositing a sediment or losing their transparency." For six centuries they have been resorted to by invalids—those affected with rheumatism, neuralgia, glandular swellings, and skin diseases—not by the merely fashionable spa frequenters, and their reputation is still great. "In many they produce slight vertigo, in more they act freely on the bowels."

GASTEIN, in Austria, among the Noric Alps, (that chain stretching between Munich and Vienna,) is another extremely pure hot water. Its temperature is about 118°, and it is unusually free from even the ordinary saline matters. It is rarely visited by the English. The fine mountainous situation of both these places must, without doubt, have much to do with the marvellous curative power they are said to possess over diseases of debility.

BUXTON is in the wilder and more mountainous part of Derbyshire. Its water is warm and pure, temperature 82°, and the solid matter such as is found in every common spring. As a bath, the slight shock it produces is followed by a highly soothing and pleasurable glow over the whole body. Persons of delicate and irritable habits of body, and with parts weakened by disease, can generally bear this degree of cold, and overcome it by a very small reaction; to produce which appears to be a most salutary effort of the constitution. The cases most benefited are those in which a loss of action and sometimes even of perfect sensation has come on in particular limbs, owing to long or violent inflammation or external injury, where the just measure of action is past. Thus chronic rheumatism succeeding acute, especially in moving parts, is often wonderfully relieved. (Saunders.)

BRISTOL. The hot-well (so called) at Clifton is a fine clear tepid water, temperature 74°, containing no other solid matter than is found in almost all common springs, and in less quantities; but it is very agreeable to the palate, from its being slightly impregnated with carbonic acid gas. Its continued use, as might be expected, increases the flow of urine, and keeps the skin moist. It is but little employed now, but formerly it was greatly celebrated in consumption; and, as Dr. Saunders well observed, it might alleviate some of the most harassing symptoms of that disease, by moderating the thirst, the dry burning heat of the hands and feet, the partial night sweats, and the symptoms that are peculiarly pectoral. (Saunders, p. 125.)

MATLOCK, in Derbyshire, is a pure tepid water, temperature 66°. As a tepid bath it produces little shock, and is fitted for those delicate and languid habits that cannot exert sufficient reaction after an ordinary cold bath, which consequently produces headach, and lassitude. These baths may be employed in preparing the invalid or the delicate for the sea. The spot is strikingly beautiful.

F. *Simple mineralized hot waters.* There is still another class of warm waters, which have an undoubted effect on the system, and which chemistry does not explain: unlike those which we have mentioned they neither contain principles whose action upon the body is undeniable, nor are they of extraordinary purity. They contain various saline matters, which medically we consider of but little potency, but yet their action on the body is well marked. Such are the waters of

Bath, many of the warm waters of the Pyrenees, and of France, generally. Of course their heat alone will explain much of their effects, but not all; still their effects cannot be doubted.

Bath. This is the hottest spring in England, its temperature being 112° to 116°. Its solid ingredients are considerable, but chiefly consisting of the calcareous salts: sulphate of lime is the principal one, which seems to exert no very marked effect on the secretions. These render it a very hard water, unfit for domestic purposes. There is a very small quantity of iron, which is deposited quickly, as the water cools. When taken internally the Bath water quickens the pulse, produces a glow in the stomach, increases the heat of the body, and promotes the flow of urine. As a bath it stimulates the skin, and the whole body, and produces copious perspiration. Some years ago it was very generally recommended in cases where various anomalous symptoms of the head, stomach, and bowels were thought to depend on latent gout. The warmth of the baths often brought the disease to a crisis, by favouring its outward development, bringing on an acute attack (a fit) of gout in a joint. From its stimulating nature, and its tendency to produce constipation, it is contra-indicated in the plethoric and highly irritable habit of body; also whenever there is a disposition to inflammation, or where a quick pulse and dry tongue indicate a feverish disposition. In some chronic diseases affecting those of a torpid, phlegmatic or bilious temperament, such as gout, rheumatism, sciatica, lumbago, and various forms of palsy, and particularly in paralysis in the arms in painters from white lead, it is remarkably efficacious. In chlorosis the bath seems to quicken the languid circulation, and to stimulate the body very favorably; the small quantity of iron which the waters contain is, in such cases, of course, beneficial. Where gout, as so often happens, is connected with congestion of the mucous membrane of the stomach and bowels, with congestion of the liver and of the abdominal venous circulation, as in those who live very freely, who grow fat and large and full, and look high-coloured and red, or else unnaturally sallow, a water which acts on the skin and kidneys without increasing the abdominal secretions, cannot be very efficacious, and may, from its stimulating the whole system, be injurious. In such cases the waters of Cheltenham or Leamington are still better; the more active ones of Germany promise still more success.

Springs of the Pyrenees. In the Pyrenees, besides the sulphurous hot waters which we have before noticed, there are various warm and tepid saline springs much resorted to by the French: they vary in temperature from 80° to 122°, and contain the ordinary saline matters in but small quantities, so that their virtue must depend on their heat, and on the peculiar combination of their chemical ingredients. Mr. Carmichael visited *Bagnères de Bigorre* and found that, in his own case, by the use of the baths his appetite improved, that a painful feeling of distension after even slight repasts, which before existed, was removed, and that the secretions from his bowels and kidneys became more natural.

There are here also *Cauterets*, a very fashionable spa for Parisians, *St. Sauveur*, *Eaux Bonnes*, and *Chaudes*: they are all more or less celebrated in rheumatic and paralytic and cutaneous diseases; but little is known of their chemical composition, as the negligence and want of energy of French men of science, as far as regards their own mineral waters, are

remarkable. One physician, who was sent there by government, gravely assures us that the analysis of all the springs of one spot (Bagneres) would occupy a laborious chemist for four or five years.—The scenery of the Pyrenees is of the finest kind. The spas are rather expensive places, as they are much crowded for a short time. Many of them are situated in valleys, or in other spots of rare beauty. Ten years ago Mr. Carmichael thought they were but little visited by the English, and so they are, comparatively, still; but travellers are directing their steps there much more frequently. Pau is now quite an English colony, and English habits and customs, as well as countenances, are known at the posting houses, inns, and hotels in that district of southern France. What amount of the supposed benefit derived from these baths depends on the fine, dry, elastic mountain air of this district, cannot be exactly determined, but that the climate has a great share in restoring the strength and spirits of the debilitated and nervous, we should judge from the experience of Mrs. Ellis, who resided fifteen months in that part of France, living at Pau in the winter, and among the Pyrenees in the summer, and who has given an account of her impressions of the place: "I do not recollect," she says, "once to have felt, during my whole residence in the south of France, that causeless and indescribable dejection of mind which most of the inhabitants of England at times experience, and with which no one is, perhaps, more intimately acquainted than myself: a want of elasticity and animal vigour from a sort of sinking of the soul which often makes the dawn of each successive morning appear like a renewal of a helpless conflict; every unexpected event a fresh hinderance to the course we have to pursue; and every necessary exertion an insupportable trial. In the south of France, supposing the mind to be free from the pressure of actual calamity, there is an effect produced by the clearness of the atmosphere, the brightness of the sunshine, and the elasticity of the air, which makes the mere animal sensation of being alive—of breathing and moving—a perpetual enjoyment."

Making all due allowances for the florid style of the lady, and for that love of sympathy which leads those of such tender organizations rather to overstate those feelings which call forth the pity of others than to confine themselves within that strict line which divides truth from fiction, yet, on the whole, we believe there is very much truth in these observations, and they strikingly show how many of the effects which, supposing the individual to have been undergoing a course of mineral waters in the same situation, would have been attributed to the waters rather than to the climate. Mrs. Ellis, however, it must be remembered, was not an actual invalid, but perhaps owed many of her uncomfortable sensations to her pen: whilst enlightening the ladies of England she was, perhaps, wasting her own *pabulum vitæ*; and whilst setting them to rights was introducing disorder and confusion into her own nervous system.

G. *Chalybeate Waters.* Iron is the principal medicinal ingredient of the waters of this class; it enters into the composition of most of the saline waters, but in so small a quantity as to be a useful, but not a predominating, principle. Thus, both in the saline springs of Carlsbad and Marienbad, there is iron, but their marked action is in increasing the secretions from the intestines and kidneys. Under the present head, we shall therefore mention only those springs which are direct tonics. These

are Spa, Pyrmont, Schwalbach, and Marienbad, on the continent, and Tunbridge Wells in England.

Spa is a cold chalybeate water, with an abundance of carbonic acid gas, rendering it an agreeable beverage. The town of Spa is thirty miles south of Aix-la-Chapelle, and it may be reached the third day from London. It is a beautifully-situated place, more resorted to for pleasure than for health, and is provided with gambling houses. It was much frequented at one time, but two causes have thinned it: the "Bubbles of the Brunnen" sent the English to Nassau; and the separation of Belgium from Holland putting it out of fashion in the latter country, the Dutch have joined the English at Schwalbach.

Schwalbach, in Nassau, is a cold chalybeate charged with carbonic acid, rendering it a very agreeable drink.

Pyrmont, in Westphalia, is a strong chalybeate with a very large proportion of carbonic acid, and besides is a very hard water with much of the earthy carbonates. It has been thought to be "considerably rougher in its operation, and more active, than the water of Spa; and hence Hoffman concludes that it is peculiarly well fitted for the use of the Westphalians "who are in general of a robust constitution, and live upon hard strong food." It is certain that whatever effects are produced on delicate stomachs by a hard water, may be here apprehended from the large proportion of earthy salts.

All these waters highly charged with carbonic acid, produce a slight temporary feeling of intoxication. At Pyrmont, the country people flock to the springs, and many of them to enjoy this effect of the waters. For those whose stomachs are easily disordered, and are of great delicacy of constitution, Schwalbach would be a preferable tonic from its great facility of digestion. It contains more carbonic acid than the waters of Pyrmont. The waters of Spa are not hard like those of Pyrmont.

Marienbad. Two of the springs, the Carolinen-brunnen and the Ambrosius-brunnen, are highly-carbonated chalybeates, the former very analogous to the chalybeate of Schwalbach. Its distance would, therefore, prevent the English, who can get a similar water so much nearer, from visiting Marienbad simply for its chalybeate waters.

Bruckenau and Bocklet, near Kissingen in Bavaria, are both highly-carbonated chalybeates. Dr. Granville says, the Bruckenauer is considered to be "the clearest and most spirituous of all the known carbonated chalybeate springs." They are conveniently situated for those patients who, after taking the active salt waters of Kissingen, require a tonic to complete their cure.

A tonic water is often advantageous after a saline, and it so happens that, at many of the saline spas, there are chalybeate springs either at the same spot or in the neighbourhood. Bruckenau and Bocklet are in the proximity of Kissingen. At Marienbad, there are direct chalybeate springs, as well as saline aperients. Those who have used the saline waters of Carlsbad can easily take a chalybeate course at Marienbad or Franzensbad, which are both in the neighbourhood; and patients who have undergone a course of bathing at Ems or Wiesbaden, can fortify themselves by the pleasant chalybeate of Schwalbach.

Furnas. The only *hot* chalybeate spring we are aware of is in the valley of the Furnas in St. Michael's, one of the islands of the Azores. It is

used as a bath by the Azoreans. It is a clear, sparkling, hot water; the carbonic acid which it contains holds the iron in perfect solution, but it is deposited in cooling in a thick bright orange crust. In the same valley are hot springs slightly alkaline, of the heat of boiling water, and a cold highly-carbonated chalybeate, like that of Schwalbach in Nassau. (Bullar's Azores.)

TUNBRIDGE WELLS. The water of Tunbridge is a very simple one as to the nature of its solid contents, having a small quantity of iron held in solution by carbonic acid. The quantity of carbonic acid, as well as iron, is very small in comparison with the German chalybeates of Spa, Pyrmont, Schwalbach, or Marienbad; in certain classes of complaints this is an advantage.

NITON. At the back of the Isle of Wight, near the village of Niton, there is a strong chalybeate spring containing a large quantity of iron and alum. It is one of the most powerful chalybeates known, but has not attracted much general notice.

Besides these there are, in very many parts of England, chalybeate waters of various kinds, the knowledge and use of which are completely local. Dr. Granville has described several spas in Yorkshire and elsewhere, which are resorted to by those living in their vicinity, and to his book we must refer those who are locally interested in them.

II. *Waters containing Iodine and Bromine.* Sea water, as is well known, contains hydriodic and hydro-muriatic acids in small quantities. Traces of iodine have been also detected in the chalybeate of Schwalbach, and of bromine in the springs of Kissingen and Wiesbaden. Recently, however, both iodine and bromine have been ascertained to exist in some German springs in quantities which are considered sufficient to render them powerful agents in the cure of scrofula.

CREUZNACH, on the river Nahe, which joins the Rhine at Bürgen, not far from Mayence, is the iodine spa of Germany. Salt for many years has been made in the immediate neighbourhood from salt springs, and the principal ingredients in the water of Creuznach are common salt and muriate of lime. The iodine and bromine are in very small quantities in the water which is drunk; thus five or six tumblers, or a pound, of the water contain only 0·27 grains of bromide of magnesia, and 0·003 grains of iodide of magnesia, but at the neighbouring salt-works the bittern (the fluid remaining after the common salt has crystallized) is more highly impregnated with iodine. In one analysis nearly five grains of iodine were found in sixteen ounces of bittern; and in another, twenty-six grains of iodide of soda existed in the same quantity of bittern. This bittern is added to the baths. If used at first in any quantity it produces irritation and eruptions on the skin, but when added gradually in the greater number of cases it produces after some length of time, eruptions, boils, or livid spots, with relief. The water of Creuznach if taken in any quantity, increases the number of stools. It is resorted to for all forms of scrofula; and, says Vetter, it will be celebrated as long as "glands and lymphatics continue to be affected, mucous membranes to be diseased, and glandular textures to be obliterated or infiltrated with pure albumen."

At HALL in Upper Austria, the water is more strongly impregnated with iodine, "according to some as much as 5·53 grains of it in sixteen

ounces of water, though Fuchs states it to be but a third or a fourth part of that quantity." This *for centuries* has been resorted to by the scrofulous. Latterly however it has been deserted for

Ischl, in Upper Austria, the situation of which is extremely beautiful. The bittern contains a considerable quantity of brome. Steam-baths are constructed above the salt-works and are said to be very efficacious helps to the cure.

To Woodhall, an iodine spa in England, allusion has already been made.

Rules for the Drinking of Mineral Waters. There are certain general rules common to most spas as to the quantity, time, and season, in which their waters ought to be taken; but as there are exceptions, the patient should comply with the customs of the spa which he frequents.

The water is taken before breakfast; occasionally the dose is repeated in the afternoon. The most favorable time of the year is thought to be from May to September. The ordinary length of the course is from a month to six weeks; but two or more months may be necessary, and perseverance for many successive years. It would be inconsistent to expect that a chronic disease, depending upon causes which had been in operation for years, and which had disarranged the whole system, and perhaps disorganized some important organ, could be cured by any treatment of short duration. Abernethy's aphorism, "a chronic remedy for a chronic disease," should in all such cases be borne in mind by the medical practitioner, and duly impressed by him on his patient. The following judicious observations by Marcard we quote, as generally applicable to the treatment of all diseases of long standing, as well as from its special application to the present question. He is discussing the treatment of chronic diseases of the abdominal viscera. "Whatever are the remedies chosen to remove obstructions, whether saline salts of various kinds, or whey or vegetable juices, or soft and attenuating drinks, or the mineral waters of Carlsbad, Ems, Pfeffers, and similar ones of the same deobstruent character; or lavements, often so efficacious; or finally tepid baths; whatever remedy has been selected with judgment, it should be used a long time, and assiduously. Neither days nor weeks are sufficient; months are necessary, and when the diseases are inveterate, the same remedy should be persevered in for years."*

The quantity of water, and the length of the course, must be often regulated by the effects produced. If the water cures by evacuations from the bowels, the stools should be constantly examined, for the same reason that every careful physician makes it a point (or should do so), to examine the stools of a patient undergoing a course of aperient medicine, in order to regulate the quantity of the remedy and the duration of the treatment by the effects produced on these secretions. But in many patients (and this effect has been observed from mineral waters of all kinds), after the water or baths have been taken and used during two or three weeks without any very visible effect, a febrile state is set up, attended with uneasiness, hot and dry skin, bitter mouth, disturbed nights, a series of symptoms called the "crisis," the "bath-storm," the "point of

* Description de Pyrmont, traduite de l'Allemand de Marcard, tom. ii. p. 39.

saturation" (the body being then supposed to be saturated with the water), and the theory is that nature is exerting herself to throw off the disease. It seems agreed by all that such symptoms are, on the whole, favorable, and that they are often followed by copious evacuations, and with great relief to the disease. But when these symptoms occur, the patient should either diminish the quantity of water, or discontinue it. The only safe way, however, seems to be to consult the bath-physician.

The glasses used in Germany contain five or six ounces of water: the patient begins by drinking two or three glasses at a quarter of an hour's interval, and increases the number gradually, according as he can digest it or according to the effect it produces. If the first glass disagrees with the stomach, no more should be taken on that day; if cold water disagrees, it may be warmed. Persons of delicate habits are often obliged to take the water at first warm, and gradually to diminish its temperature; sometimes it is necessary to dilute the water with milk, whey, gum, or rice water. Exercise after drinking the water is beneficial by promoting perspiration. Dr. Bertrand (of Mont d'Or), cautions his patients against taking a second course in the same season. He has observed, like others, that the benefit is often felt some time after the water has been discontinued, and he thinks that this secret and beneficial action of the economy may be interfered with, and prevented by a second course.

Injury is constantly done by drinking mineral waters in excessive quantities; the poor especially often think they cannot drink too much. At Vichy, M. Petit begins by giving patients who are very susceptible two or three glasses of three ounces each daily; but in inveterate cases of chronic disease, he gradually increases the quantity to ten, twelve, or even fifteen glasses daily. Many gouty patients have taken as much as twenty or twenty-five glasses a day; and one case is mentioned in which the patient, of his own accord, is said to have drunk as much as eighty-four glasses in twenty-four hours! (Rapport, page 180.)

We had intended to have described the mud and gas baths, and have entered into some details as to the rules for using baths in general, deduced from experience at the German spas. We must, however, defer this until another time, and shall conclude with a brief notice of the diet which is recommended to those who visit these watering places. A table of diet is given in each book describing the medical virtues of a spa. The one which we select as most accordant with our own practical views of diet in ordinary chronic diseases, is that furnished by Professor Von Ammon of Dresden, and published by Dr. Granville.

Drinks. Strong and new beer, all heating wines and liqueurs, mulled wine and punch are forbidden; the Germans also disapprove of tea and lemonade.

Cocoa, small quantities of coffee and chocolate, milk, but not as a usual drink, table-beer, barley-water, sugared water, and negus, at table are admissible.

Vegetables. Asparagus, peas, spinach, young beans, and cauliflowers when young, in small quantities, are allowed; also wood-strawberries in moderation.

Potatoes, all new fruits, (as apples, apricots, gooseberries, melons, medlars, quinces, raspberries, pears, and plums,) all green salads, cucum-

bers, radishes, sorrel, as well as cabbages, carrots, onions, mushrooms, and truffles, are interdicted.

Of farinaceous food. Bread and oatmeal groats are allowed, but all pastry is forbidden.

Fish. Tender fish is allowed; salmon, eels, lamprey, cray-fish, red and pickled herrings, and anchovies, are forbidden.

Poultry and Game. Chicken and fowls, hare, partridges, pigeons, venison, and eggs, may be eaten, but not ducks or geese.

Meat. Every kind of meat, except pork and salted and smoked meats, is permitted; all animal fat is interdicted. Cheese, spices, and all ices, are forbidden.

The ordinary dietetic precepts, of great moderation as to quantity, eating no more than can be borne without a sensation of distension and oppression, abstaining totally in the intervals between meals, resting completely for at least half an hour after each meal, are to be observed, and the patient should be warned to abstain carefully from any article of diet, (although it may be found among permitted food,) that has formerly disagreed with him. Coffee, according to Dr. Heidler, is found to be particularly injurious to those who have abdominal congestions, particularly in those of an inflammatory habit, producing in such, uneasiness, palpitation, oppression, trembling of the hands, &c. Tea, says the same physician, without exception is injurious to all persons whose nerves are feeble, more or less according to its quality, quantity and the habits of taking it. Water is the best common drink, then well-fermented but not strong beer, or water mixed with a pure and not an acid wine; or such wine may be taken undiluted, if the patient has been accustomed to it, and is not heated by it.

Daily and moderate exercise according to the strength, without producing great heat or fatigue. The patient should retire to bed at ten, and rise at five or six; six or seven hours' sleep is the usual time required; too much sleep weakens the body and the mind. Sleeping after dinner as it tends to produce congestion of the head and chest, should be forbidden to those disposed to apoplexy and hydrothorax.

In concluding an article which has been extended considerably beyond our intentions when commencing it, we must observe that the action of mineral waters upon the body in various forms of disease, is a subject well worthy the attention of the medical practitioner, both from its affording a chance of cure in many chronic complaints, and from its guiding him in the choice and adaptation of remedies. Its study is calculated to give comprehensive and satisfactory views of the common origin of a large number of diseases, and to lead to their judicious treatment from its bringing most prominently forward the paramount necessity of attending with minute care to the diet, exercise, and habits of life, of all patients who are submitted, for the cure of complaints of long standing, to a course of medicine.

ART. II.

Ueber Darm-Anhangs-Brüche (Herniæ Litricæ). Mit Bemerkungen über Kothfisteln und Widernatürlichen After. Von Dr. C. F. RIECKE.—*Berlin*, 1841. 8vo, pp. 192.

On Hernia e diverticulo Intestini (Hernia Litrica), with Remarks on Fæcal Fistula and Artificial Anus. By Dr. C. F. RIECKE.—*Berlin*, 1841.

FEW diseases are of greater interest and importance than herniæ of the intestinal canal, and few, if any, have been more ably and successfully investigated; their pathological anatomy especially has been so minutely and diligently examined that we might almost suppose this department of the subject was exhausted. Dr. Riecke, however, in the treatise which forms the object of this notice, directs attention to a variety of hernia hitherto overlooked, though, according to him, it is of frequent occurrence and presents peculiarities of importance in a practical point of view.

In the *Mémoires de l'Académie des Sciences* for the year 1700, LITRE describes two cases of inguinal hernia in which the proper caliber of the intestinal canal was not included in the hernial sac, the protruded portion of intestine, consisting of a digital prolongation of the ilium, which Litre concluded was formed by the gradual extension of a knuckle of the bowel that had become engaged in the inguinal canal. Litre's observations have, on the whole, attracted little attention, and been inaccurately understood. It appears obvious that his cases were examples of hernia of a diverticulum of the intestine, and to such cases should the appellation *hernia Litrica*, adopted by some authors, be confined. The term, however, has been so loosely applied that its real import has been usually quite lost sight of. Thus, for example, Rust, in his *Handbuch der Chirurgie*, comprises under the names *hernia lateralis* and *hernia Litrica* every small hernia, in which but a portion of the caliber of a hollow viscus, whether stomach, intestine, or urinary bladder, is contained in the hernial sac. To avoid such confusion of terms Dr. Riecke designates the *hernia Litrica* by the name *hernia e diverticulo intestini*, and calls that form of hernia in which but a portion of the cylinder of the intestine is prolapsed *enterocele partialis*. Having premised thus much we shall lay before our readers a full analysis of Dr. Riecke's work.

The first chapter is little more than a transcript of Meckel's account of diverticula of the intestinal canal, of which it will be recollected there are two kinds. The first is the *diverticulum congenitum*, supposed to originate from the more or less perfect continuance of the connexion of the small intestine with the umbilical vesicle, and in the composition of which all the tunics of the intestine are concerned. The second species is the false diverticulum (diverticulum spurium seu acquisitum) which is formed by a protrusion of the mucous membrane of the intestine through its muscular coat, and therefore consists of its mucous and peritoneal tunics alone.

Both these species of intestinal diverticulum may become engaged in hernia; but the *diverticulum acquisitum* exclusively constitutes the *hernia e diverticulo intestini* of our author. Dr. Riecke enumerates several of the recorded cases in which a *diverticulum congenitum* formed

the contents of a hernia, but such cases form a distinct class from those constituting the proper object of his work, and are not further adverted to.

The history of the *hernia e diverticulo intestini* is given in very lengthened detail in the second chapter. According to Dr. Riecke, this description of hernia occurs, perhaps exclusively, in the femoral canal, at least he knows no example of its occupying any other situation. Females are much more liable to it than males, but our author has once met with it in the latter sex. The etiology of the affection is the same as that of hernia generally; the diaphragm and abdominal muscles, especially during any unusual bodily effort, exert a pressure on the abdominal contents, and if that pressure exceeds the resistance offered at any particular point, say the femoral ring, the intestine finds exit, or its muscular fibres separate and allow the mucous membrane to protrude and push before it the peritoneal covering of the bowel; the *diverticulum acquisitum* being thus formed enters the femoral ring, and constitutes the first stage of the *hernia e diverticulo intestini.* The unyielding nature of the boundaries of the femoral ring explains the greater frequency of the affection in that situation; the descent of the intestine *en masse* being opposed, though the passage of the smaller diverticulum is permitted; while the more distensible inguinal ring readily allows the entrance of the entire thickness of the intestine.

The *hernia e diverticulo intestini* is invariably of slow formation; when it has augmented to a certain bulk, to such an extent for example as to constitute an external tumour, it produces changes in the condition of the intestine itself, to which Dr. Riecke attaches much importance, and which he describes with minuteness, not to say prolixity: they may be thus summed up. The descent of the *diverticulum* in the femoral canal necessarily draws the intestine to which it is attached into close contact with the crural ring, and thus alters the line of direction of the bowel, which forms an angle on itself, the apex of the angle being at the point of origin of the diverticulum, and consequently corresponding to the crural ring. This disposition of the intestine (quite similar, we need scarcely say, to what obtains in ordinary intestinal hernia) coupled with the fact, that a portion of its tissues is engaged in the formation of the *diverticulum* causes a diminution of the caliber of its canal at the point whence the *diverticulum* is given off, which diminution of the internal capacity of the intestine Dr. Riecke, to avoid circumlocution, calls the *stricture.* The intestine being thus (especially when the diverticulum has become adherent to the hernial sac,) fixed at the upper opening of the crural canal, the peristaltic motion is interrupted at the fixed point, a circumstance which, together with the existence of the *stricture* and the altered direction of the bowel at the same locality, materially impedes the propulsion of the fæces, causes their accumulation above the stricture, and thus gradually produces a dilatation of the upper portion of the gut, termed by Dr. Riecke, for shortness' sake, the sinus. These several conditions of the parts constantly tend to augment under the prolonged operation of their exciting causes. Below the stricture the capacity of the intestinal canal becomes rather diminished, the more so, the narrower is the stricture, and the greater the dilatation of the *sinus.* The pathological alterations are accompanied by habitual costiveness, liability to colic, and the various and varying dyspeptic symptoms almost con-

stantly experienced by patients labouring under unreduced hernia of any kind or in any situation; but which Dr. Riecke seems to regard as, in some degree at least, peculiar to and characteristic of the particular kind of hernia under consideration. The hernial tumour itself is rarely the seat of any pain or uneasy sensation. When of long standing and of a certain size, the diverticulum communicates with the cavity of the intestine by so narrow a neck, that the liquid and gaseous contents alone of the intestine pass into it, and if any solid material appears to find entrance, its presence suffices to excite inflammation accompanied with symptoms of strangulation.

The *causes and symptoms of strangulation* are next discussed. Dr. Riecke adheres to the phrase *strangulation* as applied to this description of hernia, although he thinks the obstruction is caused by a mechanism very different from that which obtains in other herniæ; inasmuch as in ordinary cases the causes of obstruction exist in the hernia itself or in the rings or canals through which it passes, while in the *hernia e diverticulo intestini* they lie in the stricture and sinus of the intestine.

The most frequent cause of strangulation is an unusual protrusion of the diverticulum, which either causes the descent of the stricture into the crural canal, or merely draws it so firmly against the crural ring that the intestine is flattened and its cavity obliterated. A similar effect may result from the inordinate distension of the diverticulum by gaseous liquid, or solid matters, or, if the diverticulum be adherent to the sac, the same consequences may be produced by inordinate gaseous distension of a coil of intestine lying beneath the neck of the diverticulum and the intestine whence it originates, as the mesentery, stricture and diverticulum may be thus put forcibly on the stretch over the distended intestine. A case is given (pp. 104-10,) as an example of strangulation from this latter cause, but it does not establish the point, as on dissection the diverticulum was found, not engaged in the femoral canal, but loose and floating in the cavity of the abdomen. The entrance into the diverticulum of solid matter, as fæces, or a foreign body, may, it is stated, excite inflammation with symptoms of strangulation; and a case (p. 165) is given as illustrative of such an event, which, it is easy to understand, might readily occur; it is not however superfluous here to observe that the case just alluded to is simply an example of inflammation and suppuration of a femoral hernia which opened as an abscess, and gave exit to a needle, after which recovery rapidly ensued; but there is not a particle of evidence to show that the hernia was of that particular kind, designated by our author as the *hernia e diverticulo intestini.* In one case (p. 90,) where Dr. Riecke found on dissection all the anatomical characters above mentioned as distinctive of the *hernia e diverticulo*, the obstruction and symptoms of strangulation resulted from a partial invagination of the intestine, the *sinus* having sunk into the stricture and stopped it, to use the author's phrase, like a plug.

Dr. Riecke describes the symptoms of strangulation at very great length, but we need only mention those which he considers as peculiar to, or at least as particularly characteristic of, the *hernia e diverticulo*, No pain or uneasy sensation is at first experienced in the hernial tumour, and from six to eight days may elapse before it even attracts the attention of the patient. If however the surgeon detects the existence of the

hernia, and practises too frequent and forcible attempts at reduction, the tumour soon becomes tender and inflamed. The pain is at first chiefly referred to the situation of the sinus, which becomes very tender on pressure, and so distended that in thin patients its outline can be felt through the abdominal parietes; general abdominal inflammation and febrile reaction do not set in for several days; throughout the entire course of the affection the progress of the symptoms is unusually slow and attended with marked intermissions which may deceive both the patient and the physician, (pp. 26-37.) If the diverticulum be returned, whether by the taxis or an operation, subsidence of the symptoms of strangulation and evacuation of the bowels follow much more slowly than in other herniæ, often not for many hours or even some days; because the existence of the stricture and the inflamed condition of the sinus of the intestine suffice to keep up the symptoms. If the hernia opens externally the resulting artificial anus usually heals rapidly.

On dissection, the *sinus* is found to be the chief seat of the inflammation, and is, not unfrequently, gangrened and perforated. The intestine above the stricture is greatly distended and inflamed generally; but below the stricture the intestine is contracted, and inflamed to but a small extent; circumstances, we may remark, constantly observed after fatal strangulated hernia, and by no means peculiar, as Dr. Riecke seems to imply, to the form of the affection under consideration. The *diverticulum* itself is inflamed, but less so than the sinus, and is seldom gangrened, unless attempts at reduction by the taxis have been injudiciously persevered in. (p. 31.)

The diagnosis of *hernia e diverticulo intestini* is the next point considered, and, according to the fashion of our author, few things are said in many words. The characters assigned as distinctive are, the gradual formation and smallness of the tumour which, except in cases of very long standing, rarely exceeds the size of a walnut; it being of a roundish shape, slightly elastic, usually smooth, though occasionally somewhat irregular on the surface, and sometimes hard, so as to resemble and have been mistaken for a lymphatic gland; and, finally, if the hernia be reducible, its return is accompanied by a peculiar gurgle rather felt than heard. We need hardly remind our readers that the foregoing statement is but an imperfect summary of the physical characters commonly ascribed to femoral hernia, and obviously cannot be considered as diagnostic of the *hernia e diverticulo intestini.*

As regards the differential diagnosis of the affection when strangulation occurs, the symptoms differ from those of strangulated *enterocele,* in the remarkable slowness of their progress, and in the tumour being exempt from all pain and uneasiness for perhaps several days. Dr. Riecke considers that it may often be confounded with epiplocele, and that frequently there is no certain means of distinguishing it from this latter form of disease, and in this opinion we entirely coincide.

Prognosis. Dr. Riecke regards the occurrence of strangulation as less dangerous in this than in other descriptions of hernia, because of the slow course of the symptoms. On the other hand, however, the reduction of the *diverticulum,* whether by an operation or the taxis, may fail to relieve the symptoms, because the *stricture* in the intestine may remain obstructed, and the amount of its contraction can never be determined

during life. The performance of an operation Dr. Riecke considers as more likely to be followed by a favorable result than in ordinary enterocele, provided this measure be had recourse to sufficiently early, before the inflammation of the sinus has run too far, and the stricture became too much obstructed by tumefaction of the mucous membrane of the intestine. The slowness of the symptoms, however, admits of hope at a much later period than it could be reasonably entertained in ordinary cases, even up to the 8th, 10th, or 12th day. Gangrene of the *diverticulum*, or its injury by the knife during the operation, is of less consequence than similar lesions of the intestine in enterocele, for the *diverticulum* not being essential to the continuity of the intestinal, an artificial anus consequent on its injury is much more readily cured than one in which the intestine is implicated.

Treatment. Dr. Riecke's views respecting the treatment of the affection, whether in the reducible, irreducible, or strangulated state, are pretty much those most generally recognized by modern surgeons as applicable to hernia generally, with the exception indeed of one point, which we shall briefly notice. It has been already stated that the return of the *diverticulum* may not be followed by relief of the symptoms of strangulation, the bowel continuing obstructed in consequence of the existence of the sinus and stricture of the intestine; under such circumstances, Dr. Riecke recommends the administration of metallic quicksilver as the only means of quickly ameliorating this condition of things; this remedy we are told rapidly allays the vomiting and overcomes the obstruction of the bowels; it should be administered in doses of from two to four ounces repeated at short intervals, and the entire quantity given should not exceed two pounds; to facilitate its action and prevent its lodgment in any particular part of the intestinal canal, the patient should move about, or at least frequently change her position in bed. The circumstance above mentioned is not the only indication for the administration of metallic mercury, it being also recommended where the patient absolutely refuses to submit to the operation, when the sac and *diverticulum* are so intimately adherent that the latter can be only partially returned, and when the *diverticulum* from being gangrened cannot be returned and the symptoms of strangulation continue.

The Third chapter, which extends to 20 pages, contains a very diffuse exposition of Dr. Riecke's views respecting artificial anus and fæcal fistula; but we must candidly acknowledge that we cannot very clearly understand his opinions, or perceive their practical importance. Dr. Riecke's object is to show that the hernial sac is that concerned in the formation of the interval (or membranous funnel of Scarpa,) which in artificial anus intervenes between the cutaneous opening and the breach in the intestine. Whether he limits this doctrine to those cases of artificial anus which are the consequence of the destruction of but a small portion of the diameter of the bowel, or extends it to those in which an entire coil of intestine has been destroyed, does not very clearly appear. At page 69 we are told that, "when a hernia of the diverticulum or an enterocele partialis becomes gangrened, the edges of the opening in the bowel become adherent with the hernial sac in the hernial canal, or to the cutaneous opening, and the bowel itself forms the membranous funnel which, according to Scarpa, can only be constituted by the portion of the hernial sac that has escaped gangrene." And again, at page 75, it is said,

"The hernial sac can only contribute to the formation of the funnel communicating with the cutaneous opening, when the intestine has become adherent to the superior orifice of the inguinal or crural canal, and its contents escape through one of these canals clothed by its hernial sac." We must confess our inability to see how these statements establish Dr. Riecke's doctrine or upset that of Scarpa; but as they contain the sum and substance of the author's arguments, we lay them before our readers that they may form their own opinion on the matter.

We have given so very full an analysis of Dr. Riecke's work, that we have left ourselves little room for comment or criticism, which are indeed on that very account the less necessary; but we cannot conclude without a few observations as to the nature and extent of the materials on which the treatise is founded.

From the minuteness of the details and the absolute statements made respecting the pathology and symptoms of the *hernia e diverticulo intestini*, it might be supposed that Dr. Riecke had framed the history of the affection from the observation of an extensive series of cases—but such is not the fact. Upwards of half the work indeed is occupied with an account of 19 cases, but of the entire number four only can be considered as supporting the author's views. Three are cases in which metallic mercury was successfully administered in constipation with symptoms of ileus unconnected with hernia. Four are cases of strangulated femoral hernia, of which two occurred in the author's practice, where an operation was followed by recovery. One is a fatal case of femoral hernia on which the author operated, but no post-mortem examination was allowed. Two are cases of artificial anus, which present nothing that we can discover calculated to support Dr. Riecke's opinions. Four are fatal cases of femoral hernia collected by the author from various sources, in which a post-mortem examination was performed, but in which the anatomical characters assigned to *hernia e diverticulo intestini* are *not* mentioned. All these cases, excepting of course the first three, Dr. Riecke assumes to have been examples of diverticular hernia, because of the gradual formation, smallness, and globular shape of the tumour, and the slow progress of the symptoms of strangulation. Four of the cases, however, the 1st, 2d, 3d, and 6th, are examples of strangulated femoral hernia which terminated fatally, and were examined after death by Dr. Riecke, and they entirely correspond to his account of the affection under consideration. These four cases in fact constitute the whole foundation on which the work is written; and though they certainly establish the existence of the peculiar form of hernia to which our author has directed attention, they obviously do not warrant the positive and dogmatic history which is given of it. We have nevertheless thought it right to put our readers fully in possession of the contents of Dr. Riecke's work, inasmuch as we believe he is the first who has noticed the peculiar form of hernia which he describes. Several authors, as is well known, mention having found a true diverticulum forming the contents of a hernial sac, but we are not aware of a single instance in which a *diverticulum spurium* has been observed to constitute a hernia. Cruveilhier, for example, (*Anatomie Descriptive*, t. ii. p. 492,) in describing the two kinds of diverticula of the intestinal canal, says that he has on several occasions seen the true diverticulum engaged in hernia, but makes no mention of having observed a similar fact with regard to the false diverticulum.

ART. III.

1. *Traité Pratique de l'Art des Accouchemens.* Par CHAILLY (Honoré), D.M. &c. 206 figures sur bois.—*Paris*, 1842. 8vo, pp. 784.
Practical Treatise on the Art of Midwifery. By Dr. CHAILLY. With 206 Woodcuts.—*Paris*, 1842.

2. *On the Theory and Practice of Midwifery.* By FLEETWOOD CHURCHILL, M.D. M.R.I.A. &c. &c. Illustrated by upwards of 100 highly-finished Wood Engravings by BAGG.—*London*, 1842. 8vo, pp. 479.

M. CHAILLY has been trained to practical knowledge in excellent schools and by equally good masters, and in the present volume he gives us the sum and substance of the doctrines and opinions of the well-known and highly-esteemed P. Dubois, whose pupil he has been for many years. He has added, too, the fruits of his own personal experience, derived from a long attendance at the Maternité and the Clinique d'Accouchemens of Paris; and to render his treatise complete, he has drawn occasionally upon the works of the most celebrated foreign writers, as Lachapelle, Desormeaux, Naegele, Velpeau, Stoltz, Moreau, &c. The object of the work is to give practical information; the various speculations and discussions upon merely scientific points that are entered into by various midwifery writers are purposely abstained from by M. Chailly, although their interest is not denied.

The first chapters of the work are dedicated, as they ought to be, to an account of the various organs engaged in the act of labour, and the diagnosis of normal pregnancy, from the commencement to the termination of utero-gestation. Upon the contested and still doubtful subject of the stethoscopic sign, termed "souffle placentaire," placentary murmur, M. Chailly observes that the cause of this sound has been attributed exclusively to the circulation of blood in the placenta, but that if this were the case we ought, in the same woman, always to hear the "souffle" at the same place during the whole period of pregnancy; it is not, however, he says, fixed at any particular point; besides which the same sound is heard when the uterus is developed by disease, and when the ovaries are enlarged, and in cases where there is no placenta, as after labour, when the placenta has been extracted. Bouillaud* designates this sound by the name of "souffle abdominal," and attributes it to compression of the large vessels, the aorta and iliacs. Haüs is of the same opinion.† Kennedy (on Obstetric Auscultation), and Rigby (System of Midwifery, p. 53), and the majority of modern observers have abandoned the opinion first promulgated by Kergaradec, that this so-called "souffle placentaire" is produced by the circulation of the blood in the spongy structure of the placenta. It is no longer doubtful, we believe, that this sound is not connected with the placenta, but that it depends upon the increased vascularity and peculiar arrangement of the uterine vessels during pregnancy, and hence the application of the term "souffle utérin" by P. Dubois and others.

We are gratified to find, from the remarks of M. Chailly (p. 184), that the induction of premature labour, in appropriate cases, has no

* Traité Clinique des Maladies du Cœur, pp. 274.

† Die Auscultation in Bezug auf Schwangerschaft, von Dr. C. J. Haüs.—Würzburg.

longer to contend with the mistaken prejudices that were formerly entertained against the practice by the French accoucheurs. The best among them now regard the practice, when properly and carefully limited, as a valuable and perfectly justifiable addition to our means of averting dangers that would be almost insuperable by other means. In addition to the testimony upon this very interesting subject, which the records of English writers would afford, we may observe that from the statistical account published by Stoltz in 1838, of 211 labours artificially induced, more than half the children were born alive, and M. Chailly says, scarcely 1 of the mothers died in 15. We are sorry it is not stated whether this mortality of the women was fairly to be attributed to the operation. H. F. Kilian's* cases, collected from various sources, afford less cause for apprehension as to the fate of the mother: of 161 cases in which premature labour was induced in different countries up to the end of 1831, 115 of the children were born alive, 46 died; 73 of the 115 continued to live and had good health; 42 died soon after birth, or their fate was unknown: 8 of the mothers died after the operation, but 5 of these from disease which had not the slightest connexion with the operation. In this country we have heard and read a good deal of the frequent success of the Cæsarean operation in France. Here is M. Chailly's evidence as to Paris: "We have not had within the walls of Paris a single example of a woman who has survived the Cæsarean operation." (p. 185.) In one case operated upon by P. Dubois, the woman died 17 days after the operation of tetanus, when everything promised a favorable issue. This is the longest period after the operation which any of the patients lived. Upon three or four occasions we have known artificial labour induced without any preparatory treatment. M. Chailly, and most continental writers, consider it very requisite. "The operation being decided upon, the woman should be prepared for several days before, by warm baths and emollient injections into the vagina." (p. 187.) Dubois performs the operation in the following manner: By means of a speculum he first brings the cervix uteri into view, and inserts into the os uteri a small and well-greased cone of prepared sponge, with a thread attached to it, to secure it externally. A piece of soft sponge is then introduced into the vagina large enough to fill it; the whole is supported by compresses and a T bandage. M. Dubois frequently, too, conjoins with these measures the exhibition of three doses of ergot. M. Chailly has almost always found uterine contraction take place in the course of an hour or two after the use of these means, and when the severity of pain indicates a sufficient dilatation of the os uteri, to permit the easy rupture of the membranes, the liquor amnii is evacuated by puncturing them with a pen, cut as if for writing, and the act of labour is induced. (p. 188.)

We cannot at all coincide with some of the opinions stated by M. Chailly upon the subject of abortion. In the first place he endeavours to show why it must be, and that it is the fact, that the hemorrhage that accompanies abortion is less abundant in proportion as pregnancy is far advanced. "Aussi l'hémorrhagie est-elle d'autant moins abondante, que la fausse couche a lieu plus près du terme." (p. 203.) Again, as to the prognosis of abortion with hemorrhage, "as to the mother it is the more

* Die Operative Geburtshülfe. Von Dr. H. F. Kilian.—Bonn, 1834. Erster Band. pp. 298.

serious (plus grave) the less the pregnancy has advanced." (p. 205.) Now that the very reverse of these two assertions is practically true in the great majority of cases, we have no doubt whatever, and we confess we thought there was upon this point at least a unity of opinion among practitioners. In the treatment of the first period of threatened abortion, and for the purpose of preventing it, great confidence is attached by M. Chailly to the use of repeated enemata, with from 15 to 20 drops of laudanum administered every half hour till the symptoms have subsided. In many cases he has known very severe hemorrhage occur in the early months, and still pregnancy has gone on to the full period. This is in accordance with our own experience, and we advert to the sentiment of M. Chailly upon this point, for the purpose of diminishing the belief that is entertained by many that abortion must of necessity follow if the hemorrhage is at all abundant. This opinion is mischievous, because when it is held, the practitioner thinks little or nothing of attempting that in which he may still succeed, the prevention of miscarriage. The care that M. Chailly takes to limit the use of cold during hemorrhage is very prudent, for in ordinary practice no remedy is more frequently abused. He also very properly inculcates the propriety of keeping the upper parts of the body warm by every possible means, in severe cases of hemorrhage, while cold is being applied to the lower part of the abdomen and the thighs. More than one case has fallen under our notice in which if this rational principle had been acted upon, the flickering spark of life that was still remaining might perhaps have been preserved and health restored, when the little of power that was left was exhausted by the too long continuance of cold. In severe cases of hemorrhage, M. Chailly advocates the practice of plugging the vagina, an expedient which our readers are doubtless aware is not approved of by all English authorities, from the apprehension of converting an external into an internal hemorrhage. We are not prepared to deny that this objection to the plug has some foundation, but infinitely too little, we affirm, to deter us from the use of it, at any period of pregnancy. He objects, too, to the practice of rupturing the membranes in cases of severe hemorrhage in the early months of pregnancy, and points out very properly the importance of the ovum being expelled unbroken. At a later period of pregnancy we may be justified in rupturing the membranes, because if then the placenta is not expelled, the size of the uterus will permit the introduction of the hand for the purpose of removing it. In this opinion we entirely agree; but we could quote some opponents to it among very respectable English professors. In commenting upon the physiological phenomena of labour, M. Chailly observes that in obstetrical language the words pain and contraction are employed as synonymes, because in most women pain is inseparable from uterine contraction. Some women, however, are delivered with very little pain, and still efficient contraction of the uterus must have taken place. This is true, but the fact is rare. In one case that recently occurred to us of a shoulder presentation, in which we found very great difficulty in introducing the hand, on account of very rigid uterine contraction, the patient suffered scarcely any pains.

Mechanism of spontaneous delivery. Upon this subject M. Chailly states, in opposition to some authorities, that the mechanism of spontaneous delivery is exactly the same in all positions of the summit of the head, whether the occiput looks to any point of the right or left lateral

half of the pelvis. There is, however, a slight difference in the anterior and posterior positions. In the latter the movement of rotation, which brings the occiput under the arch of the pubis, is more extended, and besides, these positions may sometimes, but very rarely and as exceptions, be converted into permanent posterior positions. The general law, however, remains valid, that spontaneous delivery is effected in obedience to the same rule, whatever may be the part of the brim of the pelvis with which the occiput may be in relation. We cannot sufficiently admire the simple, just, and practical manner in which the whole subject of the mechanism of labour is treated. M. Chailly never allows himself to be drawn from his subject by mathematical explanations and illustrations which some of his and of our brethren, too, are so fond of indulging in; they can never be necessary, and must always be "caviare to the multitude" of practical enquirers. The most experienced may sometimes, says M. Chailly, be deceived in the following manner: We have seen several such cases. Without any other phenomena of labour there may be regular uterine pains. Upon examination per vaginam we may feel assured that the process of labour is commenced. The cervix uteri is open, the membranes form a tense tumour, the pains go on for some hours, and then a calm succeeds, and true labour does not occur for several days. M. Chailly has frequently witnessed these premature contractions, and occasionally several times in the same patient during the last fortnight of pregnancy. "How can we distinguish at first this commencement of labour from a regular labour that is to go on to the expulsion of the child? I do not believe it is possible to guard against this error; time alone can enlighten the accoucheur upon this point." (p. 291.) This is true, and it teaches us not to risk the confidence of the patient by hasty and decisive opinions as to the termination of a labour. As Horne Tooke advised in politics, so ought we in midwifery practice, "to deal in generals," and learn the art of equivocating by consolotary phrases which tie us to no fixed time. M. Chailly agrees with Kilian* that, although rupture of the perineum is by most practitioners but little feared after the exit of the head, and therefore no support is given to it during the passage of the shoulders, most lacerations of this part are caused when the shoulders are expelled. The experience of thirty very active years in midwifery practice leads us to a different conclusion. When the perineum is ruptured, we believe, the accident is very generally, we scarcely refrain from saying always, caused by the passage of the *head* of the fœtus. The following hint as to the performance of a comparatively trifling duty is not without value or necessity. When the funis contains a larger quantity of the gelatinous matter than usual, it is useful, before we apply a ligature, to press out the lymph it contains with the fingers, or to give an exit to it by small punctures. Without this precaution, after the spontaneous escape of the fluid, the ligature fails to act upon the divided vessels, and the blood escapes from the umbilical arteries, and endangers the life of the child. M. Chailly is surely wrong, or we and all other observers are, in asserting that "there is in twin cases almost always vascular communications between the two placentas." (p. 307.) Such is very rarely the fact. The following remark and caution may appear unnecessary and not very complimentary to the profession. We can corroborate M. Chailly's statement, however, that it

* Loc. cit. "Von dem Schutze des Dammes." Erster Band. p. 171.

has not unfrequently happened that practitioners have felt confident of the existence of, and have treated cases for, puerperal inflammation, by bleeding, baths, &c. &c., when the only mischief that existed was a bladder over-distended with urine, which had altogether escaped attention from gross carelessness. He believes, contrary to the general opinion, that the ergot has the power of causing uterine contraction when it has not before been manifested. "This medicine, however it may be administered, is frequently rejected by vomiting. It then should be given in a clyster, and it is thus even more rapidly absorbed, than when given by the stomach, and acts more directly upon the uterus." (p. 324.) This is a valuable bit of information, *if* it be correct. We should have liked a little detailed proof of this alleged fact. In irregular, ineffectual, and exhausting uterine contractions much confidence is placed in repeated enemata of injections of a quarter of a pint of warm water, and 15 drops of laudanum, gradually increasing the quantity to 60 or 80 drops, and thus repeating the injections every quarter of an hour if necessary, but "in general, a few minutes after the first injection, the pains diminish, a calm succeeds, refreshing sleep, and a return of more regular and effective pains occurs." (p. 329.)

Anterior obliquity of the uterus. By placing the patient in a proper position, and by the application of a bandage, any inconvenience arising during labour from this not unusual malposition of the uterus is usually overcome. "But sometimes it is necessary to draw the cervix uteri forwards by the fingers of one hand, while the uterus is raised with the other. This reduction of the cervix should be effected during the absence of pain, but the fingers should keep the cervix forwards during contraction." (p. 335.) We are inclined to speak even more guardedly upon this subject than our author does. In the very great majority of cases of anterior obliquity, Dr. Gooch's "tincture of time" is the best and the efficient remedy. It is true we may often draw the cervix forwards during the absence of pain, when it is very much tilted backwards from the anterior bearing of the uterus itself, but until nature herself has, perhaps by long efforts, placed the parts in a more favorable position, we can very seldom keep the cervix forwards with the fingers without such a degree of force that the nature of the case neither justifies nor requires. M. Chailly attended the labours of two paraplegic women at the Clinique. Neither of these patients required any particular attention. "Still this disease may impede the regular uterine action and render assistance necessary. It might be the same with hemiplegia." There is a paucity of information upon this subject in midwifery records. A case recently occurred at a London hospital of a paraplegic woman, who went through the act of labour without assistance or difficulty. At page 359, Dr. Montgomery's opinion upon the subject of spontaneous amputation of the limbs of the fœtus in utero is misstated. Dr. Montgomery (vide Signs and Symptoms of Pregnancy, p. 321, et seq.) does not attribute this event to "constriction of the funis" alone. He admits it as an occasional cause of this effect; but he proves by his own experience and that of others, that the most frequent cause of this injury to the limb of the fœtus arises from the formation of firm bands or threads of organized lymph, which act as a ligature. We have now before us a preparation of a five months' fœtus. The funis is coiled twice tightly round the neck and once round the left arm, which it has deeply indented. The death

of this fœtus probably arose from the circulation in the cord being obstructed. Had it lived to the full period the arm would no doubt have been partially if not entirely amputated from the firm pressure inflicted upon it.*

M. Chailly considers it "perfectly useless" to establish a diagnosis between those cases of uterine hemorrhage which depend upon attachment of the placenta over the os uteri, and those which are what we term "accidental," inasmuch as the only indication for our practice is the degree of severity of the hemorrhage from whatever cause it may proceed. In this country we think differently, and, with respect be it said, we believe more justly upon this point. And though at page 368 this diagnosis is said to be "complètement inutile," at page 376 sufficient reasons are given to prove the contrary. The plug is mentioned as "un moyen précieux" in severe hemorrhage during the latter months. In cases of rigidity of the os uteri the application of extract of belladonna is highly spoken of. We know but little of this practice in England. "I have frequently seen the application of belladonna successful. We should use the extract of the consistence of soft wax. A small portion of it should be placed upon the nail of the index-finger, and carried within the cervix uteri. The heat and mucous moisture dissolve it, and it may be spread easily over the internal surface of the os uteri. When this remedy is successful it generally acts very quickly. The cervix softens and dilates in ten or fifteen minutes." (p. 396.) The praise bestowed upon this practice is tempered by candour, for it is confessed that it often fails. Notwithstanding the high authorities opposed to us, we feel quite convinced that M. Chailly's disposition to leave what are termed malpositions of the head to nature, rather than have recourse to manual interference for the purpose of placing the head in the strictly natural position, is very wise and very prudent. We are gratified to find at p. 424 that M. Chailly deprecates the practice of many of his brethren, who from very weak scruples, refuse to open the head of a living child, whatever may be the danger to which the mother is exposed by the continuance of the labour. The doctrine of English accoucheurs, upon this very delicate and responsible point, are liberally preferred to those which generally prevail in France and other parts of the continent. M. Chailly has frequently seen the "cephalotribe" of M. Baudelocque employed with the greatest success in cases where formerly the Cæsarian operation would have been deemed necessary. No doubt there must be difficulty and danger in the employment of this instrument, but we think it deserves more attention than it has met with here. We object to the following rule that is laid down for the application of the forceps. "The hand opposed to that which holds the handle of the instrument should always be introduced, excepting the thumb, into the maternal passage, to direct the blade and protect the vagina against the contact of the instrument." (p. 446.) This would always be very painful to the patient, generally difficult for the practitioner, and can rarely if ever be necessary. Two fingers will sufficiently and safely guide the blade of the forceps. After the example of M. Dubois, M. Chailly prefers introducing the blades of the forceps first at the back of the pelvis, and then brings them round to the point they are to be definitively placed in by moving the handle of the instru-

* In our Ninth Volume, p. 564, we have recorded a similar case from an American Journal.

ment. M. Dubois, too, and M. Chailly have strong objections against showing the forceps to the patient before they are employed. We, on the contrary, think and have always taught our pupils that it is very prudent to do so. The patient knows that instruments are to be employed. She thinks that all instruments are cutting instruments. Show her the forceps; tell her that they are, what they really are, a long pair of hands, and her fears are calmed. We at least have always found this to be the case. The following passage evinces a nicety of ear and a degree of stethoscopic tact which we quite envy:

"The accoucheur may be called upon to apply the forceps, when the head, long engaged in the pelvis, may be the seat of a considerable sero-sanguineous tumour which marks the character of the position. It is in such a case that auscultation renders us such great service. In fact, if the 'summum' of intensity of the pulsations of the fœtal heart is heard directly forward, the case is one in which the occiput is either to the pubis or the sacrum, and the application of the forceps is the same. If the 'summum' of intensity of the pulsations is to the left, there is every reason to believe that the position is with the occiput anteriorly to the left ilium," &c. &c. (p. 478.)

M. Chailly proceeds much further with these stethoscopic distinctions than we deem it necessary to quote. That no man can establish them we will not venture to assert; but we do most positively that, for the purposes of general practice, such rules, as guides for the manner of applying the forceps, are utterly worthless and unavailable. The objections that are urged against the practice of separating the membranes from the uterus when the hand is introduced for the purpose of turning the fœtus, appear to us practically sound. It is better, easier for the practitioner, and safer for the mother and child, to "rupture the membranes at the centre when the hand enters the uterus." (p. 511.) Some writers lay down very exact rules as to the manner of grasping the fœtal limbs in the operation of turning, and the precise position of the operator's fingers, as well as the parts of the fœtus on which they should be placed. All this M. Chailly very properly smiles at. "We grasp the limbs how we can. (p. 515.) In general we can turn as easily by grasping one extremity as with both." Upon the subject of the Cæsarean operation M. Chailly's sentiments are exactly in keeping with the invariable rule of British midwifery practitioners, and strongly opposed to many among the French and Germans even of high repute. "I will never perform the Cæsarean operation, except in cases of pelvic contraction, that render the mutilation of the fœtus impossible." (p. 553.) Every well-informed accoucheur is aware how much we are indebted to Madame Lachapelle (Pratique des Accouchemens), for clear notions respecting the passage of the head through the pelvis in face presentations, as well as for proving that the efforts of nature are usually sufficient. M. Chailly agrees with Madame Lachapelle. When the breech or feet present, and the back and occiput of the fœtus are directed towards the back of the mother, "the delivery in this posterior occipital position is not so difficult as is generally imagined; and when much difficulty is experienced in turning the back of the fœtus towards the mother, we should not obstinately persist in effecting this change." (p. 613.) Violence can never be justifiable to alter this unfavorable position of the fœtus, but very generally the attendant may and ought to prevent it if he has the whole management of the case.

We have noted in M. Chailly's book many other passages from which we might select good practical remarks; but we must stop. We must, however, in common justice, declare our opinion that the work merits much more than the cold praise of being respectable. It would be attractive if it were merely from the fact of its giving to the profession the practical opinions of M. Dubois. It is additionally so from the information afforded by the author himself, and the clear style in which it is conveyed.

The object of Dr. Churchill's volume is to offer to the student in midwifery a work, embracing the modern discoveries in the physiology of the uterine system, with all the recent improvements in practice, in a condensed form, amply illustrated, and at a moderate price. It contains ample evidence of the industry with which the author has consulted every authority of eminence to bring his work up to the present state of science, and he confesses, generally, in the preface that he has not hesitated to avail himself of the labours of Drs. Ramsbotham, Rigby, and others. It would have been well, we think, if more precise acknowledgment had been granted to Dr. Ramsbotham, for we observe that many of the woodcuts are copied exactly from his work: others are merely reversed. Dr. Churchill's work is upon the whole a fair and instructive compilation; but, as Malthus says, "it is born into a world already possessed," and we cannot conscientiously say that it will bear comparison as a practical guide, with other works that have recently appeared upon the same subject, and which we have noticed.

Art. IV.

Samuel Thomas von Soemmering *vom Baue des menschlichen Körpers.—Leipzig*, 1839-41. 8vo.

Zweiter Band: Knochen-und Bänderlehre. pp. 296.

Dritter Band: Muskel-und Gefasslehre. pp. 758.

Vierter Band: Anatomie des Gehirns und Nervensystems. pp. 752.

Samuel Thomas von Soemmering *on the Structure of the Human Body. Second Volume, containing the Bones and Ligaments. Third Volume: Muscles, Arteries, Veins, and Absorbents. Fourth Volume: Anatomy of Brain and Nervous System.—Leipsic*, 1839-41.

Amongst the great anatomists of Europe few have ranked higher than Samuel Thomas von Soemmering, whether we regard the completeness, beauty, and exactness of his works, as shown in the treatise De Corporis Humani Fabricâ, and in the monographs of the Senses, or the labour and toil spent in the formation of a museum of nearly 4000 preparations. The De Corporis Humani Fabricâ has, however, ceased to exist in the library of the student, and only retains its place on the shelves of the anatomist as a work of reference. The deficiency of a general knowledge of Latin has tended to this result, as well as the publication of so many other excellent works in Europe and England on the same subject. The learned and accurate work of Hildebrandt,* and the recent manual of Bock,† have supplied its place in Germany; whilst in Bichat‡ or Cloquet§ the French student has a more pleasing source of information,

* Hildebrandt, Handbuch der Anatomie. 4 Bde. Braunschweig. 1830-32.

† Bock, C. E., Handbuch der Anatomie. 2 Bde. Leipzig, 1838.

‡ Bichat, Traité d'Anatomie descriptive. 5 Tomes. Paris, 1831.

§ Cloquet, H., Traité Descriptive. 2 Tomes, Paris, 1836.

as well as in the work of Cruveilhier,* which, with a degree of accuracy hardly ever equalled in the same subject, unites an ease and facility of expression rarely met with in professional writing. In our own country the work of Mr. Quain † unites illustration with accurate detail, whilst the dissecting-room manual of Mr. Ellis‡ stands unrivalled as a work on practical anatomy. To these may now be added the work of Cruveilhier, which the excellent translation of Dr. Madden has opened to every English student.

The volumes of Soemmering's work mentioned above form part of a new edition of the treatise De Corporis Humani Fabricâ, now publishing in Germany. This edition, when complete, will consist of nine volumes, edited (we may almost say rewritten) by the most distinguished anatomists of Germany. The second, third, fourth, and sixth volumes are published. The materials of the whole work are distributed in volumes, as follows, each subject being intrusted to the all-sufficient superintendence of the respective editors:

Vol. I. Biography of the principal Anatomists since the time of Haller, with a History of Anatomy and Physiology during the same period. By Professor Wagner, of Erlangen. (Unpublished.)

II. The Bones and Ligaments. By Professor Wagner, of Erlangen.

III. The Muscles and Vessels. By Professor Theile, of Bern.

IV. The Brain and Nervous System. By Professor Valentin, of Bern.

V. The Senses and Internal Organs. By Professor Huschke, of Jena. (Unpublished.)

VI. The Anatomy of Tissues and Chemical Composition of the Human Body. By Professor Henle, of Zurich.

VII. History of Development. By Professor Bischoff, of Heidelberg. (Unpublished.)

VIII. Pathological Anatomy. By Dr. Julius Vogel, of Munich. (Unpublished.)

IX. The Anatomy of Races and Nations. By Professor Wagner, of Erlangen. (Unpublished.)

The names of these authors are all too well known to need any comment; they are each especially fitted to complete this work of Soemmering, and add to it the discoveries and observations of later years.

In the present article it is intended to give an abstract of the contents of the volumes containing the bones, ligaments, arteries, muscles, and nervous system; the volume on general anatomy, and the part of the fourth volume devoted to the physiology of the nerves, will be noticed on another occasion.

The Museum of Soemmering is described in the first volume. This consists of 3917 preparations, and is now at Giessen. Some of these preparations were collected very early in life by Soemmering during his tour through England, Scotland, and Holland, in 1778. A very considerable part of the museum was presented by Albers, Bremser, Prochaska, Tiedemann, and others, and more especially by Behrends of Frankfort. One third part of the collection consists of pathological, the remainder of anatomical and physiological specimens. The museum contains an extensive series of the brains of animals, the original prepa-

* Cruveilhier's Anatomie Descriptive. 4 Tomes, Paris, 1831-5

† Quain's Elements of Anatomy, 4th edit. ‡ Ellis's Demonstrations of Anat. 1840.

rations for the plates of the senses, and an extensive series of embryonic preparations. Amongst the physiological preparations many are intended to illustrate the opinions of various authors, and are accompanied with references to similar cases in English and foreign works.

The account of the Bones and Ligaments is rendered more complete by notes from the other works of Soemmering, as well as from later works; much less, however, is added to this than to the succeeding volumes, as the knowledge of the skeleton and ligaments was rendered comparatively complete much before the other branches of human anatomy. The plates of Albinus and Bidloo are chiefly cited for illustrations of the bones, whilst those of Weitbrecht and Langenbeck are referred to in the description of the ligaments.

The first part of the third volume contains the Muscular System, edited by Theile, of Bern, and contains 386 pages. The plan of Soemmering is adopted, although the work itself is almost entirely rewritten. This part contains an accurate description of the anatomy and varieties of each muscle of the body, with its action and relation to the surrounding parts. In the anatomy of the muscles themselves the descriptions of Albinus, Weber, Arnold, and Santorini are especially referred to, as presenting the most accurate descriptions as well as the clearest representations. In the varieties of the muscles, the differences in origin, the number of tendons, attachments, and bellies are chiefly described: the data from which these are taken are collected in many instances from separate and detached papers in Journals and Transactions, but the most considerable part appears to be the result of the observations of the editor himself. The new muscles are divisible into two classes: those which have been described by others, but never included as a regular part of human anatomy in the common works on descriptive anatomy; and those which are first described by the editor himself. Amongst the former may be classed the reflector epiglottidis, dilatator narium posterior, musculus sacci lachrymalis, &c.; amongst the latter, the rotatores dorsi, &c. Some advantage has also been gained by ascertaining the frequency of occurrence of other muscles, as the psoas minor and levator glandulæ thyroideæ. In the relative anatomy of the muscles, the relations to the skeleton and muscular system alone are mentioned; the relative anatomy of the arteries and nerves being given in the parts especially devoted to those systems.

The part devoted to the muscles includes the fasciæ and various fibrous sheaths of the body, which are described accurately and concisely. Up to the present day the majority of anatomical works have presented a great deficiency in the descriptions of the fasciæ: when they are described accurately, the writers in many cases have aimed at originality in dividing into endless layers what is at best about as thick as a sheet of strong linen. By this mode of proceeding the student has frequently been dismayed and disheartened; and he is forced to seek the best and most accurate descriptions of the fasciæ of the body in the works of the great surgical writers. The anatomy of the head and neck, by Burns, still presents the best account of the fasciæ of this region; while the surgical writings of Sir Astley Cooper and Mr. Lawrence supply the most accurate and valuable descriptions of the fasciæ of the thigh and groin that we yet possess. The descriptions of those structures in Theile, like those by Krause, of Hanover, are plain and concise, but deficient in

point and practical value, and not to be compared with those by Mr. Ellis in his recent Manual.

We shall now briefly notice the new matter which has been added to this volume: in doing so we will only mention the most important points, leaving it to the reader of the work itself to test the merit of the accurate description which abounds in every page. The first muscle revived from old works is derived from the Observationes Anatomicæ of Santorini, where an incomplete description of it exists under the name *musculus incisuræ majoris auriculæ:* it is termed by Theile

"*Dilatator conchæ.* This muscle arises from the anterior surface of the cartilaginous portion of the meatus, near the notch between the meatus and tragus; from this situation it passes downwards and outwards to the anterior surface of the tragus. *Action.* This muscle draws the tragus forwards, and thus dilates the concha." (p. 26.)

In the description of the nasal muscles some notes are inserted in reference to the opinion which Santorini, Arnold, and Tiedemann have held on this subject. The opinions expressed by them Theile considers as incorrect and imperfect. His own opinion is thus stated:

"The examination of the muscles of the nose is one of the most difficult subjects in myology. The description of the depressor, dilatator, anterior, and posterior elevator nasi is founded on numerous observations with the microscope and other means. I therefore cannot think my description of these parts superfluous. Arnold drew as compressor narium a small muscle which arose from the point of the nose and passed transversely outwards to the ala nasi, into which it was inserted. I do not know whether this is the same muscle which Santorini has represented but not named. I have never been able to find, even with the microscope, either Arnold's or Santorini's muscle. In a muscular young man I have once observed with the eye alone muscular fibres, which passed from the anterior edge of the nasal cartilaginous ala downwards and outwards in a different direction to that described by Arnold. I have never been able to find the lateralis narium of Santorini, which is represented as arising from the canine fossa of the upper jaw-bone and passing up to the ala nasi, near the anterior nasal spine. This muscle appears to me to be a part of the compressor nasi." (p. 40.)

The description given of the *dilatator nasi posterior* is the following:

"After careful separation of the fibres of the pyramidalis nasi, depressor alæ nasi, and compressor nasi, a mass of cellular tissue is observed on the inferior and posterior part of the ala nasi, in which muscular fibres may always be seen with the microscope. These fibres arise from the edge of the frontal process of the upper jaw-bone as well as from the cartilage of the alæ nasi; from this part they descend and are inserted into the posterior half of the nasal openings. *Action.* It draws the posterior half of the ala nasi backward, and dilates the nasal opening." (p. 43.)

In most anatomical works the *musculus risorius* of Santorini is described as a part of the platysma myoides; this error Theile takes pains to correct from his own observation and the original account of Santorini. This muscle is separate: it is situated on the cutaneous surface of the platysma; the fibres arise on the aponeurosis of the sterno-mastoid or platysma, and, crossing the fibres of the latter, become connected with the depressor anguli oris.

In the works of Eustachius, Santorini, Soemmering, Albinus, and Weber, a muscle has been described as *levator pharyngis internus.* This muscle is not generally described in lectures and works at the present day; but as its existence is clear from the observations of some of the best

anatomists, the only excuse for the omission must be its small size; but since the compressor urethræ and muscle of Horner have attained such notoriety, it is hardly right to plead such an excuse. "The *levator pharyngis internus* arises from the lower edge of the cartilage of the Eustachian tube close to its pharyngeal extremity, and descending in the angle of the pharynx unites with the fibres of the palato-pharyngeus muscle." (p. 82.)

In the description of the *lingualis* muscle the superior portion covering the dorsum of the tongue is placed as a separate muscle from the inferior portion lying between the hyo-glossus and the genio-hyo-glossus muscles. To the former the name of lingualis longitudinalis superior, and to the latter that of lingualis longitudinalis inferior are given. The lingualis transversus of Arnold is also accurately described, but the peculiar vertical muscles of the tongue described by Gerdy* and Cruveilhier are reckoned as part of the hyo-glossus and genio-hyo-glossus muscles, and not as a separate muscle.

The *reflector epiglottidis*, previously described by Albinus and Santorini, is stated by Theile to arise from the arytenoid and inner part of the thyroid cartilage, and to be inserted into the lateral edges of the epiglottis.

In Müller's Archives, for 1839, Theile published a long account of some muscles of the back, which he named rotatores dorsi. In the present work he gives the following short description, which easily enables the student to find and recognize them.

"*Rotatores Dorsi.* On the dorsal vertebræ are found eleven small muscles on each side, which arise from the point or upper edge of each transverse process and pass to the lower edge of the arch of the vertebra above, as far as the basis of the transverse process. The first lies between the first and second dorsal vertebræ, the eleventh between the eleventh and twelfth dorsal vertebræ. The lower (with the exception of the last) are generally stronger than the superior. They are covered in their entire course by the multifidus spinæ, from which they are separated by a layer of cellular tissue. *Varieties.* The first is frequently wanting on the first and second or the eleventh, reckoning from above. A supernumerary one arises sometimes from the transverse process of the second dorsal vertebra, passes over the first dorsal vertebra and is inserted into the arch of the seventh cervical. The *action* of these muscles is to rotate the individual vertebræ on each other." (p. 164.)

Theile refers to the extensor and flexor muscles of the coccyx, described by Gunther† in his work on the Surgical Anatomy of the Muscles: these fibres arise from the lower part of the sacrum, and are inserted into the front and back part of the coccyx. They are very small, and constitute a mere rudimentary form of the large muscles moving the tails of animals.

These are the prominent novelties in the part devoted to the muscles and fasciæ; but the whole of it presents such accuracy and minuteness of detail as will well repay the reader. As a work of reference it possesses great value, and ranks with the elaborate productions of Albinus and Hildebrandt. To the learner who is acquainted with the German language this work, on account of its correctness as well as *from the descriptions* referring simply to the skeleton and other muscles, is a good book wherewith to commence the study of practical anatomy. If simply

* Gerdy, Mémoire sur la Structure de la Langue.

† Gunter, G. B., die Chirurgische Anatomie in Abbildungen. Muskellehre.—Hamburg, 1838-9. 4to.

read by itself it will appear unnecessarily minute and tedious; but if dissections be diligently performed at the same time, the minute accuracy of the descriptions cannot fail to be at once instructive and full of interest.

The second part of the third volume contains the description of the Arterial and Nervous Systems, with the absorbents, extending over 368 8vo pages. In this the editor has shown the same laborious industry which he exhibited in the part devoted to the muscles. The distribution of the vessels is described with a minute accuracy that can only have been acquired by dissection; whilst, in the enumeration of the varieties, his knowledge of the anatomical works of other countries as well as of the various memoirs and notices scattered through journals, is strikingly exhibited.

In arriving at a just arrangement of the arteries the editor appears to have experienced considerable trouble; this he attained finally by examining a number of fresh and dry dissections and establishing a standard from them, which standard he subsequently compared with a second set of dissections. In the description of the varieties all those of abortions are rejected, as belonging to morbid anatomy; a plan which does not appear to have been generally followed in previous works. No plates accompany this part of the work, but such frequent and exact references are made to the plates of Breschet, Tiedemann, Arnold, and others, that the deficiency can be easily supplied from them.

The description of the different arteries individually is well and clearly arranged. The name of the artery is placed at the head of the description with references to the best plates illustrative of it, then follow an accurate description of its relative anatomy, the measures of its length and diameter in different parts of its course, the distribution of its branches, and an enumeration of its varieties. In the part devoted to the relative anatomy, though considerable minuteness is observed, and extreme accuracy, yet the description is not so minute nor yet so advantageous to the student as in many of the English works; all the relations to various parts being mentioned with nearly the same exactness and ranked as of the same importance. Though this is just in an anatomical point of view, yet as the study of anatomy in this country is almost exclusively preparatory to the practice of the medical profession, the mode followed in the works of Cruveilhier, Harrison, Quain, and others, of setting the important surgical relations of the arteries forward in a prominent light is the most valuable and instructive to the student. In the enumeration of the various branches of the main arteries the greatest accuracy is observed; many branches which have previously only had a general description are placed as regular branches and have appropriate names given to them; this is especially the case in relation to the arteries supplying the ear, nose, and eye, in describing which the editor has made frequent reference to the works of Arnold,* Weber,† and Schlemm.‡ The varieties of each artery are given with a greater degree of minuteness and in a far more complete manner than in any previous work; and the minuteness of description is not confined to the large trunks of the body, but extends even to the minuter vessels, as those of the internal ear.

* Tabulæ Anatomicæ.—Turici. 1838 et posteà.
† Anatomischer Atlas des Menslichen Korpers.—Dusseldorf, 1836 et posteà.
‡ Arteriarum Capitis superficialium icon nova.—Berolini, 1830.

The additions made in the description of the individual vessels consist chiefly in the enumeration of new branches, in a minute description of vessels of which only a general description is usually given, and in an altered arrangement of vessels arising from the main and secondary trunks. In the new arrangement of the branches of some of the arteries the difference is chiefly confined to the smaller branches; and although the changes are calculated to add somewhat to the difficulties of studying anatomy, they seem to be justified by the attention which the editor has evidently paid to the subject.

The external and internal carotid arteries are described according to the usual arrangement, with the exception that the transversalis faciei is reckoned as one of the branches of the temporal: in most works, as well as lectures, in this country, this vessel is counted as one of the branches of the external carotid. The difference is not great nor very important, on account of the small size of this vessel. The branches supplying the internal ear are described most completely; but for the novelties in this description we must refer our anatomical readers to the original.

The arrangement of the branches of the subclavian arteries differs slightly from that usually given in English works, but not in important relations. The relative anatomy of the main trunk of the carotid artery is less complete than in some of the English works, but more complete and exact in the anatomy of the distribution of the branches, the various collateral channels of circulation about the neck and shoulder being thereby rendered very complete.

In the works of Hildebrandt, Krause, Cloquet, Cruveilhier, Ellis, and Harrison, the arteries of the spinal marrow are traced down to the lower part of the canal in front in one channel, and on the posterior part of the cord in two channels. The true and more accurate description of the circulation of these parts is given by Theile to the following effect:

"The true relation of the vessels on the anterior part of the cord is this: Two or more vessels pass through the intervertebral foramina along the anterior roots of the nerves to the anterior surface of the spinal cord as far as the anterior longitudinal fissure; here they are bent, and after having given off branches to the cord itself, unite by an ascending and descending branch with the arteries, entering in a similar manner above and below. In this manner one continuous artery is formed, receiving its blood from lateral branches. The anterior spinal arteries are less distinct than the posterior. The posterior spinal arteries cease high up. The net-shaped plexus formed by tortuous vessels, which is found on the posterior surface of the spinal cord, is formed by numerous vessels, which enter the intervertebral foramina and pass along the nerves to the back of the cord. In this situation these vessels unite with those of the opposite side as well as with those above and below. The circulation is thus carried on by currents entering laterally at certain intervals and anastomosing above and below, instead of by one descending current; each portion of the spinal marrow having thus its own peculiar set of tortuous vessels allotted to it." (p. 110.)

Into these anterior and posterior plexuses the spinal arteries are described as entering at a short distance below the skull. The description differs from that usually adopted rather in degree than in the addition of any new point; but inasmuch as it gives a more accurate account of the anatomy of an important part and one frequently referred to, it is deserving of attention. In Haller's Icones Anatomicæ we have almost the same account as that which Theile has here given.

In the arrangement of the branches of the abdominal aorta, the phrenic arteries are described as branches of the cæliac axis, and not as arising directly from the abdominal aorta; the latter is the distribution usually taught in the English schools, and agrees with the accurate description given by Mr. Harrison, of Dublin. Haller, Bichat, and Meckel consider the phrenic arteries as branches of the cæliac axis; whilst Monro, Mayer, Boyer, Bourgery, and Hildebrandt classify them with the branches of the aorta; all these writers, however, describe their course as variable, but that given by Hildebrandt is by far the most complete and accurate. "These arteries (phrenic) are generally two in number, but sometimes arise by a common trunk. In the latter case this trunk arises more frequently from the cæliac axis than from the aorta; in the former case, if they both arise from the same artery, it is from the cæliac axis, but the most common distribution is that where one arises from the aorta and one from the cæliac axis, the latter often being united with the coronaria ventriculi."

In the description of the arteries of the pelvis and lower extremities the chief additions consist in an accurate description of the minute arteries about the ankle and foot, to many of which especial names are given. The names are well chosen, and seem to be justly applied, not only on account of their regularity but also as rendering small vessels, which in this situation are extremely important in a surgical point of view, more known.

The description of the veins presents a complete and minute account of their normal anatomy, with a copious summary of the important varieties found to occur from time to time. In some of the branches the arrangement differs from that of other writers, but not so much as to require especial mention. In the department of the absorbents very little new matter is added to that previously described, but a complete summary from the best works is given.

The fourth volume contains the Nervous System edited by Valentin, already so favorably known by his Manual of Human Development, and his Memoirs on the Nervous System. With few exceptions this volume is an entirely new work, founded on original observations and dissections—the chief object being, in the words of the editor, "to establish a basis for the physiology and pathology of the nervous system founded on anatomy." The work is divided into three principal parts: 1st. The morphology, philosophical anatomy, and chemistry of the nervous system of vertebrate animals, and especially of man. 2d. The literature of the nervous system. 3d. The descriptive anatomy of the nervous system. The first division will be treated of elsewhere. The second division contains the anatomy of the brain and spinal cord, as well as of each individual nerve under a separate head; the author referring particularly to scattered papers in the various journals of different countries. The manner in which this volume is written is different from that of most anatomical works. At the same time that these include the author's own observations, they present also the opinions of the best writers on the subject; but in the work before us, the text presents us with the most exact and minute descriptions without ever noticing, except very rarely, the name of any single writer on the subject; ample justice is, however, done to these in long and laborious notes. To the labour and perseverance of

the author the greatest credit is due; but to the English reader, unless much time can be devoted to the study of anatomy, the work presents an uninviting aspect. It is written in hard and difficult German, arranged in some places in sentences of unusual length and complexity, and presenting a degree of minuteness in description and nomenclature remarkable even for a German work. The fifth cerebral nerve alone occupies more than one hundred octavo pages; and the synonyms of the corpora albicantia amount to twenty-five. Yet with all this minuteness and labour, the writer displays a degree of modesty and candour, which might serve as a lesson to many of far inferior industry and talent. In entering on the description of the fibres of the brain he laments the imperfection of his own description, and the want of practical knowledge as yet obtained on this subject, and looks upon its working-out as a legacy to posterity.

The work commences with the anatomy of the membranes, and description of the various surfaces, cavities, and masses of the brain, especial reference being made to the composition of each individual part, and the relations of the white and gray matter to each other. The anatomy of the brain is rendered complete, (according to our present knowledge,) by describing the various parts in their relations to each other, as shown by various sections, and by a minute description of the fibres. This part is extremely minute, and without the assistance of good plates almost unintelligible; but with the assistance of the Icones Anatomicæ of Professor Arnold, to which especial references are made, and by foot-notes, it is rendered more plain and intelligible.

The account of the distribution of the nerves occupies the remaining part of the volume, amounting to about 450 pages. Although this extent may seem excessive, yet the quantity of matter is so vast, that the narrative does not appear to the attentive reader too long or capable of much condensation. The quantity of matter and information far exceeds that given by any previous writer; and at the same time that the description gives the results of the author's own dissections it overlooks none of the claims of others. In many places the previous observations of writers are arranged in an historical form with the author's comments on them, so as to give some little relief to the dry matter of detail. Each nerve is described completely, and in the following manner. The various plates of the individual nerve are named first; those of the sympathetic alone more than fill a page; the minute anatomical description of the course and branches of the nerve follows next, and then the author adds a summary of the changes in the nerve at different periods of life, and an account of the experiments which have been performed, illustrative of its physiology. Our space will only permit us to quote a few of the many observations which are of sufficient importance to render an abstract interesting.

"*Olfactory nerve.* All the three roots of the olfactory nerve are medullary and distinguishable by their white colour from the gray substance which lies near and on them; but as the gray matter covers the middle root more than the two other roots, the middle root has been supposed to be less medullary in composition than the other two. Now although the degree in which the roots are covered by gray matter varies, yet the three roots are all medullary." (p. 294.)

"*Connexion of the nasal and olfactory nerves.* I have in vain looked for a communication between the olfactory nerves and the branches of the fifth pair ramifying on the nasal mucous membrane, and have never been able to trace any

communication between the first and fifth nerves in those branches, which are visible to the eye." (p. 303, *Note.*)

"*Nerves of the cornea.* In the ciliary ligament exists a considerable nervous plexus formed of the ciliary nerves; from this plexus eight or ten branches pass forwards and ramify partly in the cornea, and partly perforate the corneal margin to join the nerves of the conjunctiva." (p. 322.)

"*Connexions of the Gasserian ganglion.* The semilunar or gasserian ganglion enters into communication with other nerves in various places. 1. At the junction of the superior and internal edge, with the cavernous plexus of the sympathetic nerve surrounding the carotid artery. 2. In the same situation branches from the fourth cerebral nerve join it. 3. From the inner surface branches pass to the cavernous sinus, which, after passing through the posterior part of the body of the sphenoid bone, unite with the branches of the opposite side. 4. From the posterior and lower side of the ganglion many small fibres pass off to the cavernous and inferior petrosal sinus as well as to the neighbouring portion of dura mater." (p. 338.)

An extremely minute account is given of the relations and branches of the otic, ophthalmic and spheno-palatine ganglia. The branches of the fifth pair distributed to the lips and the branches of the portio dura distributed to the face are described seriatim, and with a degree of minuteness suited to the work as a book of reference. A clear and concise account of the distribution of the superior and inferior laryngeal nerves is given in the text, to which the opinions of Magendie, Theile,* Swan, and Müller, are added in a very complete note.

The plates of Professor Arnold are especially cited, and form almost a companion to the work. To these references are added others to the works of Bock, Langenbeck, Weber, and Scarpa; whilst the minutely accurate plates of Swan are so constantly noticed as to show on the part of the author a high esteem for our countryman, who, it must be admitted, has shown in the study of the same subject, equal industry and perseverance.

Such is a brief outline of the volumes of this great work now before us: if the succeeding parts are equally complete, this edition of Soemmering's *magnum opus* will stand first among the numerous treatises on anatomy. The brief space within which a considerable part of the work has been published justifies an expectation of its completion within a short period.

ART. V.

1. *De la Menstruation; faire connaitre l'influence que cette fonction exerce sur les Maladies, et celle qu'elle en recoit.* Par A. BRIERRE DE BOISMONT, D.M. *Mémoire couronné, par l'Académie Royale de Médecine dans la Seance des* 17 *Décembre*, 1840. (*Mémoires de l'Académie Royale de Médecine.* Tome IX. 1841.)

On Menstruation, &c. By A. BRIERRE DE BOISMONT, M.D. 4to, pp. 130.

2. *De la Menstruation considérée dans ses rapports Physiologiques et Pathologiques.* Par A. BRIERRE DE BOISMONT, M.D. &c.—*Paris*, 1842. 8vo, pp. 560.

On Menstruation, &c. By A. BRIERRE DE BOISMONT.—*Paris*, 1842.

THESE two works may be said to be the same, and yet different. The one first on the list formed a portion of the volume for 1841 of the Me-

* Theile, Dissert. de musculis nervisque laryngis.

moirs of the French Academy of Medicine, and is, doubtless, the identical memoir for which the prize was awarded. The octavo volume is a separate publication appearing at a more recent date, and is an amplification of the quarto memoir. As accounting for placing both at the head of this article, it is necessary to state, that all the first part of the article—the statistical part—was drawn up before the second publication reached our hands. The second part of the article—the pathological part—is based on the octavo volume.

Statistics of Menstruation. The author informs us that he derives his data respecting menstruation from the examination of 1200 females, in the upper, middle, and lower classes of society; and that these observations extend over a period of ten years: 830 of these cases were investigated by himself. He commences by endeavouring to establish the mean age at which menstruation makes its first appearance in the country, in the towns, and in the capital of France. 276 women supply the data of investigation, as regards the country; 214 belonging to the northern provinces, 57 to the central, and only 6 to the southern provinces. In ascertaining the mean age at which these 276 individuals menstruated for the first time, the author shows by the following table that it is about the age of fourteen years and ten months:

Country.

Age of its first appearance.	No. of cases.	Age of its first appearance.	No. of cases.	Age of its first appearance.	No. of cases.
7½	1	13	30	19	9
8	0	14	42	20	8
9	4	15	54	21	3
10	1	16	28	22	1
11	19	17	23		
12	27	18	26		
	52		203		21 = 276

Towns. His observations upon the first appearance of the catamenia among females residing in towns extend over 205 individuals; of whom 160 came from the north, 40 from the central portion, and 5 from the south of France. The mean age presents a slight difference to the previous scale. "This difference, although inconsiderable, ought to be noticed, as showing that the effects of the towns are beginning to manifest themselves." (p. 107.)

Age of its first appearance.	No. of cases.	Age of its first appearance.	No. of cases.	Age of its first appearance.	No. of cases.
9	1	14	33	19	7
10	6	15	29	20	11
11	21	16	20	21	1
12	21	17	24	22	1
13	18	18	11	23	1
	67		117		21 = 205

"Thus in the towns the mean age at which the catamenia appear is somewhat earlier than in the country. According to our observations, the extreme ages at which this important function shows itself are nine and twenty-three, embracing a period of fifteen years. But this extent of time, especially as regards the first number, must not be considered as the strict limits of the two extremes of this epoch, for it is by no means common to meet with young people

who have menstruated at seven, eight, or even five years of age, of which we will give an instance."

The author has deduced his observations upon menstruation in the *capital* from 203 individuals; of whom four fifths were born in Paris; the others were natives of different parts of France, but had resided at least a year in Paris before the catamenia had appeared.

"Of these 203 females, 171 belonged to that class of people in narrow circumstances who had been compelled to seek assistance from the hospitals. The mean age of these 171 is fourteen years and a little more than ten months, which agrees almost exactly with the numbers deduced by MM. Marc Despine, and Bouchacourt. We are struck by the height of this number; for it appears that menstruation here is more tardy than in towns of the second magnitude, and even in the country. But this result, so different to what might have been expected, is only so in appearance, everything resolves itself into order when we analyse the elements of which this number is composed. There exists in large capitals a class of females which, from their manners, habits, mode of life, and organization, are distinct from the population from which they have sprung, and resemble, to a certain extent, the richer classes, from whom also they differ in other respects. Females of this class (which we have called '*metis*,' of civilization,) menstruate sooner than the others.

Age of the appearance of the Catamenia among the 171 females of the above class.

9	2	14	24	19	4
10	5	15	27	20	3
11	18	16	22	21	1
12	9	17	19	22	4
13	20	18	12	23	1
	54		104		13 = 171

"Hence it appears that the highest number is fifteen years, which scarcely differs from the preceding, since we may consider that the common age is comprised between the ages of fourteen and fifteen years."

Among the class of females which the author has denominated "*metis*,"* the mean age at which menstruation appears is somewhat lower than in the preceding class; and his observations on those belonging to the nobility, mercantile, and burger classes, show that the catamenia make their first appearance still earlier here. These investigations lead us to the following interesting results :

The mean age at which the catamenia make their first appearance—

In the poorer classes is	14·842 or 14 years and 10 months.
In the class called "*metis*"	14·405 or 14 ,, 5 ,,
In the richer classes	13·660 or 13 ,, 8 ,,

From the whole of his observations, as to the first appearance of menstruation in the capital among women of the three above-mentioned classes (being a total of 359), it results that the mean age is fourteen years six months. The author then gives us his grand total of the 1200 individuals on which his entire data were founded.

Table of the first appearance of menstruation in 1200 females.

5 years	1	12 years	105	19 years	35
6 ,,	0	13 ,,	132	20 ,,	30
7½ ,,	1	14 ,,	194	21 ,,	8
8 ,,	2	15 ,,	190	22 ,,	8
9 ,,	10	16 ,,	141	23 ,,	4
10 ,,	29	17 ,,	127		
11 ,,	93	18 ,,	90		
	136		979		85 = 1200

* Mulatto, or half-caste, also mongrel, i. e. *intermediate*.

The mean age in this table is 14·842.

"It is lower than that of Manchester, which is 15·191; and higher than that of Marseilles and Toulon, which give together 14·015. So that we see in this case that latitude produces a very sensible effect, since the difference as to the epoch of menstruation at Paris and at the two southern towns, taken together, amounts to about one year."

In summing up the principal points of the first chapter, M. B. de Boismont comes to the conclusion already deduced from common observation—

"That menstruation generally appears later in the country; that it appears earlier in the towns, especially in the manufacturing ones; but that the maximum in this respect is in the capital. Although this becomes in effect a law, there are numerous exceptions to it. Thus the lower classes of the population, exposed to all the privations in the train of misery, contain a large number of young girls in whom the catamenia appears very late. On the contrary, those young females whose habits and mode of life bring them under the class which we have called the *metis* of civilization manifest this function at an earlier age. But it is particularly among the young ladies of the wealthy classes, of the nobility, &c. that the menses are found to appear soonest." (p. 114.)

He then investigates the effects which difference of *temperament* has on the period at which the catamenia first appear. From the examination of 477 females of the lower classes, he shows that menstruation appears earliest in those of the sanguine temperament, viz. at 14·578; and latest in those of the lymphatic temperament, viz. 15·381.

In order to determine the effects of *constitution* upon this function, he divides 746 females into four classes or grades, beginning with those of a robust constitution and terminating with those who are delicate: the results show a marked difference as to the age at which the menses appear:

"In the first division the mean age was observed to be			14·520
In the second	,,	,,	14·706
In the third	,,	,,	14·807
In the fourth	,,	,,	15·470" (p. 116.)

"The colour of the hair has also its influence, as well as the form of the body. Thus light hair and hair of a dark chesnut are found among women who menstruate late, whereas the brunettes are those in whom the menses appear earliest. Menstruation commences sooner in small women than in those who are tall." (p. 118.)

According to M. de Boismont's observations, the number of females in whom menstruation is accompanied or preceded by general or local symptoms is greater than of those in whom this is not the case, in the proportion of four and a fraction to one.

"The daytime seems to be the most favorable period for the appearance of the menses in females of a robust habit, or who take plenty of exercise; at least, it is during this period that we have observed menstruation appear most frequently in women of this class. This evidently results from walking, from their occupation, labour, exercise, and greater activity, which leads to the conclusion that well-regulated exercise must necessarily promote the coming on of the menses. In delicate women who use less exercise, and among those who menstruate easily, this function appears to occur more readily at night." (p.127.)

The author investigated the *duration of the menstrual period* in 562 females, with the following results:

In 35	cases the catamenia lasted	1 day.
62	,, ,,	2 days.
119	,, ,,	3 ,,
78	,, ,,	4 ,,
46	,, ,,	5 ,,
21	,, ,,	6 ,,
12	,, ,,	7 ,,
172	,, ,,	8 ,,
17	,, ,,	9, 10, and 15 days.
562		

Hence it appears that the ordinary duration of the menses is comprised between one and eight days, and that the two periods during which the largest number of females menstruate are the eighth and the third days.

In examining the effects of marriage, pregnancy, parturition, and lactation upon the function of menstruation, we meet with a variety of results more or less interesting. Of 25 married women in whom menstruation had been irregular for some time, more or less, 14 became perfectly regular after marriage. The not uncommon appearance of the catamenia during pregnancy and lactation, so contrary to the old-established but erroneous law, has also been noticed by M. de Boismont:

"In 8 cases we have seen the menses appear during the two, three, and four first months after conception, in 3 instances they continued during the whole period of pregnancy. The same remarks are applicable to lactation; for if, in by far the majority of cases, the menses are completely suppressed, it is nevertheless true that they do appear under certain circumstances. Of 27 cases which presented anomalies of this nature, we noticed that they reappeared in 2 cases after six weeks of nursing, 4 times after four months, once after five, 3 times after nine, and once after eight months. In 12 cases they continued during the whole period of suckling. In the majority of these observations the health of the infant was not affected. In one instance, where the catamenia appeared regularly every three weeks during a nursing of twenty months' duration, the milk was like turbid water, and the weak, feeble child gradually sunk. Another female menstruated during eight pregnancies, but the milk was always serous and poor, and all her children died between the ages of four and five years." (p. 133.)

The effects of parturition upon catamenia are also well worthy of notice; and here again we have to thank the author for some highly valuable observations:

"Of 82 women who furnished us with distinct data as to the time at which the catamenia returned after labour, we may make the following classification:

1	menstruated	almost immediately after labour.	
1	,,	8 days	,,
2	,,	15 ,,	,,
4	,,	3 weeks	,,
9	,,	1 month	,,
38	,,	6 weeks	,,
7	,,	5 to 6 weeks	,,
7	,,	2 months	,,
6	,,	3 ,,	,,
2	,,	4 ,,	,,
3	,,	5 to 6 months	,,
2	,,	7 to 8 ,,	,,
82			

"The period, therefore, from six weeks to two months, is that at which the

catamenia most frequently reappear, but it ought not to excite surprise if they are suppressed for three or four months." (p. 134.)

We cannot agree with the author where he goes on to say that if the menses do not appear by this time, there is reason to fear some affection of the uterus and its appendages; because it is a well-known fact, at least in this country, that women not only frequently pass a much longer period without their reappearance, but even go from one pregnancy into another for several successive times, so that actually some *years* have intervened without any return of the catamenia, and yet without the least injurious effect on the woman's health.

A recent analysis of the menstrual fluid by M. Bouchardat has been quoted by the author. Great precaution seems to have been used in order to collect it pure. A speculum was closely fitted on to the cervix uteri, in order to prevent any admixture with the secretions of the vagina, and by keeping it applied for six hours, about an ounce of fluid was obtained. The results of M. Bouchardat's analysis are as follows:

Water	90·08
Fixed principles	6·92

The fixed principles consisted of—

Fibrin, albumen, colouring matter	75·27
Extractive matter	0·42
Fatty matter	2·21
Salts	5·31
Mucus	16·79
	100·00

The elements of this analysis are identical with those of arterial blood. A portion of the same menstrual fluid was examined microscopically by M. Donné, who states that it consists—1st, of ordinary blood-globules, with their proper characters, in very considerable quantity; 2ndly, of mucus from the vagina, consisting of scales of epidermis furnished by the mucous membrane of the vagina; 3dly, of mucus-globules furnished by the cervix uteri.

The author confirms the observations of M. Nauche, viz. that the secretions of the vagina in a healthy person are uniformly acid. It is also acid in women after labour; it becomes alkaline when it is glairy, and is the product of inflammation; if the affection be confined to an isolated spot, the secretion from this spot is alkaline, while the other portions of the canal continue to secrete a matter essentially acid. From the consideration of the whole subject, M. Brierre de Boismont arrives at the following conclusions:

"That menstrual blood does not differ from arterial blood; that the varieties, as regards proportion, depend on circumstances connected with the individual; that the presence of blood-globules is incontestable; that the mucus which it has been supposed to contain, belongs to the uterus, its cervix, and to the vagina; that the acidity of the mucus accounts for the contradictory opinions of different authors." (p. 137.)

We confess ourselves disappointed to find that the *object* and *design* for which this function has been established in the human female have been so entirely lost sight of by the author. With such a mass of facts and observations bearing upon every feature of this function, we had

hoped to have been favoured with many valuable and practical conclusions. He omits, or alludes very imperfectly to the fact, that diminution, irregularity or even suppression of the menses may occur without any disorder in the functions of the uterus, and may depend on a variety of causes, which act by lowering the tone and power of the system; in such cases the menses are, as it were, a safety-valve, and appear or not, according to the fulness or deficiency of its circulation. He does not seem aware that the catamenia constitute a discharge evidently designed to control the redundant supply of nutrition which is furnished during the generative life of the human female, when the powers of reproduction are at their highest degree of development. A mere accumulation of facts without a close and searching investigation of the circumstances under which they occurred, may be useful in a statistical point of view; but, practically speaking, they are of comparatively little value, because we are thus left in ignorance of the various causes which have determined the existence of these facts—the effects of them.

M. de Boismont has calculated the mean duration of the menstrual life of a woman from the results of 188 cases, and places it between twenty-eight and twenty-nine years, which does not differ materially from that which has been usually adopted, viz. thirty years.

The author's observations on leucorrhœa present nothing of particular interest: mere statistical details, without noting the state of the patients' health at the time are of little value, and lead to no practical results. From the reasons which we have already given, his summing up of the causes which influence the varieties of the catamenia, is extremely vague and unsatisfactory, and leads the reader to no clear or defined notions on the subject. Many facts of no peculiar novelty or rare occurrence are mentioned with an air of importance which would have led us to expect a few practical observations from the author in explanation, and which would doubtless have rendered their enumeration much more valuable. Thus he observes: "A great variety of circumstances may be observed in the same individual from the employment of medical treatment. We recollect the case of a lady in whom leeches were applied to the epigastrium, in consequence of an affection of that organ: the menses were immediately suppressed. The same person some years afterwards, being ill, had upwards of 160 leeches applied at different times, and, in spite of an enormous loss of blood, the menses at the next period showed no diminution of the ordinary quantity." (p. 156.) Without a single explanatory remark, such observations to a beginner are unintelligible; and to one already acquainted with the subject, they are both meager and valueless.

Some of the author's observations on dysmenorrhœa are sound and very deserving of notice:

> "Dysmenorrhœa must ultimately induce that condition which will lay the foundation of uterine disease. We have seen where a profuse loss removed all the symptoms which it had occasioned. M. Lisfranc has well observed that the violent attacks of menstruation which constitute one form of dysmenorrhœa, after being continued for ten, fifteen, or twenty years, must necessarily produce a marked effect upon the uterus. He has convinced himself that painful menstruation is hereditary; and that if we ask patients suffering under this complaint, we shall find that other females of the same family have suffered in a similar way, and died of uterine disease." (p. 160.)

It is interesting to observe the effects of monastic life upon the menstruation, for it corresponds very closely with what we so frequently meet with in the boarding-schools of our own country. "It is seldom that we do not, after a few years, meet with a very considerable diminution in the quantity of catamenia," is the author's remark; we wish we could say that this was the only derangement which this function suffers by the worse than foolish system of education which is pursued in our fashionable institutions for female education. Few girls are many months at a boarding-school before the catamenia become sparing and irregular; with the due accompaniments of deranged digestion, unhealthy and constipated bowels, chilblains, and general loss of tone and vital energy, dysmenorrhœa, with all its terrible penalties, is but a too frequent result, and surely paves the way, if not to incurable barrenness, to frequent abortion, broken-down health, and ultimately organic disease. The general dyspepsy with more or less gastroenteric irritation and attendant leucorrhœa are so many effects of the same powerless and atonic state of the system, and have been duly noticed by the author. For the other effects of monastic life on the female he must speak himself:

"The critical age is seldom dangerous. Female recluses live a long time in a state of continued indisposition: a long life with habitual bad health is their lot. Cancer and also acute diseases are rare. Phthisis is very common. Death appears chiefly among patients of this class. The symptoms of phthisis may be carried to a great extent. Laennec has mentioned that in a convent where the discipline was very severe, the nuns and novices had the mind constantly directed to the punishments and retribution of another life; continued disappointments were instituted to curb the mind: suppression of the menses came on, and in two or three months afterwards symptoms of phthisis made their appearance. In a provincial town where we once resided a convent was established on a new system, the members of which, almost all of them young, distinguished themselves by their zeal and the rigid observation of their rules. In the course of one year ten of these young devotees had become victims to pulmonary consumption. Dysmenorrhœa, catamenial colics, hysteralgia of the same nature showed themselves during the early part of monastic seclusion. There were few of the recluses who had not some organic disease of the skin, alternating with lesions of the digestive functions and headach." (p. 164.)

The author enumerates several points of treatment, most of which we cannot but approve of, although we think he has omitted the most natural and important of all—fresh air and exercise.

"Rest, the horizontal posture, tranquillity of mind, and absence of every species of excitement are sufficient in a certain number of cases. If vascular congestion is evident, we must have recourse to abstraction of blood. When the nervous excitability is very distinct, the employment of antispasmodics and sedatives is indicated. Opium combined with diffusible stimuli has rendered great service in these cases. Laudanum in enemata has been very useful. We have seen in some nervous and lymphatic habits that hot wine with sugar has relieved these acute sufferings, as if by magic." (p. 165.)

A long and minute enumeration of the causes of chlorosis is of little use if not accompanied with sound and enlarged views of practical deductions based thereon. There can be no doubt but that chlorosis arises from general functional torpor and derangement, the result as well as the cause of feeble and irregular development. The powers of nutrition are nearly at a stand still, the blood is considerably altered in its qualities, and the whole system having sunk to the lowest stage of atony,

makes but a fruitless struggle to maintain its various functions even in an imperfect state of action. As the author has referred to the Cyclopædia of Practical Medicine, we regret he has not benefited by the simple and highly practical articles on these subjects by Dr. Locock. We hardly agree with him when he states that dysmenorrhœa is less frequently met with in married women. In this country it is by no means unfrequently met with among married women, especially those of a gouty or rheumatic habit, where the uterine functions have been a good deal deranged by abortion.

In speaking of the influence which the appearance of the menses exerts upon existing diseases, the author observes: "Among the lesions which the catamenial period excites or tends to advance more rapidly, must be mentioned pulmonary consumption. We have observed it ten times, with a variety of symptoms. In every instance, with one exception, the disease was announced by hæmoptysis. . . . . . Many of these females informed us that they had constantly observed the attacks of hæmoptysis to be more constant and profuse at the menstrual periods, as well as before and after." (p. 189.)

M. de Boismont has proposed a question of deep interest, not merely in a statistical point of view, viz. What is the proportion of uterine affections among the deaths at the critical time of life? "We have heard," he says, "a celebrated professor state that he considered the number of females at Paris who were suffering under uterine diseases to be about 20,000. This estimate, not being founded on any precise statistical results, cannot be considered as very exact; but still it will not surprise the medical practitioner." (p. 192.)

How far this proportion is correct we have no means of proving: we can only state that the per centage of deaths among females in London from diseases of the generative organs is far beneath such an estimate. Thus, in the Third Annual Report of the Registrar-General for 1841, the mean annual mortality per cent. under the head of these diseases is only 0·46; and from a variety of circumstances, we have good reason to know that the proportion of uterine diseases in Paris far exceeds that in London. The following paragraph from the author not only in some measure explains this fact, but gives a lamentable view of the want of principle and morality of the French metropolis.

"Of 721 women who have formed the basis of these observations, 373 had had children, and 81 premature expulsion; so that, of the number of women delivered, a little more than a fourth part had not carried the produce of conception to the full time. Of these 81 cases, 25—nearly a third—might be considered as the results of abortion. This proportion is enormous; but yet it cannot excite surprise. From ignorance of moral principles, from want of good education, a number of young women suppose that they are committing no crime in destroying the existence of what they consider to be a shapeless and lifeless mass. Others under the influence of shame, prejudice, or naturally vicious, seek the assistance of that crowd of wretches who flourish in large towns. Misery, debauchery, certain positions in society also contribute to render the crime of infanticide more common." (p. 192.)

The influence of the "critical age" upon the life of females, is a question of much importance. If we were to pin our faith upon the popular and sometimes, too, professional belief, we should imagine it beset with dangers. But if we consult the best authorities, we find that which certainly

agrees with our own observation, that the attendant hazards of this particular period are very much exaggerated. From the 43d degree of latitude to the 60th, says Benoiston de Chateâuneuf, that is to say, upon a line extending from Marseilles to St. Petersburg, passing by Vevai, Paris, Berlin, and Stockholm, we find, from the ages of thirty to seventy, no increase in the mortality of women but what naturally arises from the increase of years. The same inference follows from the experience of Muret de Vaud. Déparcieux,* too, comes to the same conclusion, and so does La Chaise.† It appears, indeed, from various statistical records, that the age of from forty to fifty is really more "critical" for men than women, whatever may be the kind of life the former may lead. "The question, then," says M. de Boismont, "appears to be definitively determined, that the 'critical' age does not increase the mortality of women." Still we may be allowed to add, it will always be "critical" to the ladies, in the most mortifying sense of the term; for though to them life may last, their beauty vanishes, or, at least, diminishes.

II. Pathology of Menstruation. Upon this part of the subject we shall refer to the octavo work, which enters rather more into details than the quarto memoir. And first, we must be allowed to touch briefly upon one or two points that are not adverted to by the author. We know no subject upon which medical practitioners are more closely pressed and questioned by females of all ranks, than upon this of menstruation; and none upon which young practitioners are more likely to betray their inexperience, especially to women of the upper classes. This may occur even in the mere mode in which enquiries are made by the practitioner respecting menstruation, or from his not comprehending the very equivocal hints which gentlewomen so frequently give as to the regular or irregular performance of the function. If, however, from want of professional association with this class of women, these hints are not at once understood; if the young practitioner, as we have often known to be the case, drives his patient to come directly to the point, which she would willingly avoid, the inference is immediately and not unjustly drawn, that he is a novice in the medical management of ladies of "gentle degree." For example: how often does a female, either from real or affected modesty, in answer to the very common question of, "Have you taken the medicine prescribed?" reply, "No; I did not think it would be right?" To the man of moderate experience this hint is sufficient. He knows at once that his patient is menstruating. But a tyro in such matters will not be at all instructed by this evasive answer, and will show by his manner that he knows not why his directions have not been obeyed. On the other hand, to females of the lower ranks, the medical enquirer will often find it necessary to put his questions in a very homely shape, or *they* will not understand *him*. Ask them "if they are *regular*," they answer, "Yes." "How often?" "Every morning," we have more than once known to be the reply, the question being supposed to refer to the state of the bowels. The young practitioner must also ever bear in mind that whatever may be the disease or ailment under which women are labouring, they are always strongly inclined to consider it as the effect of even the slightest irregularity of the menstrual

* Essai sur les Probabilités de la Vie humaine. † Topographie médicale de Paris.

function. And although such an opinion is, for the most part, founded on prejudice and imperfect observation, it is one not slightly to be set aside in practice.

Trite as may be the first remark the author makes upon *amenorrhœa*, it conveys an important truth that is too frequently forgotten. When a young female has arrived at the age of puberty, as a general rule, menstruation takes place. But this is not always the case. And if her health does not suffer, active medical interference is improper. But how often are mothers very anxious, and how often does the practitioner very improperly comply with their wishes, for the administration of some of the tribe of "forcing" remedies, when the girl, though a woman in age, is still a child in form and general development. Nothing can be more injudicious than this too common practical error.

The general description M. de Boismont gives of primary amenorrhœa claims no particular notice. We may observe that Morgagni mentions several cases of females who died without having ever menstruated, and in whom the uterus was found remarkably small and undeveloped. We have now before us a specimen of this kind. The uterus, taken from a young lady of sixteen years of age, is quite in a rudimentary state: not larger than that of a female of five years. She had not menstruated. How much mischief would be done in such cases by emmenagogue or stimulating remedies. We cannot, during life, detect this retarded development; but our practice would be guided by the childish or "backward" appearance of the patient, even though she had arrived at the age of puberty.

"Blueness of the skin," says the author, "was observed in two cases of suppression of the menses." Cases are related by Leutin, Trotter, and Marcet, in which a blue colour of the skin appeared suddenly, in a single day, after a sudden and complete suppression of the menses; the discoloration remained for six weeks, at the end of which time the patient died. A case of a similar nature was seen by Marc and others. In the first case we refer to there was no organic mischief of the heart or the respiratory apparatus. In neither of the cases was there any preternatural communication between the arteries and veins. The author suggests that this discoloration of the skin takes place in the same manner as in asphyxia, or after narcotic poisoning, as by opium. It must be confessed, we believe, that we cannot, as yet, explain what is the modification that the vital action undergoes in such instances. "Certain medicines sometimes cause suppression of the menses. We have several times known amenorrhœa to occur after the administration of copaiba and cubebs." (p. 306.) "It is often very difficult to ascertain the cause of suppression. In 30 cases we could not detect it." (p. 337.)

The account of *dysmenorrhœa* will be regarded as meager and unsatisfactory by any one acquainted with English writers on the same subject. We have upon a former occasion (Br. and For. Med. Rev., vol. II., p. 180,) offered a few remarks upon the treatment of this disease, which further experience has corroborated.

The description of *chlorosis* is respectable, and does not materially differ from that of Dr. Ashwell, (Br. and Fr. Med. Rev., vol. II., p. 175,) and other writers. We cannot say, however, that sufficient precision marks the directions as to the treatment to render them useful guides to

those who require help from others. Several facts mentioned by the author confirm the pathological views of the nature of chlorosis, to which we have formerly adverted. (Br. and Fr. Med. Rev., vol. II., p. 176.) "The grounds for considering chlorosis a general affection, dependant upon a change in the quantity or qualities of the blood, are striking." In the ordinary state, the proportion of the serum to the coagulum of the blood is as 5 to 8 ; this proportion of these constituents varies, however, with many individual peculiarities. M. Jolly* has not seen a single case of chlorosis or anemia in which the proportion of serum did not exceed seven tenths of the whole mass of blood. In one instance it was nearly nine tenths. Still, however, M. de Boismont is inclined to the opinion that chlorosis is sometimes a local disease; that it reacts upon the general economy. He founds his belief upon the following facts: Practical experience affords us unequivocal evidence that in certain young females, menstruation is solicited in vain by tonics, steel medicines, or emmenagogues, and that it occurs after marriage or pregnancy. In his general remarks upon the influence of these occurrences in the physiological part of the work he has related some cases in point. Frank relates similar examples. The direct stimulation of the genital organs suffices to dispel the symptoms. Young females with all the signs of chlorosis are cured by sexual intercourse, without any medical agent. Chlorosis sometimes exists in women who are perfectly "regular." M. de Boismont gives examples of this rather unusual occurrence, for as a very general fact, it may be asserted, that if the chlorotic state be well marked, the menstrual function is either not performed at all, or at least very imperfectly. Difficulties, too, present themselves when we enquire into the nature of this disease. Is it always an asthenic disease? Ought it to be regarded as a variety of anemia? In the present state of our knowledge, the author is of opinion that we cannot assert that it is always asthenic. He has seen young women with all the symptoms of the disease, in whom the colour of the skin, the constitution, and temperament all indicated strength and power. Such a form of the malady has not escaped the attention of other physicians. Frank very confidently admits a species of sthenic chlorosis. Professor Wendt, of Breslau, has denominated this species by the title of "*chlorosis florida seu fortiorum.*"† Stoll, too, observed cases of chlorosis from plethora, which were very distinct from the ordinary species, and occurring in country girls accustomed to hard labour. Hoffman makes the same remark, and he occasionally had recourse to bleeding. Mason Good,‡ too, we may observe, divides chlorosis into two species, the atonic and entonic. Dr. Copland, we think, very correctly comments§ upon these attempts to establish this "sthenic or entonic" form of chlorosis in these terms: "This is an unnecessary refinement, no phenomena which warrant such a distinction presenting themselves in practice. Indeed, the entonic only consists of a state relatively of less deficiency of vital power than the atonic, and is in many cases merely the first stage of the disease ; particularly when it occurs in tolerably strong females, and whilst the torpid function has not as yet extended much further than the sexual organs in which it originated, the

* Jolly. Du siege, de la nature et du traitement de la chlorose. Rev. Méd. Dec. 1839.
† Rust's Magazine, vol. xlv. ‡ Study of Medicine. 2d edit. vol. v. p. 110.
§ Dictionary of Practical Medicine. Part i. p. 315.

digestive, assimilating, and vascular organs not having sustained much disorder." Of the truth and practical importance of these remarks we have no doubt, and sure we are that if in this "entonic form" of the disease we come to the conclusion, that "copious and not unfrequently repeated venesections are necessary,"* we shall very quickly have cause to regret our departure from the more commonly received notions of the nature of the disease and the treatment it requires. In a word, we shall by this debilitating treatment hasten the disposition that exists in all such cases to the establishment of a state of complete atony, with cold and perhaps œdematous extremities, and a crowd of other evils, that every practitioner of moderate experience must know to be the usual concomitants of the chlorotic condition, when it has lasted long enough to exhaust the sum of vital power that may and nobody denies does exist at the very beginning of the disease. M. de Boismont is quite right in deprecating the belief that steel or any other remedy will cure chlorosis without pure air, wholesome food, tranquillity of mind, regular exercise, and other obvious and appropriate auxiliaries. We must not be too rigid in the dietetic management of chlorotic girls. Capriciousness of appetite is a marked feature of the disease. Very often it is difficult to induce them to eat at all, and they may be fairly and properly allowed to indulge their taste within reasonable limits, even if the articles of food they fancy should be opposed to our general principles.

The chapter "de la déviation des regles," *vicarious menstruation*, contains several remarkable cases. Examples of such deviations from the ordinary course of nature are far too common and well known to induce us to dwell long upon the subject. We are tempted, however, to give the following very striking case. We have before merely referred to it (vol. II. p. 180). Gardien† quotes the same case nearly word for word from M. Brulé. Our author gives it from the *Médecine Pratique* of Pinel:

"Miss A. had been subject to attacks of hysteria from the age of eleven, which were followed by vomiting of blood. She menstruated at fourteen; her health was re-established, and the catamenia continued to flow regularly for several months. A sudden fright suppressed the menses, and again hysteria came on. Vicarious menstruation now occurred. The legs swelled and were covered with vesicles, and during six months blood was regularly discharged from them. The left arm swelled and the legs recovered, and for a year there was a regular sanguineous discharge from the arm. A third deviation occurred from the left thumb which had been slightly wounded. The 'menses' flowed from this opening for six months. In the fourth year, two wounds were formed on the face, from an attack of erysipelas; one upon the side of the nose, the other on the upper eyelid. For two years the periodic discharge took place from these openings, and it no longer occurred from the thumb. The abdomen, in its turn, was attacked with erysipelas, and for five months regularly there was a discharge from the navel at each menstrual period. For four months the discharge proceeded from the inner ankle of the left foot; for two months from the left ear; for three from the left nipple. When the discharge did not flow from any one part, bleedings at the nose, and vomitings of blood, preceded by convulsions, pains in the head, and giddiness took place. After remaining some time at the Saltpétrière, the health of this young female improved, and regular menstruation was established.—In another instance the young lady's

* Mason Good, loc. cit. p. 111.

† Traite complet d'Accouchemens, t. i. p. 238. 3me ed.

nose itched very much; it swelled and became red, and for several years blood was discharged from the nostrils regularly every month."

The circumstances of this case were accurately noted by Dr. Piedagnel. These examples are sufficient to instruct those who may not have seen instances of vicarious menstruation, of the curious deviations that occasionally occur. Spitting and vomiting of blood are more common, and once more, in reference to the latter kind of vicarious menstruation, we venture to quote from ourselves, to keep in mind the fact, that harmless as such cases in general are, they have been very unexpectedly fatal. (Br. and For. Med. Review, vol. II. p. 180.) We shall never forget the deep impression that the case here referred to made upon our minds, or the surprise of the learned physician who attended with us, when we informed him of the sudden and unlooked-for death of our patient. It is worthy of observation that sometimes vicarious menstruation occurs without any premonitory symptoms; sometimes, according to M. de Boismont, well-marked and very alarming symptoms precede it. Occasionally, too, a periodical diarrhœa has superseded the menstrual discharge during the whole life of the female.* We cannot quit this subject without one more remark. As a general rule we ought not to suppress, even if we can, the vicarious discharge, unless the natural flow of the catamenia is established. If we do arrest the unusual discharge from any other part, we are not certain the natural discharge may take place from the uterus, and thus we should expose the patient to the double danger of suppression of the former and possibly also of the latter. In all such cases we must act upon general principles, and endeavour, by the well known and general means, to establish the normal performance of the function of the uterus.

The subject of *menorrhagia* is dismissed too briefly by the author, considering the title of his work.

Under the head of *the influence of the first menstruation* upon disease, some rather interesting cases are mentioned. As for example, the cessation of violent and general chorea when the menses appeared.

The statistical tables, too, upon the frequency with which cancer of the uterus and polypi occur at the critical age of women are worth consulting. The chapter on the influence of menstruation on acute, chronic, and surgical diseases may be safely passed over. It deals in loose and unsatisfactory generalities.

M. de Boismont has collected with a good deal of care, all, and that does not amount to much, that is practically important respecting the influence of disease on menstruation. Louis, in his researches on Pulmonary Consumption, has noticed the influence of this disease on the menstrual discharge. Esquirol, too, has made some interesting remarks upon the same subject in reference to insanity. We have been recently informed by the physician of a large lunatic asylum that, as a general rule, menstruation is either suppressed or goes on very irregularly in insane females. Desormeaux touches upon the same subject. (Menstruation. Dict. de Med. 2me ed.) The Polish physicians state that during the existence of "Plica" the periodical discharge is frequently suspended, and that its return indicates the cure of the disease of the hair. Irritation of the nipple, from suction of the infant, causes in many women a

* Baudelocque. Art des Accouchemens, t. i. p. 185.

flow of the menses at irregular periods. "It may also cause a prolongation of the flow, in a word, true hemorrhage." (p. 469.) This statement puts us in mind of a remark made by Dr. Rigby, (System of Midwifery, p. 217,) when discussing the treatment of hemorrhage after labour. We would still continue the note of interrogation which we marked in the margin of the book at the time we read the following passage: "Where every means has failed to induce a sufficient or permanent degree of contraction, we believe that the only certain means which remains is putting the child to its mother's breast." This is strongly expressed.

After a return of the menses, any disease under which the patient may be labouring may still run a chronic and even incurable course.

The reputation that M. Brierre de Boismont enjoys as a very practised and practical writer cannot fail to be increased by the publication of this work. In a physiological point of view it is extremely interesting and instructive. By the assistance of his statistical tables he establishes many facts connected with the very important function of menstruation, in a much more striking and impressive manner than had previously been effected by preceding writers. The pathological part is decidedly inferior to the physiological. As a mere sketch it is unobjectionable; but it wants much to entitle it to rank as a complete or satisfactory treatise.

Art. VI.

Transactions of the Provincial Medical and Surgical Association. Vol. X.—*London*, 1842. 8vo, pp. 354.

The annual volume published by this admirable society contains its usual variety, though scarcely its usual amount of information. We have first Dr. Streeten's most excellent *Retrospective Address*, and then the conclusion of Dr. Shapter's account of the *Medical Topography of Exeter*. The former of these does not admit of analysis, being itself a condensed analysis; the second we shall have another opportunity of noticing.

The two following papers—that entitled "Observations on the climate of Hertfordshire, compared with that of the neighbourhood of London," by Lieut. Col. Philip Yorke; and the very excellent "Medical Topography of Shrewsbury," by T. Ogier Ward, M.D.—must be passed over for the present.

Case of a pin passing from the appendix vermiformis into the bladder. By Dr. Kingdon. This is a curious case. The passage of the foreign body left a fistulous communication between the viscera, through which several worms passed, and escaped by the urethra, occasioning the little patient much suffering. A drawing of the parts is given.

Case of a tumour developed in the cauda equina. By Dr. Fisher. The disease originated from an injury of the lower part of the loins. Violent and increasing pains were felt in that region, and at last the patient became paraplegic. On examination after death, a diseased mass was found, entirely surrounded by the divisions of the cauda equina traversed by a few of its nerves. The morbid growth appeared to be developed in the pia mater; the lower portion had the form of a tubercle, presented traces of vascularity in the centre, and had a scirrhous ap-

pearance. The other parts were softer, and were composed of a gray, semi-transparent, jelly-like substance, infiltrated amidst cellular tissue, and marked with sanguineous striæ, several of which appeared like true vessels. The original preparation is preserved in the splendid museum of Cambridge; a collection which is second to none out of London, and does infinite credit to the talents, and industry, and public spirit of the Professor of Anatomy and Physiology, Dr. Clark. The parts are here represented in a plate.

Further observations on the variolæ vaccinæ; with coloured engravings. By Mr. CEELY. It would not be easy to overrate the importance of these investigations, conducted as they have been with a philosophical discrimination and care that are worthy of all imitation; we shall, therefore, without preface, endeavour to present our readers with as full an abstract as we are capable of laying before them.

Cases of variolæ vaccinæ occurring in cows and milkers. Having received information (in October, 1840,) that the tenant of a farm in the village of Oakley, situated at the extreme and N.W. end of the Vale of Aylesbury, had two ruptured vaccine vesicles, caught whilst milking his own cows, some of which he knew were affected with the same disease, Mr. Ceely hastened to the spot in order to enquire minutely into the circumstances. The ruptured vesicles on the hand of the patient (Mr. Pollard, æt. 56, who had never had smallpox or vaccine,) were apparently between the second and third week of the disease. The cows were ten in number, eight milch cows and two sturks. On two of the milch cows there were vestiges on each of not fewer than twenty-five to thirty vaccine vesicles on the teats, and the remains of one on each udder. Two others presented about half that number; and on the fifth there was evidence of only one vesicle on the under part of the teat, which being out of the way of the milker was completely desiccated and entire, forming a characteristic, blackish-brown, oval crust. This crust and two on the udders of the other cows just mentioned were the only perfect ones observed. On the teats all the imperfect crusts had been removed by the manipulations of the milkers, and their places were occupied by florid ulcerations, many of which were manifestly depressed in the centre, and *all* surrounded by the more or less circumscribed, indurated, and elevated integumental boundary which marks the vaccine disease. On the udder of one of the affected milch cows was observed an abundance of the sub-epidermic vesicles or bullæ, which not unfrequently arise during the acme and after the decline of the vaccine disease. The remaining three milch cows had perfectly escaped, and so had one of the sturks; but the other, which proved to be in calf, had several dark-brown crusts on the teats. Another milker was also affected, as will be noticed presently.

Mr. Pollard, the proprietor of the cows, spontaneously stated it to be his opinion *that the animals had been infected from human smallpox effluvia*, and the investigations instituted into this very important point brought to light the following circumstances. It was known that smallpox had been casually introduced into the village where this farm was situated about the commencement of the preceding June, but being promptly met by vaccination, only twelve cases occurred up to the time when the cows exhibited their disease. The last three cases were a woman, aged

forty, who had been satisfactorily *inoculated* in infancy by the celebrated Sutton, a young child, and a woman rather beyond the middle period of life. The cottages in which they resided during their illness were situated on each side of, and closely connected with, a long, narrow meadow, comprising scarcely two acres. The first-named patient, though thickly covered with pustules, was not confined to bed after the full development of the eruption; but frequently crossed the meadow to visit the other patients, the woman and child, the former of whom had the disease in its malignant, confluent form, and who died on the 7th September, and was buried the next day.

"On the following day the wearing apparel of the deceased, bed-clothes, bedding, &c. of both patients were exposed for purification on the hedges bounding the close; the chaff of the child's bed was thrown into the ditch; and the flock of the deceased woman's bed was strewed about on the grass within the close, where it was exposed and turned every night and for several hours during the day, till the 13th September—seven days. On that day the above-mentioned eight milch cows and two sturks were turned into this meadow to graze. They entered it every morning for this purpose, and were driven from it every afternoon to be transferred to a distant meadow to be watered and milked, where they remained during the night. Whenever the cows quitted the meadow in question in the afternoon the infected articles above mentioned were again exposed upon the hedges, and the flock of the bed spread out on the grass and repeatedly turned, where it remained until the morning, when the cows were re-admitted. It appears, however, that the removal of the infected articles was not always accomplished so punctually as had been enjoined; for both the proprietors and the milkers affirm that on one occasion, at least, they observed the bed-flock on the grass, and the cows amidst it, and licking it up. The proprietor positively declares, and the milkers corroborate his statement, that the animals were in perfect health on their first entering this close; but within twelve or fourteen days of that event five of the milch cows appeared to have heat and tenderness of the teats, upon which, embedded in the skin, were distinctly felt small, hard pimples, which daily increased in magnitude and tenderness, and in a week or ten days rose into *blisters*, and quickly ran into brown and blackish scabs. At this period, when the teats were thus *blistered* and swollen and very tender, the constitutional symptoms were first observed, viz., sudden 'sinking' or loss of milk, drivelling of saliva from the mouth, and frequent inflation and retraction of the cheeks, staring of the coats, 'tucking up of the limbs,' and 'sticking up' of the back, and rapid loss of flesh. The process of milking was now very difficult, disagreeable, and even dangerous; and on the 14th October, the middle of the third week the detachment of the crusts and loose cuticle, and the abundant discharge of pus on attempting to milk, compelled the milkers to desist for the purpose of washing their hands. Soon after this the cows became by degrees more and more tranquil, as the tenderness and tumefaction of the teats subsided." (p. 214.)

This narrative is particularly interesting. The simultaneous occurrence of the disease in all the subjects (of which the state of the teats bore evidence,) undoubtedly points to the existence of *one* common cause: for it must be borne in mind that, although the vaccine is epizöotic, and attacks one or two cows at *different farms* about the same time, it never, so far as Mr. Ceely's experience extends, has been known to occur simultaneously in so large a number on one farm as in the present instance. Another corroborative circumstance is the fact of the young sturk being affected, because of course the disease could not have been casually communicated to it. But if there were one common cause, where shall we

look for it if not in the existence of the variolous effluvia, to which they were all alike exposed, and which certainly would have been quite adequate to the production of smallpox in the human subject? It will be observed also that three of the milch cows escaped altogether, though in every respect situated as the others, and equally liable to have been accidentally infected in the process of milking. This fact, taken in connexion with others of a similar nature, have led Mr. Ceely to the opinion that there exists among cows, as well as among men different degrees of susceptibility to the vaccine.

Cases of casual vaccine in the milkers. We shall pass over the first case (Mr. Pollard), because the patient was not seen until the vesicles had burst. The second is more fully narrated. *Case* 2. Joseph Brooks, æt. seventeen, a fine, healthy intelligent young man, who had never been the subject of either variola or vaccine, commenced milking four cows, *one* of which only had the disease, on the 9th October. During ten days he milked six times, the animals having been attended in the intervals by another milker, who had been vaccinated with variola-vaccine lymph, and did not become infected. On the tenth day (October 18th), he felt the cervical glands and lymphatics stiff and tender; and on the 20th found a pimple on the tempora-frontal region, which he could not resist scratching. On the day before that he observed a red pimple on the finger, and next day another, very small, on the thumb. He was not aware of the pre-existence of any injury of the cuticle in those points. On the 21st he had headach, general uneasiness, and pain in the back and limbs, with tenderness and pain along the lymphatics and glands, particularly in the axilla. These symptoms increased until the 23d, when nausea and vomiting took place, after which they gradually declined. The vesicles (of which representations are given,) presented the ordinary appearances, and went through the course which is usual in such circumstances, the centre of each sloughing, and the resulting deep ulcers healing slowly. A small quantity of lymph was obtained from the one on the temple by removing the central crust, and patiently waiting for its slow exudation. It was perfectly limpid, and *very adhesive.* This lymph was used upon the human subject with complete success, both in the primary employment and in the subsequent transfers; the local and constitutional symptoms varying in accordance with the age and temperament of the patient, and the texture and vascularity of the parts. Several of the patients thus vaccinated were tested at the expiration of three months with variola by inoculation, and presented nothing more than a trifling fugitive inflammation at the point of insertion, or a small vesicle, resembling the modified vaccine in form, size, and course, containing a *limpid adhesive* lymph, raised on an indurated base and terminating on the eighth or ninth day in a small, hard, blackish-brown crust, and unattended throughout its course with any constitutional symptoms.

Into Mr. Ceely's remarks upon the early stages of the casual vaccine disease in the cow we need not again enter, having fully noticed them in a previous number, (Brit. and For. Med. Rev., X. 465;) but we should be neglectful of our duty did we fail to impress upon our readers the necessity of a careful attention to the *ulcerative stage*, which has hitherto been less closely observed, but is worthy of much investigation, because it is clear that in many cases our opinion of the existence of the true

disease must be chiefly founded upon the phenomena of this period. The difficulties attendant upon this part of the diagnosis are undoubtedly very great, and much time will probably elapse before they are in any considerable degree lessened; but the cause is not hopeless so long as patience and perseverance continue to characterize the members of our profession.

The general points of recognition of the vaccine disease in its other stages being borne in mind, we shall often be able to remark some of their vestiges in the stage of ulceration; but the extent to which this is practicable must depend upon many contingencies. There is nothing so distinctive in the site, figure, or size of the vaccine vesicle as to render the nature of the resulting ulcer (when the crust and all other traces of the original affection have been entirely removed,) at once apparent; more especially when two or three ulcers have been coalescent, or thickly grouped with intervening erysipelatous vesications, in which case the ulcers are generally of large size and irregular shape. True vaccine ulcers are generally distinguishable by a rounded elevation—more or less manifest—of their outer margins, and a circumscribed induration—of greater or less extent—of their base, with a proportionate depression in their centres, sometimes caused by a slough. But these characters are obviously influenced by several circumstances, such as the stage of the disease, the texture of the tissues, and the site of the ulcers; and it should be also remembered that the ulcers resulting from the severer forms of other vesicles may present nearly the same appearances, especially if the tissues are lax and if they have been subjected to much mechanical irritation. If, however, we can be positively assured that the above-mentioned diagnostic conditions have existed in any ulcer for three or four weeks, or longer, especially if it be removed from *severe* casualties, and of a small size, we may fairly presume that it is vaccine. Still it is evident that uncertainty may yet remain, seeing that these are mere questions of degree; but the difficulty is often removed by finding a group of characteristic ulcers on the same animal, or on a series, clearly infected from one another; because such a condition of parts rarely occurs in connexion with the ulcers of other contagious vesicles.

All the cows in this farm were shortly afterwards attacked with the *epizoötic aphtha*, which had long prevailed in the Vale, and which we must briefly notice for the sake of contrast with the description of the vaccine disease given above. This epizoötic seems to be a specific, febrile, eruptive affection, with catarrhal and rheumatic characters, comprising a cold, a hot, and a critical or eruptive stage. Its invasion is generally sudden. The cold stage is very short, and *often unobserved;* the stage of febrile reaction is quickly followed by patches of inflammation on the mucous membrane of the lips, gums, cheeks, tongue, and fauces, and about the nostrils and mouth, with congestion of the conjunctiva. These red patches quickly run into irregular vesications or bullæ, containing a limpid, pale, amber-coloured fluid, and are attended with profuse salivation, difficulty and even temporary inability to masticate, with proportionate constitutional disturbance. Similar vesications are often seen on the teats, especially on the involutions of the skin at their apices, and not unfrequently mammitis results from inflammation of the milk-tubes. Still more frequently they are seen on the skin, where it

joins the claws as well as the interdigital membrane, whence they often extend under the horn, creating much mischief, and causing excruciating pain by its detachment, &c. The constitutional symptoms continue for four or five days, when the topical irritation abates and mastication is restored. It attacks all ages, and is often suddenly fatal to the very young, excessive pulmonary and cardiac congestion being found after death. In the milch cow there is a notable diminution of milk; and in this respect there is a marked difference in the effects of the disease we are now considering and of the vaccine. The quantity of butter lost in the first week of the prevalence of the *aphtha* was thirteen pounds, and in the second week seven pounds; while during the first week of their illness from the vaccine the loss was only six pounds, during the second two, and during the third none.

In the month of June of the next year, Mr. Ceely had the opportunity of investigating a number of cases of *variolæ vaccinæ* at a large dairy farm in the village of Dorton. The disease had existed there for at least three months, and out of forty-eight cows only three had escaped. We shall not enter into any details, excepting so far as to notice the varieties of cicatrices which were observed, and which are of importance in reference to the above-given descriptions of the ulcerative stage. Of the *perfect* cicatrices, some few were irregular, puckered, and uneven; but the majority were regular and well-defined, being oval or circular, and of various sizes, some scarcely perceptible, others as large as a chesnut; their outer margins were slightly elevated and gently rounded off, and their bases a little indurated. On a coloured skin the *whole* of these parts were obviously without pigment. Of the *imperfect* cicatrices, some were irregular and puckered, but most had small, florid, granular, and depressed centres. Some had small, bloody, black, thin, and flat crusts in their centres; and others, again, had in the same place small, brown or blackish brown, thin, secondary, or tertiary crusts. In most of these cases the elevation of the rounded margin and induration of the base was more marked, especially when they were large and irregular.

Two milkers, who had been formerly vaccinated, received the disease in a mild and modified form; and a young man, who had never had either variola or vaccine, was also infected. His case is detailed at great length, and representations of the vesicles in their different stages are given. We have not space for the details, nor for the results of vaccination with the lymph derived from this subject; but we must beg attention to the following experiment, which appears to us peculiarly interesting:

"Vaccination with the lymph taken from the milker on the 4th of June was performed on two boys, brothers, aged respectively eleven and six, both very fair, with light hair, blue eyes, *red tarsi*, and consequently thin, irritable skins. *On each subject* two vesicles were raised with this '*Dorton*' lymph, two with the '*Oakley*' lymph, and two with the '*variola-vaccine*' lymph. They all advanced in both subjects, *pari passû*, and could not be distinguished either before or after the eighth day. In the younger boy slight headach arose on the *seventh day*. Both complained of local and general symptoms when the areolæ were extending on the *ninth day*. There was nothing remarkable in the size of the vesicles on the tenth day, when the disease was at its acme, and the areolæ were extensively diffused. Eleventh day: areolæ below the elbows. Twelfth day: the elder boy had a very bad night, arm much swollen and inflamed; the younger had a good night, though his arm was as bad as his brother's. After this

period the general symptoms abated; but all the turgid vesicles burst the thin cuticle, the corium sloughed, ulcerated, and threw up loose, spongy granulations not easily repressed." (p. 248.)

The *variola-vaccine lymph* has been so extensively employed, and with such complete success, that little doubt, we should apprehend, can remain upon any mind as to its identity with the genuine vaccine; but for the benefit of those who may yet have some lingering apprehensions we think it right to mention that it has been submitted by Mr. Ceely to the test of variolous inoculation on twenty-two subjects, and that the results were precisely similar to those described by Drs. Jenner and Willan as following the same test after vaccination with the original virus, viz. the production of vesicles closely resembling those of modified vaccine at the point of insertion, occasionally some constitutional irritation, and in one instance a mixed, papular, and papulo-vesicular eruption, analogous to the vesicular lichen sometimes observed after vaccination.

We shall conclude this article with a caution that no lymph should be employed which is not *quite limpid and adhesive*,* and a word of comfort for those who are fearful of employing that which has been recently derived from the cow, on account of their dread of the violent local irritation, but who will perhaps be set at rest when informed that Mr. Ceely has seen "as much local inconvenience in some subjects from the *oldest* as from the *newest* lymph."

The perusal of the present paper only confirms our former estimate of the singular merits of its author. He seems to us to possess the highest qualifications of a philosophical interpreter of nature; and had the Provincial Association no other proofs to adduce of its usefulness—and it has hundreds—the investigations of Mr. Ceely alone would be sufficient warrant for all the labour and all the outlay it has occasioned to its members.

The last paper in this volume is a *Report of Cases at the Chester General Infirmary*: by T. B. Peacock, Esq., which is far too condensed already to suffer any further compression at our hands.

Art. VII.

Ueber das Verhältniss der Medicin zur Chirurgie; und die Duplicität im ärtzlichen Stande, &c. Von Dr. Ph. Fr. von Walther.—*Carlsruhe und Freiburg*, 1841. 8vo, pp. 48.

On the Relation of Medicine to Surgery; and on the Division of Medical Practitioners into two Grades, &c. By Dr. Ph. Fr. von Walther, Physician in ordinary to the King of Bavaria, Professor in the University of Munich, &c.—*Carlsruhe and Freiburg*, 1841.

It is certainly encouraging to medical reformers, and especially to those who advocate the reunion of medicine and surgery, to find two physicians to royalty in their ranks, namely, Sir James Clark, Bart., and Dr. von Walther, (or rather the Right Honorable Dr. von Walther, for

* M. Dubois of Amiens has stated that efficient concrete vaccine lymph may be recognized by its presenting a beautiful reticulated appearance under the microscope. This appearance, however, would not seem to be diagnostic, for Mr. Ceely has observed it in the *concrete* lymph of the *verrucous vesicle* of the cow, a quite different affection.

that gentleman is a privy counsellor), physician in ordinary to the King of Bavaria.

We propose noticing the views of Dr. von Walther at length; but to afford the means for a better understanding of them, we will state the circumstances which have led to their publication. During the end of the last century and the beginning of the present, medical reform was much agitated in Germany, and questions raised precisely similar to those now agitated in Great Britain. The propriety of reuniting medicine and surgery in teaching and practice, and of having but one degree of general education, attracted the greatest attention. In 1797 and 1798 the Electoral Academy of Useful Sciences at Erfurt published the following prize questions:

"Is it necessary and possible again to reunite medicine and surgery in study and in practice?

"What were the causes of their separation, and by what means can they be reunited?"

The arguments of thirteen candidates for the prize were in favour of the reunion of medicine and surgery, and were similar to those brought forward at the present time; while one only took up the opposite side of the question. The essay of this individual was published with sketches of the other essays sent in.* It is to be observed, however, that Dr. Jugler, the prize essayist, does not really grapple with the questions, but gives a sort of *tertium quid* of his own, so contradictory and inconsistent in its details, that it is matter for surprise how the palm could have been justly adjudged to him. Various writers followed on both sides, amongst whom Professor Reil was the most remarkable. Reil dedicated his work to the celebrated Hufeland, chief of the Medico-Chirurgical College at Berlin, and advocated with all possible enthusiasm the education of a class of illiterate practitioners for the country folk which were to be *Routiniers.* The *Routinier*, like the scientific physician, was to be naturally clever; but like the boy-mathematician, or the working land-surveyor, only able to act by rules, and not to think about them. He was to know the varieties of disease, but nothing of their causes or the *modus operandi* of his remedies. Petty surgery and practical midwifery he was to have at his fingers' ends; he was to be a good herbalist, and know cheap remedies; he was to learn something of toxicology; to know more than a smattering of materia medica, chemistry, and pharmacy, and to be well versed in the natural philosophy of man, both in health and disease. He was, of course, to have, preliminary to all this, an adequate elementary education; and, as the crowning qualification, he was to be thoroughly vulgar, and exactly resemble the common people in his dress, conversation, mode of thinking, &c., that they might have the greater respect for him.† Reil was serious, as is shown by the host of opponents he raised for

* Gekrönte Preisschrift über die von der Churfürstlichen Akademie nützlicher Wissenschaft zu Erfurt aufgegebene Frage: "Ist es nothwendig, und ist es möglich, beide Theile der Heilkunst, die Medicin und die Chirurgie, sowohl in ihrer Erlernung als Ausübung, wieder zu vereinigen? Welches waren die Ursachen ihrer Trennung, und welches sind die Mittel ihrer Wiedervereinigung?" Von J. H. Jugler, M.D. &c.—Erfurt, 1799. 8vo.

† Die Pépinière zum Unterrichte ärzlicher Routiniers, als Bedürfniss des Staats; nach seiner Lage, wie sie ist. Vom Prof. Reil.—Halle, 1804. 8vo.

several years after the date of his publication. C. W. Hufeland was one of these, and had no difficulty, as may be readily supposed, in exhibiting the absurdity of Reil's plan; but, singularly enough, he himself a few years after, proposed that the clergy should practise medicine as *routiniers*, a folly little more absurd than the recommendation not long since advocated by a weekly contemporary, that druggists should be allowed so to practise, but a folly actually carried into effect by the Swedish Diet in 1809. In Germany, secondary schools for the education of an inferior grade of practitioners (Landärtze, country doctors,) were established at Brunswick, at Berlin, in Bavaria, and in Austria. Our author raised his voice against this system in 1807, and now after an experience of the results for thirty-four years, and with ample opportunities for observing, he comes forward and expressly declares that these inferiorly-educated general practitioners have done incalculable mischief in Bavaria; and adds, that loud complaints to the same effect are made in Prussia. It is, then, to remedy these evils and to secure for the public a supply of skilful and learned general practitioners that Dr. von Walther has in view by the publication of his pamphlet.

The leading ideas in this absurd or, we may almost say, wicked system are, that a surgeon is necessarily something distinct from a physician and of an inferior grade; that the ill-educated surgeon is quite good enough for the provincial clodhoppers; and that men of superior attainments or graduates (promovirte ärtze) could not be got to live amongst them or attend them. Dr. von Walther traces the origin of these ideas historically, and as the question is one of considerable importance in medical reform, we will trace them with him.

Dr. von Walther justly observes that medical practitioners of one kind or other are to be found in all communities. They are indigenous in those which are savage or only half-civilized, and their knowledge is indigenous too. Such was the profession in Germany, France, and Great Britain not many centuries ago. In Germany the first trace of medical practitioners, as a distinct class, appears in the thirteenth century, when the bathmen, or attendants at baths, and the barbers practised medicine, and constituted two distinct guilds or corporate bodies. It does not appear which was the superior grade, corresponding to the pure physician, we suppose the bathmen; but it is certain that they quarrelled much; were guilty of breaches of etiquette, that is, of poaching on each other's domain; formed associations; and at last in the fourteenth century, were united into one guild, becoming in fact one-faculty men; and, swallowing up the autochthonic doctors, gave their name to the united profession. To this day the country people in remote districts in Germany call their medical practitioners "Bäder" and "Feldscheerer," bathmen, and army-barbers.

These were unlearned physicians or general practitioners; men who were masters of their craft, and taught it to their apprentices; but a formidable body of learned physicians began to appear at the time the two primary constituents united into one guild, and when the knowledge of Greek literature spread from Italy through the monasteries of France and Germany. At first it was considered disgraceful to study medicine, as many people think it to be now; but the monks found it to be profitable; so the study and practice of medicine and surgery extended amongst

them in spite of the denunciations of the two popes, Alexander III. and Honorius III., until the fourth Lateran council, by special edict, positively forbid the clergy to practise surgery, and declared that every priest should be degraded who shed blood. The bathmen and barber-surgeons with their indigenous lore remained, of course, quite distinct from the monk physicians and their exotic literature; for being unacquainted with the classics, they could learn nothing of Greek or Latin medical science, and so became the inferior, because the illiterate class of practitioners; while the ecclesiastical physicians confined themselves to the neighbourhood of monasteries, the courts of princes, and to large cities, and were the doctors of the higher orders of society.

The edict against the shedding of blood was more effectual than the papal bulls; but if the *patres* could not practise surgery, the *fratres* or lay-brethren might; and so another class of practitioners sprung up, the *monk-surgeons*, of which Frère Cosme and Jacque Beaulieu, in France, and Pravetz, in Germany, were the most distinguished. The universities of Italy and Spain (to meet the new demand for surgeons created by the edict), also sent out educated surgeons as well as physicians, under the title of *Chirurgi Physici.* These took an oath not to treat inward complaints; they were generally ambulatory, and never formed themselves into guilds or colleges, but associated rather with the learned physicians, the clerical and lay university-graduates, than with the unlearned practitioner, the bathmen and barbers, who knew little of operative surgery. The masters in surgery were not numerous, and as the pure physicians now thought it undignified and degrading to practise surgery, a great deal of minor surgical practice fell into the hands of the bathmen and barbers. These retaining their guild, assumed the title of chirurgeons, to place themselves on an equality with the masters in surgery, the pure surgeons of the age. The latter were also termed "chirurgeons of the long robe," and the former "chirurgeons of the short robe," but these last still continued to practise as general practitioners, like the surgeon-apothecaries of our own times.

The modern divisions in the profession were now established, but with serious detriment to surgical science, for Vogel von der Vogelweide could find no one near the Rhine or in the neighbourhood of Worms to operate for a simple harelip; so that he was compelled to travel into Thuringia to consult a famous "cutty-sark" chirurgeon, who gave him dreadful pain, allowed him to bleed for upwards of twenty-four hours, and sent him home, more disfigured than before. Nor was this all; for the two grades of practitioners just mentioned soon came in collision, and the long-robes and short-robes, quarrelling with each other, like their predecessors, the bathmen and barbers, formed associations. But at the same time they also came into collision with the physicians, who now became their oppressive enemies, and drove them into union as one faculty. The age of guilds being passed, colleges were founded in cities where the chirurgeons of the long-robe were sufficiently numerous, as in London and Paris, for example, and educational institutions were attached to them. But these institutions derived an unlearned character from the great numerical majority of illiterate short-robes in the profession. The students took no heed to the humanities; and science, the learned languages, and

the medical classics were despised. Those who have been admitted members know well that in practice this is the character of the London College of Surgeons to this day; and we are firmly of opinion that nothing has tended to degrade the surgeon in the eyes of the public so much as this want of a good preliminary education in the members of that college.

The chirurgeons of the long-robe, however, became more influential and more scientific wherever these colleges were founded; and the science and art of surgery were advanced in proportion. But in Germany the elevating element, the learned chirurgeon, was not sufficiently powerful to establish colleges, so the old barber-guilds remained, and surgical science was temporarily retarded. Little evil resulted, however, from the absence of this reintegration of the profession, for in a few years the great truth that surgery and medicine are one, arose out of the confusion; and Germany, sooner than any other country, exhibited accomplished physicians, who were also accomplished surgeons, while the more enterprising and talented of the barber-surgeons came little behind these in medical skill. Langenbeck of Göttingen, we understand, was one of the latter class, and Richter, Siebold, and Loder of the former, as also Van Swieten. Still the old divisions remained; the "feldscheerer" or barber-surgeons were uninfluenced in the provinces, and the surgical physician only partly superseded the "pures" in the towns and cities. But the establishment of the new class was a great step in the right direction, and, better still, it was the practical demonstration of a great principle. As such, it had an immediate effect, and thinking men soon saw that if such a class was valuable in towns, it would be doubly valuable in the provinces where the general practitioner was so lamentably ignorant. The outcry for an improved system of medical education to which we alluded at the commencement of our remarks, quickly followed (an outcry reechoed at the time in England), and the ideas then developed terminated in the institutions for educating that inferior class of surgical physicians for the country which Dr. von Walther declares has inflicted incalculable mischief in Bavaria. It should be here observed that these new institutions were the result of a compromise between the old learned and unlearned institutions; and that, as on previous occasions, the new received the hue of the unlearned majority.

Dr. von Walther infers from these historical facts that the profession was bipartite in Germany from extraneous circumstances, but particularly from the inequality of education in the general practitioners of the day, and the consequent division of the profession into learned and unlearned. That there was no such division in practice in ancient times is evident from the Greek and Latin classics. Hippocrates, Galen, Oribasius, Ætius, Alex. Trallianus, Paulus Ægineta, Ebn Sina, and Abulcasem, all made no distinction between medicine and surgery. It is certain that the operations of surgery, including obstetric surgery, were taught in the medical school at Alexandria; but they were taught as specialities, just as the use of the stethoscope, ophthalmic pathology, or any other special branch of medicine is taught in modern medical schools. In Germany (as we have already stated), the propriety of uniting medicine and surgery has been long practically demonstrated,

and not less so in France. But it is a remarkable circumstance that the old divisions of the profession produced the same results in that country as were observed in Germany, the Officiers de Santé being an inferiorly educated grade, corresponding to the landärzte. The College of Surgeons of London is the only remains in England of the barber-guilds, and in conjunction with the College of Physicians, has contributed very materially towards maintaining the old professional grades in this country. But Dr. von Walther observes that nothing but the wildest Anglomania could recommend the adoption of such singular anomalies as are seen in England, and that to establish a college of surgeons in Germany would be actually to retrograde. We agree with Dr. von Walther. The medical profession has a higher destiny than to discover minute nervous fibrils, rectify distortions, or mutilate human beings. As regards the education of physicians and surgeons, Dr. von Walther observes that the three following propositions are generally acknowledged:

1. That an accomplished physician ought to have studied surgical operations, and, if corporeally or mentally disqualified for performing them, or if without the requisite manual dexterity, ought to have a competent knowledge of surgical diseases and of the operations by which they may be cured.

2. That a good operating surgeon must be well educated in every branch of medicine.

3. That it is impossible to have a class of persons, to whom the physician shall indicate what operations are to be performed, and who will perform them as a mere mechanic under his direction.

It will have been observed that the idea of inferiority in amount of education is attached to the word surgeon, and the cause of this will also have been evident. Now, however, the scientific surgeon ranks with the accomplished physician, and the advocates for a divided profession shift their ground, and say that an inferiorly educated practitioner is necessary for the people generally; but why this practitioner should be called a surgeon does not appear. Having demonstrated that the education of physicians and surgeons should be alike, and their academic *status* equal, Dr. von Walther proceeds to discuss this question, whether it be necessary to have practitioners for the poorer classes of inferior attainments and education. He justly remarks that the notions respecting the simplicity of the diseases to which the poor are liable—that they are more surgical than medical, and consequently that a learned physician is not necessary for them,—arise from an undercurrent of money-making aristocratic pride, and are simply the fancies of a few medical hierarchs. We believe this foolish pride has materially assisted in keeping up the evil of inferior grades, and for proof we need only refer to the conduct of the two London Colleges which are (or but the other day were) *de facto*, little more than two clubs made up of a few of these same medical hierarchs. Midwifery is an ungentlemanly branch of practice, consequently those members of the College of Surgeons who practise obstetric surgery are too degraded to be eligible for a seat in the council; but the physician declares *all* surgery to be degrading. Provincial people are vulgar, therefore the college of physicians have repeatedly declared, and

still declare officially by their unrepealed regulations, that a practitioner of inferior grade and of attainments inferior to their own, is quite good enough for the vast population existing outside that charmed circle in which they themselves reside, and the radius of which is precisely seven miles. That old corporations, the lifeless *debris* of the middle ages, should cling to follies like these is not wonderful, for the attachment which all bodies of men show to their prejudices and selfish interests is always most infatuated; but, that individuals should dare to come forward and endeavour to advocate and promulgate such wretched doctrines would be incredible, if melancholy experience did not assure us that there is nothing be it ever so foolish and wicked, which has not its advocates.

Disease and death attack all men alike, whether they be rich or poor. Standing between man and his last enemy, the physician in this respect is the impersonation of that Divine Providence which is good to ALL. In the exercise then of his godlike duty he must know no difference of rank or station, nor must he think anything mean or anything degrading which enables him to fulfil satisfactorily his high mission. The pettiest and most revolting services he can render to humanity are ennobled when rendered with this object in view. The assertion that an inferior practitioner can call in the aid of a superior in cases of emergency is not valid; for in the first place, it often requires consummate skill to distinguish these cases of emergency, and, secondly, in actual practice and at the bed-side, the physician is self-dependent; he cannot await instructions from a superior officer; there he must act on his own responsibility and act too, without delay. All other subordinate professional men who receive orders from superior officers, whether in the military or civil service of the state, in the church or in the law, are educated; but the man upon whose unaided decisions the lives of hundreds may depend, need not (it is argued) have a scientific education; in other words his mind need not be cultivated, his faculties trained, his judgment enlightened. And, also, because the health and ease of man is in immediate relation with all nature, and the science of medicine thereby complicated as no other science is; it is better, say the advocates of an inferior grade, that the general practitioner, to be practically useful, should know nothing of science whatever, or at least as little as possible. By parity of reasoning a Priessnitz is a more useful physician than a Prout.

Vain pride and stupidity are so often allied in the same individual, that to demand a good preliminary education in all candidates for admission to be medical students would be useful, if the only object gained by it were the exclusion of those dunces from the profession, who are now the arrantest of quacks, and at the same time the most illiberal and most exclusive of practitioners. The individual without the mind to comprehend the physical and natural sciences ought never to be intrusted with the application of those sciences to medicine. An individual so constituted should not be permitted even to enter upon the professional curriculum of study, and so a loss (to him) of time and capital would be prevented. It is upon the mental qualifications and training that Dr. von Walther very correctly lays the greatest stress. If the physician have a scientific and classical education, while that of the surgeon is neglected, the former will be the better practitioner, even with less ex-

perience and a shorter period of professional study. The objections that well-educated practitioners will be too proud to attend the poor, or not sufficiently numerous, are easily set aside by Dr. von Walther, so far as Germany is concerned. The sons of mechanics, day-labourers, and small farmers, (as more than two thirds of the medical students in the Bavarian Universities are,) are not very likely to be overburdened with pride, especially when five sixths of them consider their profession as a mere trade by which they are to be fed. As for the fear that the supply of learned physicians would fail, it appears the graduates are so numerous that every large village might have a doctor, but for the illiterate country physicians or landärtze.

Upon taking a general view of the history and development of the profession, it appears that *inequality in education and privileges* has been the principal source of the various evils we have noticed. The tendency has continually been to the union of medicine and surgery in practice, and to equalization of rank ; the inferior class pressing upwards to get on a level with the superior, and the two classes in continual broils until that level was attained. Each time that this event took place was marked by a step forwards of the united profession in scientific knowledge and in social position ; but the united profession has invariably been temporarily depressed from the position attained by the highest grade, and its literary character has received a tincture from the lower. In proof of these statements we find the bath-men and barbers quarrelling until they form one guild ; then the short-robes or barber-surgeons, and long-robes or literate surgeons are in continual broils until a union is compelled by the attacks of their common foe, the pure physician ; and lastly, in Germany the physician and surgeon unite, and the rapid advancement of the profession is only arrested by the unwise step taken about thirty-five years ago, to provide that inferior grade of surgical physician to which the more enlightened practitioner is now opposed.

The progress of the profession in England has been similar. It is true there is and was a triple division into grades, but the two lower, the surgeon and the apothecary (or druggist) quickly coalesced, yet not until there had been warm disputes, and frequent appeals to parliament, just as the general practitioner now complains of the druggist. Still the surgeon-apothecary was an uneducated man, and the same demand for an improved system of medical education was made as we have noticed in Germany. This reform agitation was headed by Dr. Harrison and others, but their scheme of education embracing the unnatural tripartite division in the profession, necessarily fell to the ground. Nevertheless the change which had taken place on the continent went on also in England in spite of all obstacles. The pure surgeon soon equalled the physician in his preliminary education, and became, as he is now, a physician in practice, corresponding in every respect except the name, to the continental graduates. The apothecaries' act of 1815, carried into effect the principle of having an inferior grade of practitioners, already acted on in France and Germany nine or ten years previously ; and the surgeon-apothecaries were now metamorphosed into surgical physicians of an inferior class. But the tendency upwards which raised this class from shopkeepers to professional men, continued still to operate.

The apothecary examiners gradually extended their curriculum of study until, in conjunction with that of the college of surgeons, it equalled in extent the *medical* curriculum of any university whatever, and exceeded that of several. On the other hand a knowledge of the greatly augmented medical sciences was required from university graduates, rather than an acquaintance with classics and mathematics, until by the disuse of the Latin language in the examinations and the formalities of graduation, the latter process became assimilated to the examinations at "the Hall and College."

We assert, then, that in England the two grades of the profession are now nearly on a level,—are at the point of fusion, and that the struggle for equality of title, rights, and privileges is coming to a close in the establishment of one faculty. Convinced that we are right we feel anxious to point out the evils which history warns us to avoid. In the first place care must be taken to prevent the less literary and more numerous grade giving its own character to the collegiate or educational institutions of the united faculty. The course of preliminary education must be extended and most strictly insisted on. In the second place, no inferior grade of practitioners must be tolerated. If the druggist be allowed to practise petty surgery, and treat "simple" cases, as thoughtless people recommend, only a few years will elapse before he become a general practitioner; in a few years more it will be found necessary to educate him to fill an inferior grade in the profession, and in another generation he will be a surgical physician, and nearly on a level with the superior grade of practitioners. Thus the profession will pass through another cycle of strife, confusion, and interruption to the quiet progress of science, and at the termination of it, we shall only be in the position to accomplish that which may more easily be done now.

Many of those who oppose the movement towards one faculty, oppose it on the supposition that one faculty will lower the dignity of the profession, and degrade the individual members in society. We think the contrary will take place if the parallel movement towards becoming a body in the body politic be encouraged and maintained. To the corporate opponents of the one-faculty scheme, we would say that the fusion of grades now impending is really inevitable; no law can prevent it taking place, nor can any effort, however well directed and energetic; and we would seriously advise them to consult their own interest and honour, by gallantly turning round and heading the movement. Our arguments will, of course, be unheeded. "They who have power are never in time with their concessions," is the profound remark of an eminent political writer, (Professor Smyth,) and too truly applicable, we suspect, to those who have power in the medical corporations. But while we call upon medical reformers to persevere in their great undertaking, we would press on their attention the advice given to reformers by the same excellent writer, namely "to hasten on to practice and to the real wants and wishes of their fellow-citizens, *as they see them plainly existing before them*, not expecting too much from themselves or others, and above all things, losing no time."

The demand of the greater portion of the population in England is for surgical physicians who dispense their own medicines, although in many

large towns the latter qualification is by no means insisted on. Now, to prevent any misconception of our meaning, in using the term "one-faculty," we would observe that we wish to express by that term an incorporation of medical practitioners, having the same or similar qualification; such a qualification as shall render every individual member of the faculty a fit and proper person to practise as a surgical physician, or in other words, as a *general* practitioner. Yet while we would have him so educated that, like the general practitioner, he might practise every branch of medicine, we would by no means interfere with the established division of labour, demanded also by the public. Consulting practitioners there will always be, as well as operating practitioners; but we would have all to commence with the same general education, as the best basis on which to build up any special branch of practice. If colleges are to be multiplied according to the divisions of professional labour, and made independent of each other, let there be as many colleges as lectureships; and let the aurists, the oculists, the dentists, chiropodists, cuppers, lithotomists, rectum-doctors, spine-tinkers, surgeons, obstetricians, physicians, &c. each have their own petty guild. There is no more reason why there should be an independent college of surgeons than an independent college of accoucheurs. Surgical diseases, properly so called, are scarcely more important and various than diseases of the eye, or of the thoracic viscera. Indeed, it is not improbable, that ere long auscultators will become as distinct a class of practitioners in large cities, as the "pure" surgeons are now; under such circumstances, why object to a college of stethoscopists?

Surely it is time that these impolitic divisions in medicine should cease. Let there be one qualification and one faculty, and within that as many subdivisions as the necessities of the public and the progress of medical science require. That these subdivisions would not be few is obvious from the example of Germany, where the qualification of graduates is uniform, but the divisions of labour much more numerous than in England. That the social rank of individuals would not be altered is certain, because even now it does not depend upon the title of the individual practitioner, but rather on extraneous circumstances, as his wealth, talents, family connexions, and the mode and sphere of his practice. Rank in society and division of labour are both clearly independent of academic qualifications. The division of labour, in teaching, depends upon the progress of science; in practice, on the progress of society in wealth and numbers. A general practitioner could live well amongst a poor and thinly-scattered population when a physician would starve; and a physician might combine operative medicine with general medicine where, as a pure physician, he would have little else to do, than to go about to cards and tea. On the other hand, a physician-accoucheur established among a suitable population, might do nothing else but attend to the ladies, and do well too. By comprising all practitioners in one faculty the equilibrium between the wants and *means* of a population, and the talents and peculiar bias of practitioners, would be established without those conflicts and heart-burnings which do and will continually occur under the present professional divisions. We say nothing of the strength to be derived from such a union, nor of the great advancement medicine would obtain from a concentration of the profession.

Art. VIII.

Die rheumatischen Krankheiten, nach ihrem Wesen, &c. Von Georg Friedr. Greiner, m. d.—*Leipzig*, 1841. 8vo, pp. 215.

Rheumatic Diseases considered in regard to their Nature, Phenomena, Changes, &c., with the most rational Treatment of them, and with especial reference to Diet. By Dr. Greiner.—*Leipsic*, 1841.

In a late Number (XXVII.) we gave a pretty full account of an English treatise on rheumatism. We have thought it might be interesting to our readers to know how the same subject is treated by one of our German brethren. With this view we shall principally confine ourselves to an analytic exposition of the contents of Dr. Greiner's volume.

In the preface, Dr. Greiner points out the frequency, Protean varieties, and gravity of rheumatic disease. He reckons that a third part of the race suffers from the malady, which is not, perhaps, to be regarded as an overstatement, if, under rheumatism, are comprehended the various neuralgic affections, which, though not identical with, are nearly allied to, that intractable and formidable disease.

Clear and succinct accounts are given, in succession, of the local affections, as rheumatism of the shoulder, neck, scalp, sacrum, loins; of sciatica, pleurodynia; of rheumatic affections of the muscles of the abdomen and their aponeuroses; of toothach, facial neuralgia, ophthalmodynia: and then of articular rheumatism. So far, we meet with nothing novel. A brief account of rheumatism of the spinal cord follows, which we quote:

"Particular notice is to be taken of rheumatism of the spinal cord, which presents itself with the following characters. The pain is principally in the cervical region. With that is associated internal uneasiness: the look is timid and staring; the eye feverishly bright; the pulse feverish; numbness of the hand, especially of the fingers, with a sense of creeping or stunning. Then follows increased uneasiness, with flushing of the countenance. Symptoms such as those described seize the extremities; in women, the upper, and in men, the lower, are peculiarly affected. The gait becomes unsteady and bending; the feet seem to dangle. If these evils are not speedily arrested, perfect palsy of these parts takes place, which extends elsewhere. The speech becomes stammering, there is insensibility of the skin; the senses decline; the excretions of the bladder and bowels take place with difficulty. The disease may continue some years ere it destroys." (p. 17.)

From page 18 to 23 are some sensible remarks on the nature and character of rheumatic neuralgia, and on the means of distinguishing neuralgia from rheumatism. The presence or absence of intermissions of the pain and of swelling of the affected part, are the conditions on which diagnosis must be grounded. If there are complete intermissions and no swelling, the case may be pronounced one of pure neuralgia. If there are mere remissions, the case partakes more or less of rheumatic complication: and the coexistence of swelling will leave little doubt as to its nature. Neuralgia may be excited by rheumatism extending along either the neurilema or substance of nerves: and this complication is, according to the author, distinguishable from ordinary rheumatism, by the unusual intensity of the pain, by a gnawing sensation, and by a stinging as if of red hot daggers; these acuter symptoms, however,

which mark the neuralgia, remitting occasionally, more especially when the limb or part is allowed repose. Rheumatic neuralgia is rarely accompanied with swelling, and does not, like ordinary rheumatism, change its place, as from the arm to the shoulder, &c., which difference the author explains on the supposition that such metastases or transferences take place more easily between or along the continuous and extended muscles and aponeuroses, than from one set of nerves to another. The nervous system is therefore never so wholly neuralgic as the muscular and fibrous systems are rheumatic.

At page 24, the metastases are treated of, and here we shall quote the author's account of metastases to the head, (nach dem Kopfe.)

"To the internal parts, which are liable to be the seat of rheumatic metastasis, belong all internal muscular structures, in so far as they are provided with aponeurotic and fibrous tissue: further, all parts, which in whole or in part, possess a fibrous, tendinous, or membranous structure. The following are the parts which require particular attention. The dura mater:—To this membrane rheumatic affections transplant themselves more readily and frequently than we should suspect, either from the actual retrocession of the rheumatism from an outward part, as for example, the Galea aponeurotica, the cervical muscles, or other parts of the upper extremities, or from extension of the primary rheumatic affection. In this point of view, the mixed rheumatico-catarrhal affection of the mucous membrane of the throat, tonsils, soft palate, derives importance, inasmuch as from these parts, the rheumatic affection may extend to the *basis cranii*, seize the ligaments of the *atlas* and *epistropheus*, and may thence reach the cavity of the cranium and extend to the *dura mater*. This latter accident is to be suspected when the guttural pain does not reach a true angina, but manifests itself as a mere fleeting, stinging pain of brief duration; while, on the other hand, noises in the ear, remarkable sharpness of hearing, a sort of double hearing, are perceived; and further, when a painful feeling in the nape, especially on motion, arises; also, when, on turning the head from one side to the other, and the neck is bared, and all external noise hushed, a slight crepitation is perceptible in the nape from the motion of the *epistropheus*." (p. 26.)

He adds that the affection of the dura mater is evidenced, not so much by acute pain, which more marks the affection of the "Galea aponeurotica," as by a slight, dull, uneasiness, not augmented on pressure, or by shaking, or nodding the head, and by a slight trembling of the head in walking, as well as by accesses of giddiness. Thickening of the dura mater, effusion, and, in consequence, increased pain, deafness, double vision, delirium, apoplexy, follow the protraction of such a state of things.

At page 27 the metastatic affections of the viscera of the thorax are referred to, with one unaccountable exception to be noticed presently. At an earlier part of his treatise, (p. 4,) and again here, the author speaks of many obscure and troublesome affections, being really due to rheumatism, either primary or metastatic, though little suspected to be so. There can be no doubt, that, in gouty and rheumatic subjects, a thousand seemingly anomalous symptoms and ailments, are explicable simply from the fact of the rheumatic or arthritic diathesis impressing a peculiar character on every incidental derangement. Rheumatism, for example, and the remark applies to gout, when affecting metastatically a secreting organ, as the kidney in its membranous parts—may betray itself simply or chiefly by a notable derangement of the urinary secretion, and slightly or even not at all by pain. In this way the frequent

and often apparently causeless derangements of the urinary secretion of gouty and rheumatic persons (causeless, since they come on notwithstanding the most punctilious attention to diet,) must be explained. In like manner, rheumatic affections of the bronchi and the ramifications of these, may give rise to dyspnœa, and a multitude of pulmonary symptoms of the most perplexing description. The same may be remarked of the metastases to the diaphragm, to the coats of the intestines and to the abdominal muscles and their aponeuroses; all which may lead to the suspicion of visceral disease. In such cases, a knowledge that the patient is rheumatic will solve a thousand difficulties, and facilitate our diagnosis, when otherwise it would be either most unsatisfactory or else impracticable.

The truly unaccountable omission to which we alluded in the beginning of last paragraph, is that of all reference (here at least,) to either metastatic or primary affections of the heart. At a later part, indeed, that important complication is slightly adverted to. It being neither our duty or design to fill up the author's omissions, we content ourselves with simply pointing out this singular oversight.

At p. 40 the question as to the seat of rheumatism is discussed; and the author, while noticing that there is still a diversity of opinion on the subject, adopts the prevailing doctrine, that the fibrous tissue is the one which rheumatism peculiarly affects. As this tissue is the "groundwork" of all animal structures; as it includes muscle, tendon, articular capsule, cellular tissue, (the latter as " the transition-state of the fibrous substance from its fluid into its solid and organized condition,") mucous glandules and follicles, synovial glands, the internal ligaments of joints, the dura-mater as reflected on the medulla oblongata and spinal cord, and accompanying the nerves in their minutest ramifications, the skin—such being the extent of the fibrous tissue, and, by consequence, that of rheumatic susceptibility, it is obvious that the morbid action of rheumatism may pervade the inmost parts and organs. However, it peculiarly affects fibrous tissue, as organized in tendon, ligament, or capsule; and when muscle is seized by it, the lymphatic effusion which characterizes it is, in contra-distinction to the purulent one of common inflammation, never poured out into the substance of the muscle, but always into the cellular tissue. (pp. 40-3.)

The states of the blood and of the nervous system, which are essentially concerned in the production of rheumatism or produced by its development, are next considered. As regards the condition of the former, Dr. Greiner's general conclusion is, (pp. 44-6,) that the fibrous material (faserstoff), " the vital character of which is determined by a magnetic disposition," not only superabounds in the circulation, in rheumatism, but has also, during the continuance of the disease, " a peculiar tendency to quit its soluble state in the blood, and to organize itself." His account of the share which the nerves have in the disease is not very clear, and seems in some measure inconsistent with the views already stated, as to the fibrous tissue being the sole seat of the affection, (with which, indeed the account formerly given of rheumatism of the spinal cord appears also at variance,) since, after noticing that it may be through the medium of the fibrous sheath that the nerves suffer, he remarks, (p. 47,) " that the nerves are also particularly affected, *are often themselves the*

seat of the disease, and at all times suffer as organs of sensibility, is beyond all doubt."

At p. 50, the diagnosis between gout and rheumatism is stated to consist in the former being a disease of the periosteum, (?) consisting in an error and excess of the secretive function of the capillary vascular system, by which the bony mass is deposited: rheumatism, on the other hand, is a disease of the aponeurotic system, has its seat in the sheaths and tendons of muscles, and consists in a derangement of the action of the vessels and nerves ministering to the secretion of tendon. The two diseases may run into each other; rheumatism sometimes reaching and affecting the more tendinous kinds of periosteum; and gout, on the other hand, occasionally extending from its original and native seat (the periosteum) to the articular capsules and cartilages. Gouty pain continues irrespective of the limb being at rest, and migrates less readily and to less distances than rheumatic pain does. Gout principally attacks middle and old age, and comes on in spring or in autumn. Rheumatism makes no distinction of ages or seasons.

In the author's etiology of rheumatism we do not find anything new. Cold and moisture and changes of temperature are the chief predisposing causes. Thus it is more frequent in temperate than in either the high or low latitudes. Dr. Greiner is of opinion that certain other diseases, "by producing weakness of the nerves and a change in the quality of the blood," which he elsewhere terms "the rheumatic dyscrasy" of that fluid, predispose to the malady. These diseases are gastric, bilious, mucous, intermitting, and particularly miliary fever. (pp. 54-5.)

We shall not introduce to the reader the abstruse and characteristically German speculation into which the author here departs, (p. 55,) when endeavouring to explain the proximate cause and the essential nature of rheumatism. All of a sudden, Dr. Greiner, the course of whose observations has been hitherto remarkable for its sobriety and rationality, abandons himself to the most fanciful, wild, and, indeed, unintelligible hypotheses. We are told of a "cosmo-telluric life-spirit," of a "nature-life," of a "world-life," "a sun-life." "The earth-life," it is stated, (p. 57,) "strives against the solar-life, in order to gratify its own life-impulse to the development of its inherent creative power: and, in union with the sun-life, to institute a relative independent existence, as child and prototype of the world-life. This upward striving of the telluric to the solar life is realized in the atmosphere!" Our readers will scarcely believe that this unexpected and strange outburst of mysticism and extravagance is from the same pen as that, the calm and judicious remarks of which we have been recording. We do not attempt to convey to the reader any idea of this extraordinary episode in Dr. Greiner's work, for the good reason that it is utterly incomprehensible. Happily Dr. Greiner's return to common-sense is as sudden and complete as his aberration from it; and he proceeds to consider a number of interesting topics, which want of space compels us to pass over. We shall very shortly advert to the therapeutic portion of the volume.

The detail of treatment is sufficiently ample and minute: that relating to acute rheumatism occupying from p. 90 to 113. Our notices must be brief and partial. In the stage of "premonitions," entire rest, avoidance of all violent motion, reclination in bed, or on the sofa, strict diet, mild

light nourishment, the eschewing all heating drink and solid food, the use of some gentle salt, as tartrate of borax, in doses of ten or fifteen grains, with the addition of a quarter of a grain of ipecacuan, several times daily, constitute the first parts of the treatment. If the patient be young or middle-aged and plethoric, and the attack be characterized by marked excitement, bloodletting, to the extent of from four to six ounces, may be practised. If an alkaline bath can be taken with convenience, it may be had recourse to. An emetic may be employed, as by its action on the stomachic nerves and through them, on the whole "reproductive nervous system," (by which here and elsewhere the author means the assimilative system,) it may destroy or, at least, modify the rheumatic fever, without either weakening the circulation or augmenting its excitement. The following powder is recommended:

℞. Potassæ Nit. ʒj.
Carb.
Magnes Carb. āā, ʒss.
Sacch. Lactis, ʒvj. M. ft. Pulvis.

A teaspoonful to be taken every two hours; if the patient is of advanced age or debilitated, regulated doses of the spirit of mindererus, as a *scruple*, to which four drops of the tincture of ipecacuan are added, and six or eight grains of Dover's powder at night is the treatment suggested. We have given these details with a view to afford the reader some insight into the author's therapeutic management, in the stage of "premonitions." (pp. 88-9.)

But it is seldom, until the *first* stage of the *formed* disease, that the physician is called in; and, in giving directions as to the management of it, Dr. Greiner quotes a variety of authorities as to the propriety of bloodletting. The general conclusion drawn by him and by them is, that this measure is moderately and cautiously to be resorted to, it being kept in mind that the rheumatic is an inflammation *sui generis*, and differing from ordinary inflammation. A variety of internal treatment, called "cooling" (p. 94), into which nitrate of potass, tartar emetic, carbonate of potass, Glauber salts, tamarinds, enter, is here detailed, but which we have neither space nor leisure to quote. The tincture of aconite, along with that of hyoscyamus, in the almost homœopathic doses, of three or four drops of the former to two of the latter, at night or morning, is also proposed. If narcosis (!) does not follow, the above dose is to be augmented by two drops. If great, but not symptomatic (query, critical?) sweats take place, corrosive sublimate, in the proportion of one grain to ℥j of water, dose fifteen or sixteen drops, three times daily, is alleged to be useful. In the second stage (stadium status), in which there are heavy, but not critical sweats, camphor is recommended in doses of two or three grains four times daily, in an emulsion. Dr. Greiner esteems it an object to arrest the sweats above referred to, and it is partly with this object that camphor is to be employed; but if after a few days those sweats continue, then oleum terebinthinæ or juniper is to be substituted for the camphor in doses of four or five drops.

Here, at last, the author refers to the possibility of metastasis to the heart as well as to the diaphragm. To obviate these mishaps, now apt to take place, he advises, more especially if the pulse of the patient be

full and the excitement great, nitrate of potass, along with small doses of tartar emetic; mercury is to be abstained from. When the circulation is quieted, yet some traces of nervous excitement remaining, digitalis, nitre, and chalk are to be given. The digitalis may be united with tartar emetic.

The indication in the third stage (stadium decrementi), is to aid nature in her curative efforts, by promoting secretion and excretion, and by acting on the nervous system, and by regulating the diet. The physician is to make it an object to *produce* sedimentous urine (p. 104), where the excretion does not spontaneously contain deposit, and the diet is to be strictly regulated. For these objects, the following formulæ are recommended:

℞. Sal. ammoniac. depur. ʒij.
Pulv. gum. Arab. ʒiij.
Camphor. grana x. ad ℈j.
Aquæ fontan. ℥iv.
Oxymel. scill.
Syrup. althææ, āā ℥j.

Dose, a table-spoonful every two or three hours. One or two powders of the following combination are to be taken at night: Pulv. aconiti, grana iv.; sacchari albi, ℈iv. Mix most carefully by pounding for many minutes, and divide into four powders. The use of the aconite is to be regulated, as regards quantity and time, by its production or nonproduction of narcosis.

The treatment of the metastasis is chiefly to be managed by anodyne and anti-spasmodic means, as opium, musk, camphor, and in case of the stomach or pericardium being affected, by epigastric frictions of croton oil or tartar emetic.

Rheumatismus universalis (p. 112) of the acute kind, is to be treated by lukewarm alkaline baths and assiduous infriction of mercury. Rheumatic tetanus, of which no proper description is given, is usually fatal. According to Neumann's experience, who tried camphor and opium, with cupping-glasses and leeches applied in large quantities along the nape and spine, and the freeest general bloodletting, all remedial measures are utterly futile.

The treatment of chronic rheumatism occupies from page 113 to the end, and we can only very briefly refer to it. The two grand indications are stated to be, 1st, to operate on the nerves, and give them a new tone; and, 2dly, to adopt such measures as shall act beneficially on the blood and free it from the rheumatic dyscracy. The former of these two principal indications is subdivided, 1st, into those means which act directly on the nervous system; and 2dly, into those which act indirectly. The production of narcosis is stated as the first among the direct means, and under this head aconite, stramonium, hemlock, solanum, belladonna, opium, and nux vomica, are enumerated and discussed, in the order here stated. Stoerk's recommendation of aconite is repeated and confirmed in cases of sciatica, rheumatic pains of the limbs, rheumatic headach, cramp of the stomach, megrim, &c. (p. 119.) Stramonium is also recommended, though rather on the experience of others than of the author himself, and great caution is inculcated in its employment. Hemlock is described as acting powerfully on the ganglial system, and to solanum dulcamara the same character is given. Belladonna, opium, and nux vomica, are rather

lukewarmly spoken of, and are asserted to have little influence over the peculiar rheumatic derangement of the nerves. Opium is described as not sufficiently "constant and commanding" against the disease.

The metals and earths are the next among the means which act directly on the nerves. Mercury is a powerful agent of this class, operating not only on the nervous, but also on the sanguineous systems, and that in one or all of three ways: 1st, by reducing excitement; 2dly, by determining rapidly to the mucous membrane of the intestines; or, 3dly, by operating on the aponeurotic system, and at the same time improving the quality of the circulating mass. Antimony, iron, copper, arsenic, zinc, are noticed and recommended as occasionally useful. Gold is recommended in the following form and dose:

℞. Auri muriati. gr. vj.
Ext. aconiti
Pulv. herb. aconiti, āā, ℈j.
Pulv. succ. liquir. ℈iv. M.

This mass is to be divided into pills of the weight of one grain each, one pill to be taken morning and evening. The above preparation of gold may also be used in friction, in the proportion of four to five grains to the half ounce of pommade of roses. (p. 136.)

The means by which we act indirectly on the nervous system are as follows: Colchicum, arnica, senega, guaiac, ol. terebinthinæ, camphor, sulphur, sulphuric alcohol, ol. dippelii, cajeput, &c. The *modus operandi* of these is stated to consist in their stimulating the nervous plexuses and functions, producing a salutary influence on the nerves of the muscular and aponeurotic tissues, and an alterative effect on the rheumatic derangement of the nerves, and improving the constitution of the blood. As to colchicum, Dr. Greiner, while admitting its efficacy, is doubtful whether this be due to its veratrine, since this substance acts chiefly on the alvine canal, producing vomiting and purging, and but slightly affecting the urinary secretion. He thinks the oxymel the best preparation of colchicum: arnica, from its property of stimulating the lymphatic system and mucous membrane, and from its "salutary effects on sanguineous stagnation and extravasation," is useful in promoting absorption in rheumatic exudation and swelling. Senega is eligible in similar cases. Guaiac is commended; and of the beneficial operation of the etherial-oleaginous substances, in general, in rheumatism, the following explanation is given. (p. 140.) "Their action is, in some measure, explicable from their constitution; since, consisting in great part of inflammable oxydizable materials, carbon and hydrogen, and containing little oxygen or none at all, they excite in the living frame organo-vital oxydation, and stimulate consequently the nerves of the irritable organ, namely, the nervous plexus of the arterial capillary vascular system, to exalted action." Turpentine is to be employed only after acute symptoms have subsided, and seems to be useful by acting on the skin and on the "nerves of the kidney." Juniper oil operates similarly. Camphor (p. 150) is stated to act as "a moderator, soother, and rectifier of the circulation, particularly of the capillary vascular system." Sulphur is praised, and it is asserted that "it seems, on account of its analogy with hydrogen, and its union with the same, in the organization, to act more on the venous division of the sanguineous system." (p. 153.) Sulphuric alcohol is stated (p. 155) to

be a substance worthy of the greatest attention. It is said to be "a powerful means when the extension of the rheumatic affection on internal aponeurotic parts, especially on the serous tissues, as the dura mater, pleura, or peritoneum, is apprehended, since it acts rapidly on the nerves of the reproductive [assimilative] system, and directs a vivifying action to the periphery of the organism;" by which, we presume, morbific action or metastasis to inward parts is prevented by the peripheric stimulation, as just described.

At page 157, the second grand indication is discussed, namely, that which relates to the blood. This, like the preceding, is subdivided into direct and indirect means. The direct are, fat oils, cod-liver oil, sugar, milk-sugar, whey, milk, soda, potass, chalk. The bringing back the blood from its state of rheumatic dyscrasy to its normal condition is affirmed by the author to be as important as restoring the proper function and sensibility of the nerves; and the medicinal employment of the substances first enumerated, is alleged to fulfil the former of these indications, "by being directly taken into the blood, and partly by being there either assimilated as nutriment, or partly by being unassimilated, and thus acting in a therapeutic manner on the sanguineous mass: by these means we aim at directly changing the rheumatic dyscrasy; also at deoxydating, neutralizing the hyper-oxydated, acrid, acid, nerve-irritating, fibrous matter in the blood, at lessening the abnormal coagulability of this fluid, and at diminishing and diluting its too plentiful fibrous material." (p. 158.) As regards fat oil, the author affirms, that while, on being swallowed, it operates mildly and soothingly on the nerves and mucous membrane, and though occasionally apt to disagree with the stomach, affords, when duly digested, strong nourishment, the impregnation of the carbon-hydrate of the fat with oxygen, in the course of its process through the mesenteric glands and the circulation, enriches the blood with coagulable and fibrous material, producing some excitement in irritable organs; which, however, is compensated for by its withdrawing oxygen from the blood, thereby rendering that fluid less acrid and stimulating. It is consequently useful in cases in which "the rheumatic disease is accompanied with poverty of nutrient material in the blood, and at the same time with a peculiar irritability, with emaciation and a slight degree of fever." (p. 160.) Cod-liver oil is moderately praised, and by no means lauded to the extent which Dr. Bennett's late treatise on it would have led us to expect in a German writer.

We are somewhat surprised at the warmth of the eulogium passed on sugar, as one of the direct means of improving the blood. We pass over the chemical and physiological explanation of its modus operandi, and merely notice that the author strongly recommends it in both acute and chronic rheumatism, where sthenic excitement prevails and antiphlogistic means are indicated. To be useful, it must be taken in doses of from one to two ounces, three or four times daily, in syrups, conserves, or plain. Milk may be advantageously combined with it. Whey, milk, soda are noticed and recommended as occasionally and in various degrees and circumstances, useful in the disease.

At p. 90 et seq. the means which operate indirectly on the blood are considered. Rheumatism may attack two classes of persons: either young subjects, plethoric, and with a rich blood, or else those old and de-

bilitated, in whom the circulating fluid is thin and dyscrasic. In the first of these, the salts of mercury, the neutral salts, nitre, sulphuret of potass, common salt, and soda are to be employed to reduce the fibrinous state and coagulable tendency of the blood : in the second, animal food, leguminous vegetables, farinaceous food (mehl speisen), etherial oils, the resins, iron, wine, aromatic spirit, preparations of sulphur are to be had recourse to. (p. 170.)

The *external* means to be employed in rheumatism are divided in the same way as the internal, namely, into those which act directly and indirectly on the nervous and vascular systems respectively. Among those which are alleged to act directly on the nervous system are the following: magnetism mineral and animal, electricity, galvanism, Perkinism, acupuncture, insolation, cold, heat, hot vapour, hot iron. Of both mineral and animal magnetism the author entertains very moderate and therefore, in our opinion, very just expectations. The former sometimes has relieved temporarily, but seldom or never effected radical amendment. A rather more favorable report is given of electricity and galvanism. Perkinism, an American invention or discovery—as the reader may please to consider it—consists in stroking the affected part alternately with an iron and brass needle; a hundred times with each needle separately, and then two hundred times with both together. This operation renders the skin red; but the author does not state whether this effect be not entirely due to the force of the manipulation. It appears to have dropt into disuse. Acupuncture is more confidently spoken of by the author. Both foreign and British (Copland) authorities are adduced in its favour. It is more particularly suited to local rheumatic affections of neuralgic type or complication. Insolation, which is said to act "purely and universally on the nervous system, and to operate as a vital principle in nature, favouring, by constant desoxydation, the plastic of organic life," may be useful with other means. Cold and heat, applied more particularly by the medium of water, are noticed, and their advantages and disadvantages correctly discriminated. Cold applications in rheumatism are to be very cautiously resorted to; since cold and moisture are the principal exciting causes of the disease; and, if unskilfully employed, are exceedingly apt to cause metastases on inward organs. Cold is more particularly indicated in merely local rheumatism of a neuralgic type. As a general prophylactic against that peculiar sensibility of the cutaneous nerves to changes of temperature, which often predisposes to rheumatism, cold is highly useful when employed by sponging with cold water, followed by frictions. Heat is a more safe and more frequently-indicated means. After the first stage of acute rheumatism, it allays pain and soothes both nervous and vascular excitement. In the chronic form of the disease heat acts as a salutary stimulant of the depressed nervous system, and excites the arterial capillaries. The warm water may be impregnated with stimulant or anodyne substances, as may be necessary. Care is to be taken not to persevere in the use of warm applications beyond the proper time, as thereby debility and atendency to lymphatic tumefaction may be caused. Vapour-baths, with camphor, alcoholic liquors, sal-ammoniac, red-hot iron, the moxa, are noticed; but the peculiar cases in which these measures are eligible or justifiable are not at all, or are not clearly pointed out. We merely

add, that, as further *external* means of *directly* affecting the nervous system, all the narcotics formerly enumerated may be employed as outward applications.

At p. 188, the indirect external means of operating on the nerves are discussed. These are subdivided into three classes: first, those, such as pitch and mustard-plasters, which immediately act on the cutaneous nerves, producing in the dermoid tissue a peculiar modification of its secernent action, or else some other change which by sympathy reaches the inmost and central parts of the nervous system. Secondly, terebinthinate, balsamic, and alcoholic liniments and plasters, which stimulate both the nervous and sanguineous systems, and influence the processes of secretion and excretion. Thirdly, those means, such as the preparations of mercury and antimony, which correct the derangements, at once, of the sanguineous and nervous systems, and produce, through absorption from the dermoid tissue, specific discharges. (p. 195.)

At from p. 198 to 202, inclusive, sea-bathing, native, mineral, and simple domestic and medicated baths are considered. Great efficacy is justly ascribed to several of the German spas, and the subject is sensibly though briefly discussed. Diet and regimen occupy from 202 to the end. Diet is truly stated to be a matter of great importance in the treatment of rheumatism, since, by means of it, both nervous derangement and sanguineous dyscrasy may be moderated or removed. The author divides diet into the austere, the antiphlogistic, the nourishing, the exciting, and the specific. The circumstances in which the first four descriptions are indicated will easily suggest themselves to the reader. The specific diet consists in the use of saccharo-mucilaginous vegetables, such as strawberries, currants, grapes, carrots, turnips; gruels of barley and groats, cresses, radish, and horse-radish, &c. This section concludes with some remarks on the use of wine, which requires caution, but may be permitted to the old, feeble, and cachectic. Regimen, or the manner of life of the rheumatic subject, should be arranged on the general principle of maintaining the nervous system at the highest tone that can be attained without the use of over-stimulation: this end is to be accomplished by regular exercise, avoidance of fatigue, and the use of all the measures already enumerated.

In treating this volume, our aim throughout has been rather simply to present to the reader the views of one of the most recent German authors on rheumatism, than to make his pages an occasion for critical comment. In concluding, we shall briefly notice the faults of the work. It is deficient in general views; there is no pathological anatomy in it; the metastatic affections of the heart and other internal organs are all but entirely overlooked; there is no reference to the iodide of potass. On the other hand, there are not wanting scientific and acute views and suggestions; the treatment proposed is, on the whole, judicious; and, in so far as the work gives us a notion of the curious and somewhat mystical speculations which prevail at this moment in Germany, in regard to the influence of telluric, solar, atmospheric, and other causes in the production of disease, it will not be uninteresting to the English reader.

ART. IX.

1. *Hydropathy, or the Cold Water Cure, as practised by Vincent Preissnitz at Graeffenberg, Silesia, Austria.* By R. T. CLARIDGE, Esq.—*London*, 1842. 8vo, pp. 318.

2. *A Practical Treatise on the Cure of Diseases by Water, Air, Exercise, and Diet, &c.* By JAMES WILSON, Physician to his Serene Highness the Prince of Nassau, &c.—*London*, 1842. 8vo, pp. 202.

AN Austrian peasant among the mountains of Silesia, an unlettered uneducated hind,—in spite of difficulties of situation, remoteness, rudeness, rough roads, and rougher accommodation, and without availing himself of secrecy in his remedy, but on the contrary proclaiming that he uses simple water only,—has induced seven or eight thousand invalids in the course of the last ten years to submit themselves for weeks and months to his treatment; to endure the coarse food of his table d'hôte, the straw mattresses, and barrack-room deprivations of his own dwelling (which is a great hospital), or the still coarser appliances of his mountain village; and to pursue the self-denying routine that he absolutely enjoins, and the discomforts of his applications. He has become notorious throughout Europe, and the publication of the second thousand of Mr. Claridge's work shows that the English public are not likely to be ignorant of his existence. To set down, without inquiry, such a man as a quack, to attribute his reputation and success altogether to the large gullibility of mankind, may be the flattering unction employed by self-love to soothe its less successful efforts, or by indolence to avoid the labour of thought, or by contempt to magnify its own littleness; but is it not rather wiser to admit that such a man must have some ability, that public opinion is not totally wrong or contemptible, that his means must be often successful, and that even "practical men" may learn something from him? For where is there ever a Great Error even, without some truth at the bottom?

The success of such irregular men brings home to us the imperfection of our more regular methods of treating disease. It gives us a glimpse of the region which is still to be opened for discovery; of that ocean of truth lying beyond the shore on which we are now picking up our few shells. For if we could cure all diseases with facility and certainty, *secundum artem*, removing the internal cause of suffering, or restoring the lost power, with the same sureness that the dentist removes the aching tooth, no Austrian peasant would seduce crowds of invalids to visit his rude abode and his wild mountains.

But we cannot do this; and why?

In the first place our art is limited. There are incurable diseases, and there are certain stages at which all diseases are incurable; and as far as books can teach us we are yet deficient in precise rules to guide us as to what diseases and what stages of disease are curable by medicine; to what extent many are curable; and how many may be left to the unaided powers of nature. From this ignorance the practitioner often attempts what is impossible, by subjecting the body to the action of drugs to cure a disease which is beyond their control; or, on the other hand, he neglects remedies from an erroneous belief in their inefficacy; whilst the patient, who is deeply conscious of his own loss of health, after having

subjected himself to every routine plan, often flies to the quack or to the pretender who is the most liberal in his promises.

It is easy to see and to point out defects; but the difficulty lies in ascertaining the remedy. The most striking deficiency in our art is in prognosis (foreknowledge). It cannot be denied, even by the most adhesive "laudator temporis acti," that in diagnosis we have made great progress during the last half century. The works of Corvisart, Laennec, Baillie, Abercrombie, Louis, and Bright sufficiently attest this. But can the same be said of prognosis? Are clear laws, or rules, or maxims, or aphorisms yet made out in sufficient number to enable a man, with any certainty, to foretel the progress of a particular case of disease? And can books teach this? Can such fixed maxims be ever laid down so as to be applicable to diseases which differ in every individual? To a certain extent certainly; but within narrow limits. For prognosis is that branch of medicine which more than any other requires not merely experience but long-continued thoughtful and watchful attention to disease. Whilst experience gives to a reflecting practitioner daily additional reasons for feeling humility for his own deficiencies, for the fallacy of his judgment, the treacherousness of his memory, the want of observation of and attention to minute particulars, and for the little power he really possesses over many diseases; it also affords him satisfaction by enabling him to decide with greater certainty as to what cases are likely to be benefited by medicines, and under what circumstances the "nimia diligentia" is even more injurious than complete neglect. The young, the ardent, and the slightly experienced are too much inclined to believe that their power over disease by medicines is unlimited. With all the skill in the diagnosis of organic diseases (often at an incurable stage) which they have acquired at hospitals, with the knowledge of the principles of medicine which they have learned in books or in lecture-rooms, and of the actions of drugs which they have in like manner studied until their insight seems so clear that all difficulties are over, they have to learn other lessons by the sober certainties of actual practice. The belief of the young practitioner in himself, as well as in his remedies, will be much mitigated when he has been baffled day by day by a slight gastrodynia or a trifling headach; rendered anxious by an obstinate and blemishing eruption of acne on a lady's face; or had to listen week after week to the stale story of hypochondriacal distress, alike exhausting to his patience, his sympathies, and his materia medica. More strongly still will he feel his weakness, after having been compelled to watch the slow growth and the unrelenting progress of an abdominal tumour; or the devastation of a malignant fever prostrating and destroying those whose life seems to those around them of the highest value, whilst all his medicaments are at least powerless, if they be not actually injurious. Some men, indeed, there are, of such buoyant and sanguine temperament, of such incurable elasticity, as not to be depressed, dismayed, nor even taught by any such lessons, how often soever repeated, but whose faith in drugs and in themselves reaches into old age. But there are others, on the contrary, as easily discouraged by failure, and who are apt to sink into entire scepticism. Both extremes, like all extremes, are wrong. The practitioner who has gained real knowledge from his experience holds the middle path, or endeavours to keep as near to it as he can, neither disbelieving

in the power of medicines, nor credulously trusting in their unfailing efficacy.

One reason that books do not furnish us with all the exact information we require as to what diseases and what stages of disease are incurable and what diseases may be left to nature, is the infinite varieties of forms which even a disease to which we attach a single and well-recognized name takes, according to the causes which produced it, the constitution of the individual, its complication with other diseases, or with morbid peculiarities of organization, and the treatment or external and internal influences to which the patient is subjected. In the great class of febrile complaints, which are more regular than any other, the observation of Sydenham is still applicable, that the treatment (and consequently the prognosis) must vary with the nature of the epidemic, and that the physician must discover for himself (and often by losing several of his first patients), the peculiar means to be adopted for the new form of disease: and what is true of an epidemic is especially applicable to each individual case which differs in some degree from every other of the same species. The most carefully compiled statistical tables can no more furnish satisfactory information as to the duration of a particular instance of disease, as to the recovery or death of the patient, than can the best calculated insurance tables inform us how many years each insured individual has to live. The single case is to be decided on its own individual merits; it may be *the* exception to the most general rule.

There is another difficulty, partly the effect of circumstances, partly of our own creating, which prevents the attainment of more certain knowledge on various other obscure points of medical science.

It need hardly be said that the practice of medicine is in a very artificial and complicated condition in this country; and that from the quantity of drugs taken on all occasions, the powers of nature must be so constantly interfered with that our opportunities of understanding her real power are but few. The *quid pro quo* system, by which the general practitioner has been in the habit of having his services remunerated in this commercial country, is the root of a wide-spread evil. It is a practice alike prejudicial to the progress of medical science, to the health of the patient, and to the reputation of our profession. If it were not so universal a custom, it would be difficult to believe that the English were, on the whole, best satisfied to pay for medical advice by the strange fiction of an extravagant sum charged for the medicines. We by no means wish to assert that on this account patients are purposely compelled to swallow what their medical attendants know will be injurious to them; on the contrary, we believe that mere placebos are constantly prescribed; and that where under other circumstances soda-water, imperial drink, or hot and cold water would be recommended, under the present system, effervescing draughts, saline mixtures, lotions in bottles, and ticketed fomentations are substituted. But still the tendency of our customs is to lead the patient to take and expect, and the medical practitioner to prescribe and to believe in, the efficacy of much more medicine, in many more cases, than is necessary, and to give to mere drugs an importance beyond their due. How many patients believe that little is doing for them unless they have a regular supply of physic, "twice or three times daily," or "every four hours;" and how many practitioners think that when they have pre-

scribed the dose they have done their whole duty? Doctors are too apt to forget that even in cases where "inactivity" in the administration of active remedies is injurious, activity for the patient's welfare may be shown in the perfect remembrance of all his symptoms, and in minute attention to air, rest, or exercise, diet, hours, and habits, and to those other more common circumstances which influence so strongly his comfort and his well-being. This branch of medicine, which the French call hygiène, and for which we have no other term than the Latin word "regimen," is certainly considered of much less importance in the cure of disease than it merits; and undoubtedly a more scrupulous and watchful attention to it, as a branch of medicine second in utility to no other, would be one mode in which the present faulty and artificial state of our medical practice would be improved. Both medical men and their patients would be then on the road to a more moderate estimation of the power of mere drugs in the treatment of diseases. If, indeed, this mode of practice were abandoned the means of our art would be most limited, and our power trifling. For although many diseases may require no active medicines, yet there are none which do not require "regimen," and the regulation of these common influences is as much within the province of medicine as the exhibition of the most powerful drugs. And perhaps more real success, and consequently reputation, attaches to the practice of those who minutely attend to this branch than to those who attach more exclusive importance to the more intricate and apparently deeply scientific parts of medicine. The aphorism of the moralist is here applicable:

"Verily, methinks
Wisdom is ofttimes nearer when we stoop
Than when we soar."

We once knew a provincial practitioner, a man by no means of scientific acquirements, but who was extremely successful in the cure of chronic diseases. In the first place, he minutely inquired into the habits of his patients, to ascertain the causes of their ill health; and in cases in which there was general derangement of the health, of some standing, he adopted one plan, which was this: He sent his patients into the country, ordered them to take exercise on horses, donkeys, or ponies, and on foot, without fatigue, to drink very freely of whey, and to clothe themselves completely with flannel. The medicine he gave (very little) was some mild vegetable tonic pill, to be washed down with chamomile or sage tea. And in this way he cured the nervous, hypochondriacal, obstinately dyspeptic and the debilitated, or improved those incapable of being cured. It may be worth while to add, as examples are more convincing than mere argument, that the most popular provincial physician of the present day is especially attentive to all that relates to the "regimen" of his patients. And to the same cause must be attributed very much of the success which mineral spas deservedly attain; for although the water of each differs, from the purest up to the most highly mineralized, yet at all places "regimen" is much insisted on, and much advantage does arise from the rational way in which the patients are compelled to live; for although the water might not be of much benefit, yet good habits must improve all. In this way also the homœopathists may be of good service, by teaching us how many diseases require no

medicine at all, but are (if curable,) to be improved by "regimen" solely.

The foregoing remarks have been suggested by the consideration of the plan (the main subject of this article,) proposing the cure of all diseases which are not in such a stage as to be absolutely incurable, without any medicine whatever, but by the employment of pure, cold water, applied externally and taken internally, together with a close attention to regimen—such as mountain air and constant exposure to it, much exercise, total abstinence from distilled or fermented liquors, and plain and coarse food. To those who would at once reject this as quackery, enquiring into which is beneath their dignity, we beg to submit the following quotation:—"Having paid some attention to the proceedings of empirics, foreign and indigenous, regular and irregular, we venture to say, that we may sometimes be taught by them useful lessons; and we ought not to decline assistance even from such sources." This was written by a physician of the highest reputation of his day; who to a scientific knowledge added great practical skill, gained by watchful, long, and very successful consultation-practice in a city famed for the celebrity of its medical men--by the late Dr. Cheyne, of Dublin.

Vincent Preissnitz, the founder of what is called the Hydropathic System, is one of those self-educated men who occasionally become notorious by their own unaided efforts in a path of their own selection; men who know not such a word as "impossible," and for whom things which are hinderances to others, are among the means of their success. Reserved, silent, a man of the fewest words, who promises nothing; irritable and impatient at opposition; with a strong will, unyielding firmness, and untiring perseverance in carrying out his plans; and withal thoughtful and watchful, capable of adapting his means to the individual circumstances, and fruitful in expedients under unexpected difficulties: such is the character which he shares with many of those whom the world willingly worships; or, rather, to whom small sections of it everywhere submit as their guides or rulers. The unbounded confidence which such men have in themselves seems to be willingly believed in by almost all who are directly subjected to their influence.

Preissnitz's father was a small farmer in the mountains of Silesia, who brought him up to the severe manual labours of an agricultural peasant. His inclination to his present very different condition seemed to have depended on an old man, who was accustomed to use the water-cure in diseases of cattle; and he taught the art he himself possessed to Preissnitz. He applied the same remedies to his sick neighbours, and was confirmed in his views (as doctors often are,) by the good he gained by treating with water dressings a severe injury which he himself experienced. The success of his own case was also the means of spreading his fame. He is now forty-three years old; his reputation has gradually increased, so that within the last dozen years between 7000 and 8000 persons have submitted to his treatment under his own eye.

Besides the numerical method there are, we know, two other processes of proving a medical man's success—by the money the favorite accumulates, and the "great" people who put their faith in him, and submit their "porcelain clay" to his care. By both of these tests Preissnitz may be tried, and his celebrity strictly proved. "At present, in 1841,"

says Mr. Claridge, "there are under his treatment an archduchess, ten princes and princesses, *at least* 100 counts and barons, military men of all grades, several medical men, professors, advocates, &c., in all about 500!" (p. 69.) And besides this high patronage, he has accumulated solid pudding to the amount of £50,000; not from the accumulation of guinea fees, or journeys at a guinea a mile, or occasional cheques of £1000 in nightcaps thrown at the surgeon's head, but from fees ranging from the minimum of four shillings a week to the maximum of double that small sum, and from the profit arising from his great boarding-house, where his patients are fed for eight shillings a week, and lodged for four shillings more. For, notwithstanding his celebrity among the celebrated —in spite of a list of patients which reminds one of the court circular on the day after a drawing-room, Preissnitz receives several hundred patients into his own house, which has been built for the purpose, and his wife, "who," says M. Gross, "is pretty and fair, very natural, but very clever," is perfectly acquainted with domestic economy, and alone manages the entire household. It speaks well for him that he retains much of the simplicity of his former station, both in habits and manners— rising very early, temperate, and a water-drinker, and still keeping up the custom of the poor in that part of Europe, of kissing the hands of their superiors (especially of all ladies,) on entering and leaving the room.

The information we have of his treatment is not from his own writings, as, from want of education, he is probably no writer; nor from his own mouth, as he is no talker or explainer of his plans and views. The number of his patients must prevent his talking much with each; and although his practical ability may be considerable, yet his ignorance of the science of medicine, and, probably an inability to explain clearly the rules which guide him, may make him pursue what is certainly his safest course—that of not explaining anything. He is said to have an aversion to medical men: this is not unlikely, as he must have met with opposition from them; and irregular workmen of all crafts look suspiciously on those into whose legal province they are intruding. What means we have of judging of Preissnitz is from the works of medical men and of others who have visited him as patients. The two English works at the head of this article are those which we shall make use of. One is written by Mr. Claridge, a non-professional gentleman, who was the first to introduce Preissnitz to the British public in a separate book. After suffering from rheumatism for many years, which was not relieved by the usual remedies, or the regular physicians to whom he submitted himself, he went to Graeffenberg, and was cured. Active-minded, acute, and observant, he has described the means of cure he witnessed, and has translated the opinions which many German physicians have published after visiting the same establishment. Like many intelligent people who are ignorant of the practice of medicine, especially if they have suffered from a disease which has baffled the ordinary remedies, Mr. Claridge sees clearly enough that the medical art is imperfect, and then hastily arrives at the conclusion that there ought to be a perfect system of cure, that there must be means to remove without fail the actual cause of the disease. Such persons, with their scanty stock of knowledge and their enormous self-conceit, are the easiest prey to quacks. The homœopath horrifies them by describing the consequences of bleed-

ing and purging, remedies, as he says, directed to effects only, whilst he adds, "this little globule goes at once to the cause, and destroys that without any injury to the constitution," and they believe him. The advertisements of our newspapers show us daily that both regular and irregular quacks—from doctors with diplomas who advertise their books, to the chemist who trades in his peculiar pill—adopt the same principle of delusion; strongly setting forth some obvious defect in the medical art, and announcing unscrupulously that the new remedy or new plan supplies infallibly the great deficiency. Mr. Claridge has evidently no doubt at all but that all diseases depend on some material poison, some materies morbi, and that this is to be eradicated, not by drugs, but by water, producing perspiration and eruptions, boils and abscesses, which contain the true cause of the disease itself. But although we do not seek in Mr. Claridge's book for either just or comprehensive views of the nature or treatment of diseases generally, or for a fair estimate of the advantages of the system he upholds, compared with other plans, yet, from his having submitted to the treatment, he is fully competent to describe the mechanical plans adopted, and the regimen to which he was subjected. He is an enthusiastic and undoubting believer, and seems sincerely desirous to communicate to others the knowledge of means which in his case have proved so serviceable. And on this account his book is likely to be more convincing to others than the second pamphlet, the work of a medical man, who (as we see by the daily advertisements,) uses it as the means of acquainting the English public that they may be cured in the same way at home. This book is written in too jocose and flippant a style to be of much weight. Quotations from Don Juan do not enhance the value of a medical book. The writer is also one of those practitioners who never lost a case of scarlet fever, and who reflect on the treatment when a fatal epidemic occurs—a sure proof that he has seen very little practice. On the whole, therefore, we give the preference to Mr. Claridge's work, as one written with more earnestness and singleness of purpose.

The channel by which Preissnitz cures, or attempts to cure disease is through the medium of the skin; he endeavours to produce perspiration or cutaneous eruptions, boils or abscesses. For this purpose he employs water as an external application and as a beverage, together with exposure to air, exercise, and strict abstinence from stimulating drinks. He does not indiscriminately attempt all cases. He refuses to treat patients with diseases in his opinion incurable, such as consumption, organic defects, or where the *vis vitæ* is manifestly exhausted; but we should gather from the books before us that he subjects all diseases which are curable by medicine, to his plan. But although using one plan, he modifies that very considerably, according to the constitution of the patient, his strength or weakness, and the seat of the disease. And in thus accommodating his means to his ends he is said to show much judgment. In theory he is exclusively a humoral pathologist, regarding all diseases as dependent upon a morbid state of the fluids, and that the cure depends on an evacuation of the diseased matter in a visible form, in sweat or pus, and that this evacuation is preceded by a febrile state of the system—the body struggling with the disease and throwing it out. This crisis he seeks to produce.

Graeffenberg, where Preissnitz lives, is a small hamlet, half way up one of the mountains (called Sudates) of Silesia, in Austria. He has accommodation in his own houses for about 200 patients, and the villagers can lodge 150 more. The fastidious would not be suited. The cottages seem equal to the lowest dwellings of our agricultural poor. "What a bad lodging!" exclaims M. Gross. "Our servants would not put up with it. My landlord conducted me by a narrow staircase, almost perpendicular, placed at the low and dirty entrance of the house, to a very small room, so low that I could not stand upright in it. I constantly ran the risk of knocking my head against the beams of the ceiling." Even those who are fortunate enough to get a room in Preissnitz's own establishment (and this is only done by coming very early in the season, or writing to hire one beforehand,) have to put up with much. The bed-room is equal to a soldier's barrack-room. A bedstead and a straw mattress, a deal table, two chairs, a chest of drawers, a large basin, a decanter and a glass are the sole contents. The whole place, too, is permeated with a smell, compounded of the cows which are kept in stables beneath the house, of cabinets d'aisance upon the staircase, and of the kitchen beneath the common room, which opens into it by a trap door, through which the cooked food, as well as the various odours find entrance. The less comfort there is within doors, (thinks Preissnitz,) the less time will my patients spend at home ; and the coarser the fare, the less danger will there be of their bodies being too highly nourished by it. Three miles from Graeffenberg is the town of Freiwalden, where those are accommodated who cannot find room nearer, and where there are better lodgings. Those who wish to put themselves under Preissnitz's care are recommended to write their case to him in French or in German, as he will not undertake those patients whom he considers to be incurable.

If the patient is of a sufficiently vigorous constitution, he is submitted to a sweating process—one of considerable originality. At four in the morning, the servant throws off the patient's bedclothes, and wraps his body closely in a blanket, covering this with a small feather or down bed, placing a counterpane over that, and tucking all tightly in. In a space of time varying from half an hour to two or three hours, he begins to sweat; the windows are opened, he is allowed to drink cold water, and the sweating is kept up from half an hour to two hours. Then the bed and counterpane are removed, the face and neck wetted with cold water, and the patient (still covered with the blanket,) puts on straw shoes and walks to a cold bath in an adjoining room. He again wets his face, neck, and chest with the cold water, and then, wet with perspiration, plunges into the cold bath, and remains there from two to eight minutes. When it agrees, reaction follows, and the skin becomes red. He dresses, walks, and then breakfasts. This is repeated daily, or every second or third day. Or instead of the whole-bath, a douche or a half-bath may be substituted. This is the occupation of the early morning: douche and local baths and wet compresses employ much of the day. For those who are too weak for this process the *wet sheet* is used. A sheet is well wrung out of cold water, it is then spread upon a blanket, and wrapped 'round the patient, and upon this is placed a feather bed, as in the former process; afterwards a cold or half-bath. Preissnitz

treats fever and febrile diseases, such as scarlatina, smallpox, and the like, with this wet sheet. It is also used as a portable bath. On the patient's getting out of bed, a wet sheet is thrown over him; he well rubs the front and the attendant the back of his body for several minutes; a dry sheet is then substituted, and the same friction repeated until he is dried. It is said to be a convenient bath when travelling, relieving fatigue, and rendering the skin less sensitive to cold. Very delicate persons are washed all over, water being poured over them whilst the attendant rubs the whole body and the affected part with the hand. Where the whole bath is too much for the patient's strength, and generally as a preparatory step to the sweating process, a *half-bath* is used: the water being from three to six inches deep, and not below 60°. Local half-baths are much employed. A *sitting-bath*, in which a person sits as in a hip-bath, in water a few inches deep; a *foot-bath*, with two or three inches of water; a *head-bath*, the patient lying with the back of his head in a basin. There are local baths for all parts—eye, elbow, leg, and finger baths. The *douche* is not merely used locally, but generally. A mountain spring is carried through pipes into rude huts, so as to fall from ten, twelve, eighteen, and twenty feet, in a stream the size of the wrist. The patient (perhaps after sweating,) first washes his face, head, and chest with his hands; then he allows the stream to fall slanting upon his shoulders, hips, and loins; then upon his head, guarding it with his hands held over it roofwise; and then upon the weakened or paralysed part, if such there is. He dresses in a similar hut close by; and generally much enjoys this part of the treatment. The time varies from three to fifteen minutes. In those benefited by it the skin becomes of a rosy red; it gives tone to the muscles, and increased activity. If it produces feverish symptoms, it is discontinued. *Wet compresses* are much employed, not merely to produce cold by evaporation, but as we use "water dressings," only instead of oiled silk, the wet compress is covered with a dry linen compress, and changed when dry. But besides employing these for all kinds of wounds, ulcers, &c., and nothing else, Preissnitz orders them for every sort of local disease, such as come within the province of the physician as well as the surgeon. Those whose throats or chests are affected wear them around their necks and chests at night; the gouty and rheumatic have their legs incased with wet clothes at night; and those affected with any abdominal disorder, such as indigestion, feeble digestion, constipation, diarrhœa, colic, and the like, wear a wet towel round their bowels during the day. A towel three yards long and one foot wide, two thirds of which are wetted, is thus applied: the wet part is wound round the belly, and the dry part covers it, strings being attached to the dry end.

But besides the external application of water, Preissnitz enforces a general regimen.

He directs his patients to drink as much cold, pure, spring water as they possibly can swallow without feeling a painful distension of the stomach. Before breakfast is the time at which most water can be drunk, but it is to be taken at meals and between meals also. Habit does much: in a short time from twenty to thirty glasses can be drunk daily. He allows neither beer, wine, nor spirits; no tea, nor coffee, nor any hot fluid. He allows his patients to eat at will of coarse foods; he does not

limit their quantity, nor is he careful as to quality, as his table is supplied with (what we consider) very indigestible food, such as pork, veal, ducks, geese, cucumbers, and all kinds of pastry. All mental occupation is forbidden, and much exercise, with constant exposure to the open air, required.

The breakfast consists of cold milk, brown bread and butter, and wild strawberries, which grow abundantly there from May to October; the evening meal at seven p. m. is the same. The dinner at one o'clock consists of soup with beef boiled in it, followed by pork, veal, or mutton alternately, fowls, ducks, geese, salads, cucumbers, and preserves, and all kinds of pastry. Vegetables—except cabbages and sour-krout—are very scarce, but are permitted. The patients at the table d'hôte—where upwards of 200 are congregated—are described as eating indiscriminately great quantities, amidst much noise and merriment; so that the new-comer finds it difficult to believe that so many healthy-looking, happy, mirthful people are very sick. Mr. Claridge's own case will exemplify the application of these means.

At four in the morning he was wrapped in a blanket, covered with a bed, and kept in a profuse perspiration for an hour, and then went into a cold bath for three minutes. He dressed, drank cold water abundantly, and walked until breakfast. At ten a douche-bath at some distance; and between this and dinner, at one o'clock, he took a sitting-bath and a foot-bath, remaining in each a quarter of an hour. Another douche at four in the afternoon, a sitting and a foot-bath again at seven, and to bed at half-past nine, sleeping with both legs wrapped up in wet clothes. During the intervals he walked vigorously as much as ten miles every day; and he says he was in more robust health than he had ever known. His disease was rheumatism of many years' standing. As he seems to have been subjected to the most rigorous application of water, such as a daily sweating and the douche twice every day, we conclude that he must have a robust and vigorous constitution; and the enthusiasm in which he talks of the effects of abstinence from stimulants and of cold ablution, indicates one to whom these very obvious means of health are novelties. This treatment was persevered in for three months.

The information we have as to the application of these means to the various diseases is by no means satisfactory. The pamphlet by Dr. Wilson is on this point extremely scanty; and Mr. Claridge's want of knowledge of medicine and enthusiastic zeal incapacitate him very much as a witness, although his attention to the subject makes him the best witness we have. The translations which he gives from the works of foreign physicians do not indicate that the writers have sufficiently studied for any length of time the practical application of the treatment. Gout and rheumatism were the diseases which Preissnitz first attempted to cure; but it seems that individuals with all kinds of chronic diseases, such as resort very commonly abroad to mineral springs and baths, put themselves under his care: and that when any of these whilst residing with him are attacked with acute inflammations, or with fevers, or eruptive diseases, that they are subjected to similar means of cure. Many of these chronic invalids are affected with diseases of their digestive organs and their sequelæ, and when brought about by full living, drinking much beer, &c., incessant smoking, with the sedentary habits, con-

finement to the house, deficiency of pure air, and uncleanliness of skin and linen which such sensuality supposes, the beneficial effects of so contrary a system are not surprising. Those who are of weakly or delicate constitutions, whatever may be the local disease, are evidently not subjected by Preissnitz to his heroic treatment. Frequent cold ablutions, the body being at the same time rubbed by the hand of an attendant, and *half*-baths, in which there is but a small quantity of water, which soon becomes of the temperature of the body, is substituted for the sweating process; and it seems that Preissnitz, as he has more experience, is more cautious in the application of his remedies. A man himself of the strong, much-enduring frame of a peasant, who began the practice of medicine upon horses, cows, and dogs, and passed from animals to his own poorer countrymen, living, like himself, the simple hardy life of mountaineers, must require some experience to teach him how wofully civilization and luxury and refinement impair the native vigour of the constitution, and render practices hazardous in one case which in the other were at least harmless. Scrofula is mentioned as successfully treated. The external application of water as a general tonic, as well as an application to enlarged glands, ulcers, &c., together with the mountain air, exercise, and milk diet, may be in many cases beneficial; but as strumous patients are usually of feeble constitution, they are not probably submitted to the most active treatment. Hysteria and hypochondriasis are subjected to the treatment according to the strength or weakness of the constitution. Patients are not placed in the bath during the attack of epilepsy, or whilst in a nervous convulsion. Diarrhœa and cholera are treated with sitting-baths for half an hour three or four times a day; cold water in abundance, as drink and as clysters, and a wet compress over the abdomen. After the bath the patient is put in bed to encourage perspiration.

In *congestions in the head* the sweating process is omitted: but sitting-baths with cold, wet bandages to the head, with abstinence from all stimulants and excitements, mental and physical.

In *habitually cold feet*, besides abundant exercise, cold foot-baths twice daily for fourteen to twenty minutes; and wet compresses at night. The same treatment for fetid perspiration from the feet.

Chilblains are covered with wet compresses.

In *headaches*—especially gouty headaches—and for deafness the head-bath is used. The patient lies on his back, with the back of his head in a basin of water for a quarter of an hour, then he turns on one side, then on the other, and then on his back—thus keeping his head in the water for an hour. We are not surprised to hear that this often produces abscess in the ear: Preissnitz reckons such a consequence critical and beneficial; but to us it seems a very hazardous and rash proceeding, and that the abscess might destroy the ear entirely.

Toothach is said to be relieved thus: either by a cold foot-bath, or by the patient's keeping tepid water in his mouth and constantly changing it as soon as it gets warm, whilst he puts his hands in a basin of cold water and rubs his face and the back of his ears violently with them, *until the pain ceases.*

For *habitual costiveness* two or three cold clysters are given daily; much cold water is drunk; a wet compress is worn over the belly; cold

food, and plenty of fruit. If it has lasted for years, the sitting and foot bath and the douche.

For a *common cold* perspiration in the wet sheet, or the half-bath is said to be curative. The same treatment for gripes.

For *sprains and stiff joints* cold local baths and wet compresses; for *fractures* wet bandages; for *sores*, *ulcers*, *wounds*, *bruises*, wet compresses.

In *fevers* and *eruptive fevers*, as *smallpox*, *measles*, *scarlatina*, &c. the patient is wrapped in a wet sheet, which is often renewed, and this is covered with bedclothing; when perspiration breaks out, a half-bath at 61°.

In *inflammation of the lungs and abdomen* the patient is placed in a sitting-bath at first, and the legs, which are out of the water, are well rubbed. Cold, wet, evaporating cloths are placed over the chest or abdomen, and frequently renewed, and the body is wrapped up. This is said to produce warmth, and then the wet sheet is used.

In *intermittents* the patient is placed in a *half-bath* and rubbed during the cold stage, and he is made to drink plentifully until he vomits. Wet compress over the belly.

Itch, *piles*, *diseases from syphilis and from mercury* are subjected to the full treatment.

Any estimation of the value of hydropathy as a means of curing disease must at present be imperfect, as the medical evidence (from causes previously mentioned,) is very scanty. There is little more indeed than the bare fact that several hundred persons annually submit themselves to this treatment for several weeks, or even months, together, without medicines of any kind. And it appears that the invalids are such as usually resort to mineral spas abroad, with chronic diseases of all sorts, all of which unless they have arrived at a stage manifestly incurable, are subjected to the same means, modified according to the constitution of the patient, or the locality of his complaint. It is but just to admit the improbability of as many as 1400 patients in one year, and for several years in succession, submitting to such a course of treatment unless it were in many instances beneficial. It may be said, that the same argument would prove the benefit of Morison's pills, the sale of which is said to have been for many years most considerable. The difference between the two plans of treatment, however, is very great as regards their effects on the body. Purging, and aperients relieve many diseases and disordered conditions of the body during their administration, and it is not discovered until some time afterwards that the constant use of such stimulating medicines has materially injured the constitution. But in hydropathy the injury, if any, must be felt at the time. If a patient has strength enough to undergo these hardening processes, we cannot imagine any subsequent mischief. It is then but fair to conclude that if the system had been a dangerous one, as the injurious effects would have been at once apparent, the success which has attended it would not have followed, and that Preissnitz has performed experiments of the effect of pure cold water upon the body on so large a scale, and for so long a period of time, as to merit our inquiry and attention.

Preissnitz's remedies are of three kinds:

1. The administration of the cold-bath when the body is in a profuse

sweat, not from exercise, but from warm clothing. The principle that as a rule there is no danger in applying cold to the surface when in a state of perspiration, unless the body is at the same time fatigued or exhausted, is known and acknowledged. The Russian vapour baths and the mode in which the Indian treats fever, prove medically what every one who has seen schoolboys bathing in hot weather, or Neapolitans drinking iced water must have felt to be true—that cold may be applied externally and internally with impunity, even if the skin is profusely perspiring from mere external heat; whilst sudden deaths in our harvest-fields and the injury even from local application of cold water after heat produced by fatiguing exercise are an opposite class of facts. Preissnitz is original, not in discovering this principle, but in its application. He produces perspiration by warmly covering the body, and then subjects it to cold. By this means it appears that great action of the skin is produced. The free sweating in bed, promoted by large quantities of cold drink, proves that his diaphoretic remedy is a vigorous one; and the effect of the cold bath afterwards is, according to those who have tried it, to make the skin red and warm; thus increasing, obviously, the whole capillary circulation. These means of curing diseases are therefore powerful ones. Practically we must all own that we can cure very few diseases without producing evacuations. The humoral pathologists of former days conceived that in this way the actual materies morbi—"the humour"—was got rid of. Since John Hunter's time we talk learnedly of a new action being set up to supersede the old and diseased one. But whatever may be the theory, it cannot be doubted that diaphoresis produced every morning for several weeks must have a considerable effect on the economy; and if it can be produced without debilitating the body, but by means which actually strengthen it, it is a judicious mode of attempting the cure of many chronic diseases. We have in this country a somewhat analagous process in training. When the greatest muscular strength is necessary, as in men preparing for boxing or pedestrianism and for race-horses, regular sweating is thought essential to the process; so that in practice it is found to be a necessary part of a tonic regimen. But in addition to ordinary perspiration and reaction subsequently from cold, the treatment, when persevered in, produces a general febrile disturbance of the whole system, sometimes trivial and sometimes severe, and often attended with skin-eruptions, boils, and abscesses. In this country we are extremely unwilling to admit the doctrine of crises: we take matters so entirely into our hands, and use the bodily machine as if it were a passive piece of mechanism that we have to clean and regulate, that we are almost sceptics as to the influence which Nature herself exerts in curative processes. But it is with due consideration that almost all physicians who watch the effects of mineral waters of all kinds upon the body are agreed as to one point—that the waters or baths act by producing a febrile disturbance in the system, followed by some visible excretion or new action. Bordeu, who studied for years the action of the *simple thermal* waters of the Pyrenees; Marcard, who observed the effects of the *Chalybeate* of Pyrmont; Heidler, who is still the resident physician at Marienbad, where the most active spring is a *saline aperient;* and Bertrand, who with personal attendance watches the action of the *hot alkaline* baths of Mont d'Or—

all men of practical sagacity, observation, and judgment—besides a host of others, agree in this point. Preissnitz, also lays much stress on this disturbance which his cold water applied so constantly is likely enough to produce; and he regards it as indicative of cure.

2. But besides sweating, followed by a cold bath, perspiration is produced by the application of a wet sheet to the whole body, and by wet compresses to various parts of the body. A similar plan was common in the northern parts of England a century and a half ago. Children at a very early age with rickets were taken to the wells of St. Bede, Honwick, or St. Mungo, "which are extream cold springs," and thus treated. "Some dip them," says Dr. Ellison, in a letter to Sir John Floyer, dated 1701, "twice and thrice over head and ears, with their shifts and nightcaps on, in the evening, for a fortnight or longer. Others dip them no further than the neck. Others, where the well is not capacious enough, content themselves to put the children in a tub of water gathered from the spring, and dash the water upon them over head and ears. All which immersions are to be despatched as quickly as may be, so that the child may not continue any longer in the water than is necessary; that is, till his body, and shirt, and night-cap be thoroughly wet. Others, out of tenderness to the child, or in regard to the child's weakness, content themselves with dipping only the shirt and nightcap in water, and put them on wet upon him. As soon as the children are dipped, they, *with their wet clothes on*, are wrapt up in warm blankets over their head and whole body, and put immediately to bed, which instantly puts them into a violent sweat. In this condition they lie all night, till towards morning the clothes are taken off by degrees, that so they may cool gradually; and in the morning they have dry shirts and head-clothes put on."* The writer adds that he never heard of any children who died of dipping, and of few or none but found great benefit. There are frequent indications in books of the same period of "water dressing" being applied by the poorer people who frequented springs in the same wholesale way. "There is," says Sir John Floyer, "a dangerous practice at Willowbridge, (a cold spring,) of which I have heard some patients complain: they wear the wet shirts in which they bathed all day afterwards, by which some were over-chilled; but I have heard others, that were more strong, who bore that practice without any injury, as they informed me." Saunders, in his work on Mineral Waters, mentions the practice of the country-people at some of the springs of very pure or tepid water was, to keep clothes, constantly wet, to the diseased or weakened part. The substitution of water-dressings for poultices, introduced by Dr. Macartney, need hardly be alluded to as a great improvement in the treatment of wounds and injuries. Preissnitz has carried it still further, using water-dressings for all local diseases which come under the province of the physician, as well as to all external ulcers, wounds, or bruises. He does not use oiled silk, but covers a wet compress with a dry one. The half-baths are a peculiar part of his system: sitting in a tub, the water of which is but six inches deep, or using a foot-bath, with the water only just covering the toes, for a quarter or half an hour, or even more, at a time, and repeating this more than once during the day, are processes to which he subjects his patients in order to produce a determination of blood to the part immersed. The shallow water soon becomes

* Floyer's History of Cold Bathing. Letter Fourth, p. 129.

of the temperature of the part which is immersed, and the patient does not seem to be chilled by it. The application of the douche to every part of the body in succession is an improvement on its mere local application, and it must act as a powerful tonic to those who are strong enough to bear it. The application of wet sheets, with external warmth in cases of fever, seems to be worthy of a trial in such cases as are likely to be benefited by perspiration, where the skin is hot and dry and the circulation vigorous.

3. *The frequent application of cold water.* The effect of cold water in hardening the skin and rendering it less sensitive and less liable to be affected by changes of temperature, is well known. By cold bathing alone, if we consult enthusiastic admirers of it, we shall learn that Preissnitz has been anticipated in the cure of gout, rheumatism, all kinds of neuralgias, hysteria and hypochondriasis, fevers, agues, asthma, and a variety of chronic diseases.*

Hydropathy would teach us one useful lesson if it would direct our attention more closely to the beneficial effects of this common remedy. There is no tonic more efficacious, and no single means more likely to decrease that sensibility of the skin to external influences, from which so many invalids suffer from constant colds and their sequelæ, than the outward application of cold water; and where a cold bath cannot be provided, or a shower-bath, a sponge and a large tub is an excellent substitute with which the body should be rapidly wetted on getting out of bed every morning. Foreigners reproach and satirise us for copying, to our own great inconvenience, the habits of the highest classes. There is one of their habits which cannot descend too low—extreme cleanliness from the plentiful use of water. Now that all external distinctions of rank, from clothing or decorations, have gone into disuse, there seems but one external mark left, that of cleanliness; the degrees of which, to a minute observer, are criterions of rank approaching the infallible.

With regard to that very important part of Preissnitz's treatment, his regimen,—it is on the whole so good that many of his cures must be alone attributable to it. Bathing ensures extreme cleanliness of skin; early hours, abstinence from all stimulants, both physical and moral, full exercise in a clear mountain air, a plain, simple, and coarse diet, such as exercise and air alone would enable invalids to enjoy or to digest with the appetites "of hungry workmen,"—is a mode of living which is very rational. All studious exercise of the mind is forbidden, and indeed there must be so much dressing and undressing, so much dipping and dashing and paddling and splashing, and rubbing and scrubbing—as to furnish full employment. The unlimited quantity of food permitted is objected to by both writers; perhaps Preissnitz has discovered the uselessness of limiting the *quantity* of food where air and exercise produce the appetite of a "workman," and is content in setting before his patients coarse and plentiful viands. The abundance of pure cold water which is taken must supply copiously the fluid for perspiration and thus contribute to the cure. All those patients who have been previously accustomed to wine, spirits, or beer, which, as a general rule, must be hurtful in all diseased conditions of body, would, of course, feel a marked improvement

* See History of Cold Bathing, by Sir John Floyer, Knight, and Dr. Baynard, Ryan on Asthma, &c.

merely from the substitution of a milder liquid; whilst those who are disposed to eat more than is useful, must find less room when parts of their stomachs are occupied with this less substantial article of their host's dietary.

This question may perchance be put to us,—Would you recommend me to try Hydropathy? And in such cases the inquirer has usually almost made up his mind to try it, and asks for confirmation. The answer would be, that Hydropathy is no amusement; by no means similar to a trip to one of the German Brunnens; but that much inconvenience must in every way be submitted to. If, however, it is a case of confirmed chronic disease, not incapacitating the patient from the long journey, not curable by ordinary methods, and not glaringly incurable, much seems to depend on the constitutional strength of the individual. If there is still vigour of constitution there would be less doubt in approving of the journey. If the disease was the effect of intemperate habits, and a journey to Graeffenberg alone was likely to cure these, by all means let your patient go. If he will not take sufficient exercise, if he is scanty in external ablutions, and if his habits are not to be broken through at home, and on these his indisposition depends, even a journey to Silesia will not be too laborious a means of benefit. But if hydropathy is to be tried, we should recommend the invalid who is bent upon it to go to Preissnitz himself. Setting aside the advantages of his local habitation and his name, we would trust more to his strong will and bold and energetic ignorance than to the calculated refinements of his would-be scientific followers. We have faith also in that "art unteachable, untaught"—that *medical tact*, which seems native to the man, and which, had his station in life and his education been different, might have made him, instead of the inventor of the empirical water-cure, the all-admired Hippocrates or Sydenham of these latter days.

Art. X.

The Pharmacopœia of the United States of America. By Authority of the National Medical Convention, held at Washington, A.D. 1840.—*Philadelphia*, 1842. 8vo, pp. 279.

In the year 1818 the New York Medical Society, at the suggestion of Dr. Lyman Spalding, made a proposition to form a National Pharmacopœia, (the want of which in the United States had long been felt as a serious evil,) under the authority and by the conjoint labours of the profession throughout the Union. The proposal seems to have been received with almost universal approbation. Accordingly, delegates from the different medical bodies assembled in each of the four districts into which the United States were divided, and by them draughts of a Pharmacopœia were prepared, and subsequently submitted to a General Convention which met at Washington on the 1st of January, 1820. The different draughts underwent deliberate examination, and were incorporated into one work, which was adopted with the title of *The Pharmacopœia of the United States of America*, the publication of which was intrusted to a Committee appointed for that purpose. The labours of the Convention did not cease here. It made a provision for the decennial revision of the work; and resolved that the General Con-

vention should be held at Washington in 1830. In accordance with this resolution it assembled on the 4th of January in that year, and after adopting measures for the publication of a revised edition of the Pharmacopœia, which appeared in 1831, it was determined that the third meeting of the Convention should take place in 1840. It accordingly met at Washington on the 1st of January in that year, and appointed a Committee of seven physicians (including Drs. Wood, Bache, and Dunglison,) who, we believe, were the acting members for the revision and publication of the Pharmacopœia. It was not until near the end of the year 1841 that this Committee had completed the review of the work, which had then to be arranged and prepared for the press.

The first two editions of the United States Pharmacopœia were printed in duplicate, namely, in both Latin and English. The publication of a national pharmacopœia in the vernacular language was at that time a novelty; but the Convention felt so strongly the propriety and value of such a proceeding that it boldly, and, as we think, very wisely, resolved to break through the long-established custom. But several reasons, one of which was that other languages than the English were in extensive use in some parts of the United States, led the Convention to publish their Pharmacopœia in both the Latin and English languages conjointly, the Latin being used on the left-hand page, and the corresponding English on the right. The reasons assigned for the duplication of the work do not appear to have had much weight with the Convention at its third meeting, for we find that it resolved to print this revised edition exclusively in English. In the preface to this work it is observed that—

"There seems to be no practical advantage to counterbalance the inconvenience of attempting to present ideas in a language which has no appropriate words to express them, and the labour and expense incurred in printing twice as much matter as is necessary to convey the meaning intended. The recent publication, moreover, of the French Codex and the Edinburgh Pharmacopœia in the vernacular languages of the two countries respectively, gives the sanction of high authority to the course now pursued." (Pref. xvi.)

The general plan and execution of the United States Pharmacopœia are very similar to those of the British ones. But the subdivision of the Materia Medica into a primary and secondary list is peculiar to the former. We cannot say that we are convinced of the advantage or propriety of this proceeding, to which several objections may be urged.

Willdenow's edition of Linnæus's *Catalogus Specierum Plantarum*, and De Candolle's *Prodromus Systematis Naturalis* are the works usually relied on as authorities for the names of plants, while Cuvier's *Règne Animale* is taken as authority for the names of animals. On several occasions, however, other authorities have been cited; though not so frequently, we think, as ought to have been done. Thus the retention of the old Linnæan names of *Laurus Camphora* and *Laurus Sassafras* appears to us peculiarly objectionable, since for the cinnamon plant, which belongs to the same family, Nees's authority has been followed, and his nomenclature adopted. For the sake of consistency and uniformity, therefore, we think the names applied by this distinguished botanist to the other officinal species of *Lauraceæ* ought to have been adopted. We observe that *cinnamon* is declared to be "the bark of Cinnamomum Zeylanicum

(*Nees*) and of Cinnamomum aromaticum (*Nees*)." But though *cinnamon* properly so called is the bark of Nees's Cinnamomum Zeylanicum, his Cinnamomum aromaticum yields *cassia lignea.* So that by this error the United States Pharmacopœia authorises the substitutions of cassia lignea for cinnamon, and likewise the oil of cassia for the genuine oil of cinnamon. In a medicinal point of view this is a matter of little or no importance; but the commercial value of the substances being very different, the Pharmacopœia ought not to have authorised the substitution. Moreover, the proceeding just referred to must lead to great confusion.

We highly approve of the plan adopted in the Prussian, London, and Edinburgh Pharmacopœias, and followed in the present work, of annexing to some of the articles of the Materia Medica and certain preparations, "brief notes indicating the readiest means of ascertaining their genuineness and purity." These notes are, for the most part, very judiciously drawn up; though, in some few instances, they are unnecessarily long, as in the case of *Sulphate of Soda.* We select the note to *Citric Acid* as a fair example of this part of the work, conjoining, for the sake of comparison, the notes respectively given to the same substance by the London and Edinburgh Pharmacopœias:

United States Pharmacopœia.	*London Pharmacopœia.*	*Edinburgh Pharmacopœia.*
In colourless crystals, wholly dissipated by a red heat, and freely soluble in water. The solution affords with acetate of lead a precipitate soluble in nitric acid, and yields no precipitate when added in excess to a solution of carbonate of potassa.	This acid is soluble in water; what is precipitated from the solution by acetate of lead is dissolved by nitric acid. No salt of potash except the tartrate is precipitated by solution of nitric acid. It is totally dissipated in the fire.	A solution in four parts of water is not precipitated by carbonate of potash: when incinerated with the aid of oxide of mercury no ash is left, or a mere trace.

Of these three notes we prefer for its conciseness and precision that of the Edinburgh College.—We have been greatly surprised to find no notes attached to *Elaterium*, *Scammony*, *Opium*, and many other important articles of the Materia Medica. Surely some means should be pointed out of recognising the gross adulterations practised on elaterium and scammony, and of ascertaining the value of opium.

It is stated that *Acetate of Lead* "is dissolved by distilled water, with a slight turbidness, which is removed by the addition of distilled vinegar." This statement, however, does not apply to *pure* acetate of lead, which yields a perfectly transparent aqueous solution, but to the common sugar of lead of the shops.

Under the head of *Vinegar* we are told that "one fluid ounce is saturated by about thirty-five grains of crystallized bicarbonate of potassa. It affords no precipitate with solution of chloride of barium, and is not coloured by sulphohydric acid." According to this statement one fluid ounce of American vinegar (which is prepared from cider), contains 17·6732 grs. of anhydrous acetic acid; whereas the same amount of the best British malt vinegar, called *proof* or *No.* 24 *vinegar*, contains 20·548 grs. of acid. Moreover, British malt vinegar and French wine vinegar invariably contain a certain quantity of sulphuric acid; indeed, British manufactures are permitted to add $\frac{1}{1000}$ part of sulphuric acid to their vinegar. But if it be true that American vinegar "affords no pre-

cipitate with solution of chloride of barium," it is evident that it must be quite free from every trace of sulphuric acid.

In this edition an improvement has been made in the names applied to certain parts of plants used in medicine. Thus on various occasions the terms *rhizoma* and *cormus* (applied to certain underground stems) have been substituted for *radix* and *bulbus*; while in several instances the word *seed* has been correctly altered to *fruit*. Thus the "rhizoma of Acorus Calamus," the "cormus of Colchicum autumnale," and the "fruit of Pimpinella Anisum," are examples in point. In some few instances, however, this principle of giving the right name to parts has not been strictly adhered to, as in the case of the fruit of conium maculatum, which is incorrectly denominated the "seeds."

Many of the processes of the last edition of the American Pharmacopœia have been left with little or no alteration. On several occasions those of the last editions of the London and Edinburgh Pharmacopœias have been adopted, and we are told in the preface that whenever the Committee of revision

"Could wholly approve the processes of the London or Edinburgh College [they] have adopted them in preference to others, from a disposition to promote uniformity in the preparation of medicines, so far as practicable, throughout those countries in which the English language is used."

The committee have followed the Edinburgh college in permitting most tinctures to be prepared either by the old method of digestion, or by the modern and much more expeditious process of percolation or displacement, as introduced by the French chemists. General directions for the performance of the process, which however requires no slight skill and experience to become successful, are given in both the Edinburgh and United States Pharmacopœias.

It would be tedious, we apprehend, to enter into a detailed account of the different preparations contained in the American Pharmacopœia. A few, however, deserve notice, in consequence of not being contained in the British pharmacopœias.

"*Vinum Ergotæ. Wine of Ergot.* Take of ergot, bruised, two ounces; wine a pint (wine measure). Macerate for fourteen days, with occasional agitation; then express, and filter through paper." (p. 241.)

It is obvious that every fluid ounce of this preparation contains one drachm of ergot.

"*Acidum Tannicum. Tannic Acid.* (*Tannin.*) Take of galls, in powder, sulphuric ether, each a sufficient quantity. Put into a glass adapter, loosely closed at its lower cord with carded cotton, sufficient powdered galls to fill about one-half of it, and press the powder slightly. Then fit the adapter accurately to the mouth of a receiving vessel, fill it with the sulphuric ether, and close the upper orifice so as to prevent the escape of the ether by evaporation. The liquid which passes separates into two unequal portions, of which the lower is much smaller in quantity and much denser than the upper. When the ether ceases to pass, pour fresh portions upon the galls, until the lower stratum of liquid in the receiver no longer increases. Then separate this from the upper, put it into a capsule and evaporate with a moderate heat to dryness. Lastly, rub what remains into powder.

"The upper portion of liquid will yield by distillation a quantity of ether, which, when washed with water, may be employed in a subsequent operation." (p. 62.)

Tannic acid occasionally proves an exceedingly useful astringent in positive hemorrhages and in profuse discharges from the mucous membranes. It may also be used as an antidote in poisoning by the vegetable alkalies. The dose of it is three or more grains.

"*Unguentum Mezerei. Ointment of Mezereon.* Take of mezereon, sliced transversely, four ounces; lard fourteen ounces; white wax two ounces. Moisten the mezereon with a little alcohol and beat it in an iron mortar until reduced to a fibrous mass; then digest it with the lard, in a salt-water bath for twelve hours, strain with strong expression, and allow the strained liquid to cool slowly, so that any undissolved matters may subside. From these separate the medicated lard, melt it with the wax at a moderate heat, and stir them constantly until they are cold." (p. 233.)

This is the *Pommade épispastique au Garou* of the French Codex. It is an irritating application, which may be used as an epispastic or as a substitute for savine ointment in maintaining a discharge from blistered surfaces.

In concluding our brief notice of the Pharmacopœia of the United States, we beg to express our opinion that it is highly creditable to the knowledge, industry, and talents of the gentlemen composing the committee of revision, and to whom the getting up of the work was intrusted. It is obvious that great care has been bestowed on its preparation, as well as on its passage through the press. The alterations have been very judiciously effected, and all such new medicines and preparations introduced as the progress of discovery in the healing art has rendered necessary. As we have in some few instances expressed our dissent from the statements contained in this work, we would remind our readers that experience has fully shown that every modern pharmacopœia, British and Continental as well as American, presents numerous topics adapted for the cavillings of critics; and we verily believe that the present work contains fewer of these than many others of its kind produced much nearer home.

Art. XI.

1. *Der Leberthran als Heilmittel, auf Grundlage vielfacher Thatsachen und Versuche an Thieren, vom physiologisch-pathologischen Standpunkte dargestellt.* Von Dr. Hermann Klencke.—*Leipzig*, 1842. 8vo, pp. 127.

Cod Liver Oil, as a curative means, physiologico-pathologically illustrated by a variety of facts and experiments on animals. By Dr. Hermann Klencke.—*Leipzig*, 1842.

2. *Beobachtungen über den Leberthran, besonders in seiner Anwendung gegen scrophulöse Krankheitsformen.* Von Eduard Adolph Panck, Dr. Medicinæ russisch-Kaiserlichem Hofrathe, &c. in Moskau.—*Hamburg*, 1842. 8vo, pp. 41.

Observations on Cod Liver Oil, particularly in its action on Scrofulous Diseases. By Dr. Edward Panck, of Moscow. [From Vol. XX. Part III. (July, 1842) of the Hamburg "Zeitschrift für die gesammte Medicin.]

In a former Number (XXV) of this Journal we reviewed a treatise, by Dr. Bennett, on the therapeutical uses of the cod-liver oil. We are in-

duced to revert to this subject, from having before us the two works, the titles of which head this article.

In the course of Dr. Klencke's work very important questions on the theory of digestion, such as the manner and nature of chymification, chylification, the uses of the bile and of the pancreatic juice, are introduced, and handled with more or less amplitude. In fact, although an inquiry into the therapeutical merits of cod-liver oil is professedly the main subject, yet actually it proves to be rather an incidental and secondary one, in Dr. Klencke's treatise; the physiological disquisitions on digestion being the principal, both as regards the attention and the space allotted to them. As, however, the object of the present article is to place before the reader simply any additional evidence, which Dr. Klencke's work may contain, of the value of the oil as a remedial agent, rather than to embark in a speculative discussion on digestion, and collateral questions, we shall be compelled, in a great measure, to pass over, for the present at least, those portions of the volume which are devoted to these subjects.

Now, on referring to our former notice, and on again looking through the volume of Dr. Bennett, we find that both that gentleman and we have already pretty well fulfilled our respective tasks. In other words, we observe, that while Dr. Bennett's treatise embodies, to a considerable extent, the principal facts already ascertained in regard to the therapeutical uses of the cod-liver oil, we have on our part given a very fair summary of, and passed what we conceive to be a very unexceptionable judgment on the contents of Dr. Bennett's volume. Still, as Dr. Klencke's work forms a completer and abler exposition than that of Dr. Bennett—moreover, as it presents evidence more circumstantial and decisive of the success of the cod-liver oil treatment, we deem it worth while, as we have already said, to devote a short space to a further consideration of the alleged—we may now say established—antistrumous and other virtues of the oleum jecoris aselli. It is almost unnecessary to observe, that we shall refrain from repeating here any of the facts or information contained in our former article.

Dr. Ascherson conceives that oil-globules are essential to the formation of the elementary cells of animal tissue, and that these globules are constituted from fluid fat and albumen. He observed that on fat, in a fluid state and albumen being brought into contact, a small quantity of the latter instantly formed a case or pellicle round a globule of the former, and that from the globules thus produced, as we have already stated, the elementary cells of all tissues are formed. Now, according to Dr. Ascherson, while on the one hand normal bile furnishes a due supply of oil, chyme supplies a due proportion of albumen, and thus, by the union of these in the manner already described, the formative rudiments of animal tissue are produced.

Dr. Klencke, while admitting what Dr. Ascherson alleges as to the mutual effects of fluid fat and albumen, denies (p. 26) that the phenomenon is to be regarded as having any true resemblance to organic cellular formation, since the organic cell is first developed as a nucleus, and the periphery is formed posteriorly to the centre. He also observes that the oil and albumen of the globules could not enter the lacteals

without being previously dissociated, if not decomposed; and he further notices that chyme, *prior* to its admixture with bile, contains fat. Throughout the whole of his work, Dr. Klencke assumes that fat used as food and reduced to albumen, may be reconverted to fat, in the mesenteric glands, in the blood, in the cells of the parenchyma of the liver, and in the gall-bladder; and that this reconversion is favoured by a state of quiescence, as, for example, that produced by sedentary habits.

The author seems to think that cod-liver oil owes its power of improving chylification to its resemblance to bile, to which he conceives the oil to be a succedaneum. (p. 35.) Bile and the cod liver oil resemble each other in so far as both contain fat, resin, and similar saline constituents. (p. 20.) He does not think that the therapeutical action of the oil is due to its containing iodine, or bromine, or resin; but that in its oleaginous nature, and its general character as an animal oil, consist its medicinal efficacy (pp. 18, 19, 39). Elsewhere it is asserted that an aliment of the fat of pork or bacon produces all the beneficial consequences of the cod-liver oil. Its physiological properties are more minutely defined as consisting in stimulating the lymphatic and capillary systems, and the functions of secretion and excretion, and in improving nutrition; the deterioration of which is one of the omniform "bad effects of scrophulosis." It also replenishes the blood with an energetic and rich plasma, and, amid the activity of the vital processes to which it gives rise, promotes the absorption of all scrofulous depositions. (p. 78.) According to Dr. Klencke, the use of the oil serves to develope in the chyle those non-nucleated corpuscles, in the blood those colourless globules, and in the lymph the molecules peculiar to it, which furnish the formative constituents of the tissues; and the rarity and plenty, respectively, of which corpuscles in the three fluids now named are a sign of normal or of cachectic assimilation. (p. 64.) In short, the principal therapeutical excellence of the oil seems to consist in its power of acting as a substitute for the bile, when that secretion is either vitiated in quality or deficient in quantity.

That cod-liver oil possesses the property now ascribed to it Dr. Klencke infers from a variety of experiments on animals, principally cats and dogs. He found that after ligature of the ductus communis, the chyle, in cases in which no cod-liver oil was given, wanted the non-nucleated corpuscles, and showed, on being allowed to rest, no fatty stratum; but exhibited both of these when the oil had been administered prior to the death and dissection of the animal. (pp. 64-70.)

There is a disease to which cats are subject closely resembling scrofula in man. The animal is scabby, emaciated; its belly is tumid; it has a ravenous appetite; its eyes are tearful, the lids inflamed, and the nose exhibits sores. In this state even the endermic application of the oil appears to operate beneficially. A cat in the condition described was plunged, up to the neck, twice in the day, into "ordinary train-oil," and then kept wrapped in a flannel soaked in the same. The effect seemed to be equal to that of taking the oil internally.

The author also recommends inhalation of "an atmosphere impregnated with the oil gas" in cases of tuberculosis. The case of a lady labouring under "full-formed [ausgebildeter] tuberculosis" and hectic

fever, is recorded, in which a cure was effected in four months by this method of treatment; although the oil seems to have been employed in other forms besides that of inhalation. The author conceives that, used in the atmospheric form, the oil acts partly by endosmosis; partly by "its stimulating effects on the capillary system and peripheric nerves of the lungs." (p. 106.)

From the fact that the resin, which abounds in the darker-coloured sorts of the oil, appeared in the stools of one of the cats to which he administered it, Dr. Klencke infers that it is not to the resin that the efficacy of the oil is due; and he presumes from analogy that the resin of the bile, in like manner, is purely excrementitious, and exerts no positive influence in chylification.

In one respect the cod liver oil differs from bile, namely, in its reaction being acid, while that of bile is alkaline. (p. 25.) Yet we find that it is highly useful, and that too, probably, just on account of the reaction referred to, in cases of persons of biliary diathesis, the secretion of whose livers is very alkalescent and very inspissated. (p. 92.)

Towards the end of his work Dr. Klencke gives similar cautions and directions as to his cases proper for the exhibition of the oil, which Dr. Bennett has done in his treatise, and which we have quoted in our Twenty-fifth Number, page 201. Thither we refer the reader.

We are surprised by a statement which occurs at an early part of Dr. Klencke's work. He says that between the action of the three principal sorts of oil, there is as great a difference as between that of the various preparations of mercury. (p. 4.) We shall therefore briefly state the circumstances proper for the exhibition of these three sorts respectively; premising that, so far as regards colour, they are distinguishable into a tar-like black species, a fluid red, and a fluid brown and golden yellow species. (p. 3.)

In atrophic states, and states of cachectic assimilation, in which the indication is to improve chylification, the oleum subflavum, or the clearer and purer sort, is the suitable one. It is adapted for children and adults, with marked gastric irritability, but the action of whose bowels is languid. (p. 122.) But if, after cachectic symptoms, there manifests itself a tendency to morbid formations or depositions, and it seems desirable to correct the constitution of the fluids by a stimulant action on the nervous, vascular, and secretory systems, the dark oil, or that which abounds with resin, and even the fusco-empyreumatic species, is indicated. The author states that he has cured gouty and rheumatic cases more speedily by these last sorts than by the subflavous variety. These diseases, in adults, require the red oil, which species is also peculiarly adapted for cases of nervous irritation accompanied by dyscrasia, as well as "inflammatory affections which seem to result from a vitiated state of the plasma." (p. 123.)

Dr. Klencke and his "colleagues" appear to have treated successfully, by cod-liver oil, five cases of tuberculosis; two cases of acute hydrocephalus in children; three rachitic cases; two cases of caries scrophulosa; one case of chronic gout, with a cachectic constitution; four cases of spinal irritation, accompanied now with paralysis, now with pain in the backbone and pericardium; one case of epilepsy; nineteen cases of chronic skin-

disease; one case of the "purulent diathesis, without marked gastritic symptoms;" eight cases of inflammatory affection of the glands in all stages and varieties; two cases of chlorosis; one case of fluor albus; one case of gastro-malacia. (p. 107.)

The author directs, as the smallest dose to begin with, three table-spoonfuls of the oil daily; which quantity may be increased daily to twenty, or until the patient comes to live almost entirely on the oil.

The second essay which heads this article forms a long original contribution of 40 pages, in the "Hamburg Zeitschrift für die gesammte Medicin." We notice it on account of the strong testimony which it yields in favour of Oleum Jecoris Aselli, as a remedial means in a variety of diseases, but chiefly in those of a strumous character. Although, however, a useful practical dissertation, it does not appear to contain anything very new, or calling for particular notice.

"I have been," says Dr. Panck, "eight years, physician to the Alexandrian Orphan house, in Moscow, which contains three hundred children, of both sexes, and various ages. Here I have had much to do with scrofulous affections, and I must confess that, in the first years of my practice, when I did not employ the oil, the result of my treatment was much less favorable than of later years, when I did employ it." (p. 2.)

After detailing, with considerable minuteness, fifteen cases of various kinds, some cured, others not, he concludes with the following inference:

"From these cases we see how beneficial the oil is, in various forms of the scrofulous dyscrasy. Obstructions and indurations in the lymphatic system, congestions and infarctus (anschoppungen) in the glandular organs of the abdomen, of the liver and spleen, affections of the mucous membranes, swellings of the salivary glands, serous effusions, diseases of the fibrous tunics and periosteum, &c.; all these frequently find, in the cod-liver oil, when fairly indicated, and properly administered, an indispensable therapeutic agent." (p. 40.)

In conclusion, we beg to state that we still retain the belief which we formerly expressed, that the cod-liver oil will not, in practice, realize all the expectations which Dr. Bennett's treatise was calculated to excite in its favour, and we would make the same remark of the treatises now under review. For example, we are firmly persuaded that by far the majority of cases of chronic gout and rheumatism, particularly in adults, will *not* own the Ol. Jecoris Aselli as a radical curative means. We also believe that, in a majority of cases, it will fail to eradicate the scrofulous diathesis, or to affect any lasting amendment in cases of tuberculous lungs, whether the tubercles be solid or soft. And we fear that the case of phthisis, referred to by Dr. Bennett, (Treatise, p. 133,) "in which a vomica, with perfect pectoriloquy, was detected under the right clavicle, and which, "by means of the oil," had "a successful result," is destined to have but very "few and far between" parallels indeed. Still, the strong and recurring evidence of the beneficial effects of the Oleum Jecoris Aselli (and we can add our own on a small scale) forbids us to doubt that it is an agent of considerable efficacy in some cases; and we trust that our present notice of it will induce members of the profession to embrace suitable opportunities of testing its merits, and of reporting the results of their observations.

ART. XII.

1. *Hygiène philosophique, ou de la Santé dans le Régime physique, moral, et politique de la Civilisation moderne.* Par J. J. VIREY, M.D. &c.—*Paris*, 1828. Tomes ii. 8vo, pp. 282, 312.

The Philosophy of Hygiène; or on Health in its relation to the physical, moral, and political Régime of modern Civilization. By J. J. VIREY, M.D., &c.

2. *Essai d'Hygiène générale.* Par L. C. A. MOTARD, Docteur en Médecine de la Faculté de Paris.—*Paris*, 1841. Tomes ii. 8vo, pp. 496, 592.

Essay on general Hygiène. By Dr. L. C. A. MOTARD.

3. *The Principles of Population, and their Connexion with Human Happiness.* By ARCHIBALD ALISON, F.R.S.E., Advocate. 1840. Two vols. 8vo, pp. 572, 544.

STATE-MEDICINE, Medical Police, and Hygiène are terms which have been used synonymously to express the art or science which has for its object the application of the principles of medicine to securing the well-being and amelioration of society. We know that not a few persons connect with the term hygiène the ideas of diet and regimen, gymnastics, early-to-bed and early-to-rise doctrines, and the dull every-day matter relating to the health of valetudinarians. People are apt to think, too, that medical police implies nothing more than the seizure of stinking fish or unsound meat; or at most a fear-spreading contrivance termed a Board of Health, and brought into action when cholera rages. Hygiène is a science so little cultivated in England that we believe no treatise on it has yet appeared in our language, although we have literally cart-loads of valuable materials for such a treatise in our parliamentary reports.

Matters are managed better abroad, or at least in France and Germany; there governments and the press are alike interested in the subject. Indeed it is so hacknied, that scarcely a year passes in which we are not favoured with more than one philosophical, sentimental monograph on this captivating subject. Dr. Virey's publication is of this kind. Dr. Virey is very *gloriosous*, as all true Frenchmen are, very sentimental, and very philosophical; but his book has no practical value. He is a *fine* writer; that is, he has good and even noble ideas, but he conceals them amidst so much and such ridiculous magniloquence that it is painful to search for them. We fall upon an amusing specimen of his style at the close of the third book. Dr. Virey attempts a comparison between love and glory and their hygiènic effects on mankind under the form of a mythological fiction. At a grand levee Jupiter resolves to do a great action, and make man the first of living beings, as it is declared he shall be in "the book of destinies." To effect this he creates and sends to earth two genii, Eucle and Eros; the former taught by Minerva, the latter brought up in the bosom of Venus. When they get to earth, Eros gives "ardeur physique" to brutes as well as man; but Eucle, more dutiful, confines her gift—namely, the love of glory, to mankind. And so Dr. Virey defines man thus, *Homo, animal gloriæ.* Did Dr. Virey ever witness a cock-fight?

We have referred to Dr. Virey's work partly for the purpose of placing Dr. Motard's in a favorable point of view, and partly to exhibit the great

progress the science of hygiène has made since 1828. The essay of the last-mentioned author commences by stating that the object of hygiène is, to satisfy the physical and moral wants of man in such a manner as may best advance his individual and social development; or, in other words, the health, ease, and welfare of the human race. The most natural method of considering the subject of his essay, Dr. Motard thinks would be, to determine the influence exercised upon mankind by the various governments, laws, and creeds, which have successively appeared in the world; by climates, and by the manners and customs of various nations, as regards diet, clothing, labour, &c.; as well as by all the physical circumstances which modify our organization so considerably. He thinks, however, that the present imperfect state of our knowledge, especially of ancient nations will not permit of this plan being adopted. In this opinion we scarcely agree with Dr. Motard, and we think when he peruses the work of Mr. Alison, which we shall presently notice, he will find that much of the inquiry he proposes has already been made, and in a manner equally philosophical and practical.

The plan adopted by Dr. Motard is the following. Referring to his definition he arranges the physical necessities of man under five heads. 1. The necessity of a habitation and a locality. 2. Of having food and drink. 3. Of taking care of the person. 4. Of labour. 5. Of preventing the attacks of epidemic diseases. He devotes no portion of his work to a consideration of the moral wants of man (and this we think is a defect); but in the various subdivisions he estimates the influence of each physical agent on the intellect and morals. His plan, as it is, is of a most comprehensive character, and embraces very numerous details. Into these we cannot at present enter, but as an example of his method we will cursorily notice the arrangement of his first book, in which he treats of "The necessity for a locality and an abode." The book is subdivided into three chapters, the first of which is devoted to a consideration of the atmosphere and soil, or of the earth and air; and the chapter contains three sections. In the first section is a general view of climates, comprising, *a*, their meteorology, *b*, their statistics. Under the head of meteorology, Dr. Motard notices the various climates of the globe in relation to their latitudes and seasons; height above the level of the sea; relations to the sun's rays, and to forests, mountains, deserts, rivers, lakes, &c.; the prevailing winds, hygrometric condition and purity of the atmosphere; intensity of light; magnetism and electricity. In the second section he considers—*a*, the special influence of climates on man as they vary in the particulars previously considered; and *b*, the general modifications climates exercise, in respect to mortality, formation of habits and customs, and their influence on the intellect and morals. In the third section the hygiènic rules to be deduced from the preceding considerations are stated; and first the best method of being acclimated, on changing from one climate to another, is considered, and the special rules applicable to individuals as they vary in age, sex, temperament, and state of health, and as the seasons vary. The second chapter of the first book treats of water in relation to climate, and particularly of stagnant water; this chapter also contains three sections. In the first, the subject is considered generally; the second is devoted to the special influence of marshes and of their varieties, and the third contains the hygienic rules

proper to be observed, if we would obviate their bad effects, and render them useful. In the third chapter, Dr. Motard considers houses, cities, and villages, and follows strictly the triple subdivision into sections: first he notices them generally; then ascertains their influence on mankind under various modifying circumstances; and thirdly, states the hygiènic rules to be observed in reference to topography, hydrography, the nature and density of the population, &c.; and the best methods of constructing houses and public buildings. It may interest Mr. Chadwick to be informed that Dr. Motard allows from 866 to 1039 cubic feet to each patient in an hospital; and directs that the beds be at least a metre or about three feet three inches apart. The inspectors of prisons in England allow 1000 cubic feet to each prisoner.*

The plan of the succeeding four books is exactly the counterpart of that which we have detailed. The second book treats of food, condiments, and beverages; the third of articles of dress, baths, and gymnastics.

In the fourth the various occupations of mankind are reviewed as well as the subject of rest and sleep. Agriculture, war, commerce, navigation, and trades and professions as carried on in cities, are all noticed at length. The fifth and last book discusses epidemic and endemic diseases,—their nature and prophylaxis; and concludes, we do not see why, with a most meager chapter on the important subject of hereditary diseases. This and the preceding book are exceedingly interesting and deserve most careful perusal.

We think Dr. Motard has added materially to the value of his work by a statistical appendix, in addition to numerous statistical details scattered throughout the essay. This appendix contains thirty-three tables drawn up with special reference to the subjects discussed. In constructing these tables Dr. Motard, by making a very free use of English literature, has set a good and not unnecessary example to his countrymen. Tulloch, Marshall, Sir J. Clark, B. Hawkins, Macculloch, are some of the names quoted, somewhat disguised, it is true, according to French custom, but recognizable enough. Our periodicals are also laid under contribution; and Dr. Motard would no doubt have sunk a shaft into the mine of "blue books," if he had had an opportunity. Some of these tables are interesting: we do not remember to have seen the following:

"Table of the mean mortality in different degrees of latitude.

Within the tropics	1	death in	25	inhabitants.
From 20° to 40°	1	,,	34·5	,,
,, 40° to 60°	1	,,	43·2	,,
,, 60° to 70°	1	,,	50	,,

(Table vi. p. 565. Tom. ii.)

Nor this:

"Ratio of mortality per cent. during the year 1807 in the hospitals of Paris, classed according to the occupations of the sick.

Trade.	Mortality per cent.	Trade.	Mortality per cent.
House-porters, both sexes	33	Stonecutters	20·75
Ropemakers	25	Saddlers	20·5
Laundresses	23	Cooks	20
Labourers	21·5	Sick-nurses	20

* In the plans for workhouses sanctioned by the English Poor Law Commissioners, and published in their first Report to Parliament, only 108 cubic feet of space is allowed to each inmate.

Trade.	Mortality per cent.	Trade.	Mortality per cent.
Metal-founders	19·5	Tailors	12
Carders, of both sexes	19	Painters	12
Quarrymen	18	Thrashers	12
Bootmakers	16	Dressmakers	11·6
Valets de place, or waiters	16	Jewellers	11·6
Journeymen	15	Sawyers	11
Water-carriers	15	Locksmiths	10·5
Carters	15	Printers	9·5
Gardeners	14·5	Bakers	9·2
Joiners	14·5	Frameworkers	9·1
Butchers	14·5	Gilders on Wood	9
Prostitutes	13·9	Paviers	5·5
Female spinners (?)	13·	Soldiers	4·5
Carpenters	13		

(Table viii. D. p. 566. Tom. ii.)

When discussing the numerous details that come successively under his notice, Dr. Motard adopts a tone of moderation and impartiality in stating facts and drawing inferences; so that, although his arguments may not be always convincing, his method, at least, is quite satisfactory. As an example, we would refer to his examination of the disputed question, whether the emanation from animal matters in a state of putrefaction be injurious or not to the living. Contrary to popular belief and the unanimous opinion of writers on the subject, Parent Du Chatelet in France, and Dr. Warren in the United States, maintained that these emanations were not only innocuous, but actually beneficial in giving immunity from the attacks of epidemic diseases to those within the range of their influence. Dr. Motard briefly relates the facts advanced on both sides, and observes that positive facts are always more weighty than negative; that animal poisons do really produce dreadful results when introduced into the system by inoculation; and as absorption into the system may take place, analogy is in favour of the positive facts. He then discusses the chemistry of the question, and concludes with these inferences: A. There are three kinds of putrefaction: 1, mammificated air or water, having no access to the animal substance, this species is perfectly innocuous; 2, exposure to the air and decomposition into gaseous products and a carbonaceous deposit; odour fœtid; very slightly innoxious; 3, a mixed kind of putrefaction dependent on a limited supply of air, or excited by a fermentative principle itself the product of a limited supply of oxygen; odour variable, but usually disgusting; noxiousness very great. B. In all the facts quoted by Du Chatelet and Warren, the putrefaction had taken place in the open air; in all those on the opposite side, the contrary was the case. C. The varying circumstances under which the exposure to the emanations takes place necessarily lead to varying results. For example, the system may become habituated to a poison applied in moderate doses; or the season of the year, or hour of the day, the temperament of the individual or state of health and susceptibility of absorption will modify the results. D. The virulence of the emanations may be counteracted by a plentiful supply of fresh air, and a frequent renewal of it. We may add that a similar eclectic method is adopted in discussing the contagiousness or non-contagiousness of the plague, yellow fever, &c. and facts apparently conflicting are made to harmonize in a satisfactory manner, both parties in this fierce dispute being right and both wrong.

Dr. Motard draws a comparison between the vital statistics of the past and present century highly favorable to a system of public hygiène. The mortality in Paris has diminished just in proportion as the public health has been protected by hygiènic measures. In the year 1350 the mortality was so high as 1 in 17; in 1660 it was 1 in 25; in 1821, 1 in 32; in 1826, 1 in 36. In 1750 the mortality in London was 1 in 20; and from 1744 to 1800, the deaths out-numbered the births by 267,000; in 1821 the mortality had fallen to 1 in 40; and from 1801 to 1830, the births exceeded the deaths by 102,975. Epidemics are fatal in proportion as the population is crowded [and poor.] This was shown in the official report on the cholera in Paris. The number attacked in the city quarter was thirty times greater and the deaths twenty times more numerous than in the quarter of the Tuileries.

We shall now take up Dr. Motard's essay conjointly with Mr. Alison's, and notice from time to time the opinions and statements in which the two writers agree. It appears that Mr. Alison wrote the first draft of his work more than thirty years ago, when he had just terminated his philosophical studies, and when the doctrines of Malthus were making a great impression. From 1810 to 1819 was occupied in making observations and adding them to the work, which at last became so voluminous that Mr. Alison determined to rewrite it. It was not, however, until 1828 that this task was completed; but when completed, it appeared to the author that the time for successful publication was past; and laying aside the manuscript for his executors, he immediately commenced "The History of Europe during the French Revolution." The process of reaction has been developed sooner than Mr. Alison expected, and has induced him forthwith to launch his principles on the advancing wave of anti-malthusian legislation.

Mr. Malthus lays it down that population increases in a geometrical, food only in an arithmetical progression; that therefore in every country long inhabited, population must necessarily press on the means of subsistence; and that its progress beyond these means is limited only by the *positive* checks of vice and misery, or the *preventive* check of moral restraint. To these principles Mr. Alison opposes his own. He does not deny the power of moral restraints, but on the contrary asserts that they are not merely preventive, but positive checks on population; but he asserts also that they cannot coexist with vice and misery: while vice and misery, so far from being positive checks on population, will of themselves, under certain circumstances, encourage the excessive multiplication of the human race.

"It is a most remarkable fact, totally at variance with what might *à priori* be expected, but confirmed by the universal experience of mankind, that the dominion of reason over the passions, the habit of foresight, and the power of forming a systematic plan for the conduct of life, are just in proportion to *the degree in which the danger of immediate want, or the pressure of actual suffering have been* REMOVED *from mankind.*" (Vol. i. p. 243.)

"A certain degree of poverty among the labouring classes totally annihilates all limitations on the principle of increase, and renders early marriages as universal as the indigence which creates them. Men never look before them *unless they have some permanent object to look to;* they are always influenced exclusively by present enjoyments, unless they see some sufficient reason to forego them." (Vol. ii. p. 43.)

Referring to Dr. Motard's definition of hygiène, it will be seen that he and Mr. Alison have a common object in view, but approach it by the different routes of political economy and medicine, using the latter term in its widest sense, as comprising the science of human physiology and pathology. It is, then, as a contribution to the philosophy of hygiène, and as discussing some, and only some, of those moral wants of man, which Dr. Motard has not specifically considered, that we present "The Principles of Population" to the notice of our readers.

According to Mr. Alison the true moral checks upon population result from the agency of certain desires, which he terms artificial wants. These wants are developed by civilization, and become more powerful in their operation as civilization advances, and when, theoretically, there is the greatest danger of the population increasing beyond the means of subsistence. One of these wants is the want of animal comforts and luxuries; the desire to eat wheaten instead of rye-bread, to drink beer and tea instead of train oil or ditch-water, to wear fine clothes, &c. But "the desire to better one's condition" gives rise to the most powerful moral checks, *we* would add, also to the greatest moral evils. The mechanic feels pride in attaining to the dignity of a name-plate and brass-rapper on his door, his father being a decent man but a step below such grandeur. The wealthy tradesman builds his villa, and his daughters never rest till they get amongst "the carriage people," and not even then. The rich manufacturer aims at lands and a title; and the head of an ancient noble house aspires to be the head of the government, or at least of one of the parties in his country. We all want to rise; and as a decent coat is the first step from the mud of poverty, and indicates the birth of ambition and the first dawnings of rational enjoyment, foresight, and prudence, we may readily agree with Mr. Alison when he asserts that "tailors and milliners would do more in the end to improve the habits of the lower orders than all the efforts of the benevolent." Our readers will at once assent to the correctness of the following illustration:

"It is the continual pressure *from below* which occasions the excessive competition in every profession and business of life. Ask the physician, the lawyer, the tradesman, or the merchant, to what cause the difficulty they experience in making their way in the world is to be ascribed, and they will all answer that it is the influx of persons into their professions from an *inferior rank* which creates the competition." (Vol. i. p. 112.)

The application of this principle is obvious. A young man will take care how he embarrasses himself in his struggle upwards, with the dead weight of a wife and children. If he marry at all, it is not until the age of thirty of forty that he can do so safely, an age at which, according to Quetelet, the fecundity of marriage is diminished one half. The rate of increase is slowest with the most opulent; that is with those who have the most expensive artificial wants, and increases as we descend in the scale of society.

Mr. Alison next proceeds to analyse the circumstances by which in societies of mankind these principles are modified; and developes in an interesting and comprehensive manner the influence which climates, government, laws, religions, and customs have or have had upon human happiness. He passes in review over every country and through every age, including our own, with a special reference to the double effect

which the modifying circumstances have in increasing or diminishing the ease and welfare of mankind. The result is, that he shows Malthus to be in error when he asserts that "the misery produced by government is slight and superficial, compared with those deep-rooted seeds of evil which have their origin in the principles of human nature." So far from this being true, it is clear from the history of all times and of all nations, that bad government, unwise laws, and false religions, invariably induce social misery and degradation. Bad government renders property insecure, and therefore industry is relaxed, for wealth cannot be accumulated, and if wealth cannot be accumulated, artificial wants will not be developed, because they cannot be enjoyed. Nor can "the desire of bettering one's condition" exist if gradations in rank be interfered with by *castes* or sumptuary laws; or annihilated by despotism, whether aristocratic or ultra-democratic. Good government and wise laws have invariably contributed to the welfare of man.

Mr. Alison and Dr. Motard are agreed in several points. One we will notice. Of the nations of the north the latter author observes, when discussing the moral influence of climate:

"In the northern latitudes man requires an abundance of food, and as the soil does not supply it, necessity and habit render him carnivorous. He is consequently shepherd and hunter, nomade and sanguinary, warlike, and inured to danger. Such being his character, he despises the peaceable inhabitant of a more fertile soil. The warlike German tribes who left their forests to overrun Europe, and the Scythians who have invaded Asia from the most remote periods, always exhibited these characteristics. But none have shown them in so terrible a manner as those robbers of the Mongolian race, who are accustomed to sweep in their chariots over that immense plain of Northern Asia which extends from China to the Oby, and from the Great Desert to the Icy Ocean. Three times in particular have they terrified the world by their fearful irruptions, massacring and burning all before them; and leaving nothing in their track but the quiet of the desert and the stillness of the tomb. Attila, their first leader, wished that no grass might grow where his horse had trod; and Tamerlane, the most civilized of them all, erected at Bagdad, a horrible trophy of 90,000 human heads. Depopulated Asia still shows traces of their presence." (Motard, tom. i. p. 106.)

It is further shown that the inhabitants of northern climates are the most virtuous, and exhibit most of the dignity of the human character, in accordance with the doctrines of Montesquieu. Morals are more depressed as we approach to the southern latitudes; the passions more violent, crimes more numerous. Dr. Alison thus writes:

"How much soever wealth and luxury may corrupt their possessors, in the rich and fertile districts of the globe, there are always some situations in which the influence of such sources of depravity cannot be generally felt. In the cold and sterile regions of the north, and in the desert plains of the south, wealth cannot be accumulated, and men decay. Scythia and Arabia, accordingly, have in every age been peopled by inhabitants leading the same nomade life, and possessing the same active and hardy character. .... If the Scythians and Arabs do not share in the improvement of civilization, they are not weakened by its vices; if they are stained by the cruelty of savage manners, they possess the energy of the savage character. It is from these great high-lands of humanity that the stream of conquest has in every age flowed down upon the inferior scenes of existence. It is in their recesses beyond the reach of dissolution, that the energy necessary to sustain the human character is prepared; like the glaciers in the physical world, which the sun of summer is unable to dissolve, and from whence, in the solitude of the Alps, those undecaying foun-

tains are fed which spread life and fertility through the surrounding plains. . . . . When an empire had risen to eminence in ancient times, its inhabitants became corrupted, and its cities swarmed with a degraded population. But the shepherds of the north were always the same, and in the solitude of their deserts, nature was preparing the regeneration of the world. While the civilized empires of the east were advancing in wealth, in numbers, and in wickedness, the wandering tribes of Scythia were preparing to burst from their deserts in quest of subsistence. Unmarked as it was, amid the blaze of military glory, the fatal hour was approaching which was to witness their fall; and the cloud, which seemed at first only a speck on the horizon, swelled till it had buried the universe in its darkness." (Alison, vol. i. p. 264.)

Taking a practical view of the high questions here started, we should doubt much, as physiologists, whether any system of public hygiène could effectually resist the influence of an enervating climate on man, or modify the thick neck and broad jaws of the Mongol, so indicative of his destructiveness. It seems to us that the customs and habits created by climate induce changes in the cerebral organization of nations, as well as in the muscular and osseous conformation, and that the mental and corporeal qualities which result from these changes becoming hereditary, characterize the race. We know that lower animals when subject to change of climate and habits, or in other words, when domesticated, acquire new instincts contemporaneously with peculiarities in the form of the body; and that distinct breeds of the same species are thus developed. The character of a government is unquestionably regulated all over the world by the character of the people governed; and if the climate determine the character, by stamping its effects on the mental and physical organization of its inhabitants, how can we hope to discover or apply successfully principles of government or of hygiène, not simply powerful enough to resist its ever-acting influences, but able also to change those vices of organization which have been the growth of ages? All experience is against us. The moral character of the French at the present moment is given by Julius Cæsar, when he describes the character of the Gauls; and what says Mr. Alison of the nomade Tartar tribes, of China, with its throne supported by the press, or of India with its ancient civilization?

"The Tartars of the present day differ in no respect from their ancestors in the days of Herodotus; and in the manners of the wandering tribes who now infest the deserts of Mesopotamia, we are transported to the days when Abraham sojourned in the land of Urr." (Vol. i. p. 265.)

"To whatever cause it may be owing, nothing is more certain than that the government and institutions of the oriental states are precisely the same at this time as they were at the earliest period of which history makes mention. The descriptions of Porter, of Buckingham, of Morier, of Fraser, differ in no respect from the picture which may be gathered from the graphic sketches of Herodotus; and the most faithful portrait that ever has been given of the present manners of Bagdad and Ispahan, is that which for a thousand years has given delight to every successive generation, in the Arabian Night's Entertainments." (Ibid. p. 399.)

The physiological doctrines we have advanced would be incorrect, if facts were otherwise; but the melancholy practical inference to be deduced from the theory and the facts is, that it is just as easy to educate a Bengalee into a Mongol, or an Italian greyhound into a Newfoundland dog, as to teach the Hindoo how to enjoy and maintain a free govern-

ment. Ten centuries would be uselessly spent in the attempt to annul the *climatic* effects of fifty or sixty, perhaps of a hundred.

We do not make these statements with any other wish than to render philanthropists and legislators aware of the important assistance they may derive from a knowledge of medicine, or rather of the absolute necessity they are under to acquire and act upon such knowledge, if they would act aright. And with this object in view, we will mention other considerations arising out of the subject. Great Britain in establishing her colonies is in reality founding empires, which at some future period will be greater than any that have yet appeared in Asia. "There is something solemn," says Mr. Alison, "and almost awful in the incessant advance of the great stream of civilization, which in America is continually rolling down from the summit of the Alleghany mountains, and overspreading the boundless forests of the far west. Nothing similar was ever witnessed in the world before." The plundering multitudes of Scythians and Tartars that poured into Asia are as nothing compared to this torrent. Not less than 300,000 persons, almost all in the prime of life, now yearly cross the Alleghany mountains, and settle on the banks of the Ohio or its tributary streams, and they settle never to return. Should the inhabitants of the United States continue to increase at the same ratio as they increase now, in fifty-eight years they will number 100,000,000 of souls. The basin of the Mississippi alone, if as densely peopled as the British Isles, (it is incomparably more fertile,) would contain upwards of 350,000,000 of inhabitants; but under favorable political circumstances it would easily support more than 1000,000,000. The same process is going on in Australasia and Polynesia. "We behold the British race peopling alike the western and southern hemisphere, and can already anticipate the time when 200,000,000 of men on the shores of the Atlantic and in the Isles of the Pacific, will be speaking our language, reading our authors, glorying in our descent." (Alison, vol. ii. p. 348.) Need we say that the responsibility of British statesmen and of the British nation is most solemn? In two or three centuries a larger population than exists in the whole of Europe will curse or bless us according as we have given a bias for good or evil to their infant institutions. Reverting to our previous remarks we would, as physiologists, warn our colonial secretary and our colonizing companies against the mixture of superior with inferior races of men; the Hill Coolie with the Highlander, the African with the Englishman. Liberty and a real equality are necessarily coexistent; and we should have no difficulty in proving that such a mixture of races will inevitably lead to slavery and despotism, to destructive foreign and civil wars, and a retrograde civilization; and for the primeval solitudes now being broken upon, will substitute the silent ruins of desolated cities.

Climate will undoubtedly change the character of the English race. It changes it in India; it is changing it in the United States, and in less than a century will dissolve the union. It is of importance, then, in marking the limits of new colonies, to consider the ultimate effects of climate, and place natural boundaries between them. When the United States separate, the northern will coalesce with the Canadas, and these unitedly will constitute the dominant empire of the western continent, and perhaps of the world. These changes will hardly take place without

wars; and the length and destructiveness of these wars will depend considerably upon the nature of the boundaries, and the compactness of the territories to be defended. Portions of our empire in India might be garrisoned by colonies. The climate of the high lands in central Asia so nearly resembles our own (as do also the inhabitants ourselves), that Englishmen would not deteriorate there; and would do more for the civilization of Asia and the glory of England than innumerable colleges and missionaries in Hindostan. We would close our observations on this part of the subject with the hope that British statesmen and legislators will ere long be able to estimate properly the lives of Englishmen as the lives of men springing from a race in whose cerebral organization the ideas of rational freedom and self-government are stereotyped by climate, laws, and habits, and so have become instinctive; and will adopt those measures best calculated to enhance the value of such a population by lessening the mortality and ameliorating the condition of the labouring poor.

It is with the means best calculated to effect this purpose that the second volume of Mr. Alison's work is principally occupied. In the first chapter the results following the acquisition of landed property by the poor are discussed, as well as the nature of the government, when land is possessed by few or many proprietors. France, during the revolution, uprooted her hereditary aristocracy, and repealed the law of primogeniture. She thus, according to Mr. Alison, created 10,872,000 landed proprietors, whose territorial possessions average eleven acres and a half, and their clear annual profits £4 5*s*., or half the value of the produce. Mr. Alison thinks that by these two acts, France exchanged European for Asiatic civilization; and asserts that her peasants are fast sinking into the condition of the ryots of Hindostan. But what was the condition of France before the revolution? The despotism of her court was oriental, the sensuality and servility of her nobility were oriental, the oppression of her peasantry was oriental. The fruits of orientalism were ripe when the revolution took place; the gathering of them in fact constituted the revolution. The government of France is oriental still; but not because the land is subdivided. It is the character of the people that determines the character of the government, as we have before observed, a general principle illustrated by the history of France. The extreme division of land has not led to that increased cultivation and increased produce which might have been expected. In Great Britain, according to Dr. Motard, each agriculturist produces annually to the value of 720 francs, or £28 16*s*.; in France it is only 234 francs, or about £9 7*s*. (vol. ii. p. 239.) And why? The French *peasantry*, whether in France or Lower Canada, are oriental in their nature; they are "not given to change."

The eleventh chapter is on the moral evils and management of the poor in great cities; a subject peculiarly within the scope of hygiène, even in its more limited sense. We have a fearful description of urban populations, especially of our own, and of the evils which will inevitably result if we continue to neglect all means of ameliorating their condition. In Paris *every third child is a bastard*, and *one sixth* of the whole population die in the hospitals; in London one tenth of the whole population are paupers, and 20,000 persons rise every morning without knowing

where they are to sleep at night; at Glasgow nearly 30,000 persons are every Saturday night in a state of brutal intoxication, and *every twelfth house* is devoted to the sale of spirits; and in Dublin 60,000 persons in *one year* passed through the fever hospital. These are undeniable facts. Mr. Alison testifies to the correctness of the following account of the wynds in Glasgow given by Mr. Symonds, the government commissioner for examining into the condition of the hand-loom weavers:

"'The wynds in Glasgow comprise a fluctuating population of 15,000 to 30,000 persons. This quarter consists of a labyrinth of lanes, out of which numberless entrances lead into small courts, each with a dunghill reeking in the centre. Revolting as was the outward appearance of these places, I was little prepared for the filth and destitution within. In some of these lodging-rooms (visited at night), we found a whole lair of human beings littered on the floor, sometimes fifteen and twenty, some clothed and some naked; men, women, and children, huddled promiscuously together. Their bed consisted of musty straw intermixed with rags. There was generally little or no furniture in these places; the sole article of comfort was a fire. Thieving and prostitution constitute the main sources of the revenue of this population. No pains seems to be taken to purge this Augean pandemonium, this nucleus of crime, filth, and pestilence, in the centre of the second city in the empire. . . . . A very extensive inspection of the lowest districts of other places, both here and on the continent, never presented anything one-half so bad, either in intensity of pestilence, physical and moral, or in extent proportioned to the population.' (Arts and Artisans at Home and Abroad. By J. C. Symonds, Esq., p. 116, et seq.)" (Alison, vol. ii. p. 89, note.)

Of course in such a population fever slays its thousands. The mortality in the Niger expedition raised a dreadful hubbub, and the public was frantic when it heard of the Cabul massacre. But how little has been said about the wholesale destruction of our countrymen at home! The average annual mortality over all England is 1 in 51; it was 1 in 41 in Glasgow in 1822; in 1835 it had risen to 1 in 29·53; in 1836 to 1 in 26·68; in 1837 to 1 in 24·20. Advocating the necessity for a legal provision for the poor in Scotland, Mr. Alison makes observations which are quite applicable to a system of public hygiène.

"Of the dreadful danger of such a state of things, and the manner in which it speedily comes to affect the higher orders in their lives and property, if they cannot be reached through any other and more honorable channel, decisive proof is afforded by the facts that no less than *twenty thousand* persons were seized with typhus fever, the well-known attendant on want and misery in Glasgow, in the single year 1837, of whom 2180 died; that 40,000 persons have had fever in that city within the last three years; that 10,000 persons have had fever in Dundee in the last four years; that in 1838, 1 in 30 in Edinburgh was a fever patient." (Vol. ii. p. 219.)

Dr. Motard gives similar statistical details; but statistical details of this kind are as dry as they are horrible, so we will exchange them for the opinions of the Bishop of Strasburg. Dr. Motard quotes these from an article which appeared in the "Constitutionnel" for July 15th, 1840. It may be a melancholy satisfaction to know that we are not the only sinners against the labouring poor:

"I have surveyed," says this prelate, "from every point of view one of the departments most celebrated for the grandeur and prosperity of its manufactures, and I could only mourn over the sanatory and moral condition of that country. I shuddered to learn that in these centres of industry the youth of

both sexes abandoned themselves to unrestrained licentiousness, and that, in consequence, a population formerly healthy and handsome was deteriorating in the most alarming manner. In addition, the constitutions of the working people are debilitated by their sedentary life and the close foul air of their workshops. I saw poor children going in the evening to these palaces of industry to work all night and earn a few pence, the miserable return for their injured health and wasted youth. The complexion of these victims of avarice was pale, their cheeks hollow, their countenances haggard and deformed. The unhappy children marched with a weary step to their place of torture. In fact, the physical deterioration in some of these establishments peopled by from three to four thousand operatives is so great, and the exemptions from the conscription so numerous in consequence, that a superior recruiting officer loudly declared if the general government did not promptly interfere to remedy the evil, the supply of soldiers from that department would fail altogether." (Motard, t. ii. p. 427.)

It is added from the same article, that at Mulhouse (Mulhausen), formerly celebrated for a fine race of men, the deterioration was so great that for every hundred conscripts found fit for military service, 100 are rejected; but at Rouen 166 (we cannot help giving the figures), and at Elbœuf 168 are rejected for every hundred passed.

Dr. Motard accuses Birmingham of having first introduced the system of infant labour, by commencing a traffic in children with London. This trade, he says, was carried on with such cool impudence that an agreement was entered into between a parish in London and a Lancashire manufacturer, in which it was stipulated that the latter should take one idiot for every twenty healthy children sent to him! The French manufacturers were afterwards compelled to adopt infant labour in self-defence, and now employ more than 100,000 children in their cotton factories alone. (Tom. ii. p. 373.) Mr. Alison apprehends the greatest danger from this system of employing children in factories; he does not appear to have been aware of the horrors of coal mines.

"It may be affirmed, without hesitation, that the system of employing children in these great establishments is the most ruinous to the moral character and habits of increase in a nation that human ingenuity ever devised; and that, if either experience does not discover some remedy for these evils, or Nature, in some way to us inscrutable, does not work out its own cure, the empire will in the end be overturned by their effect." (Vol. i. p. 531.)

"The constant employment of the young in manufactories for fourteen hours a day renders it almost impossible for any education to do them much good; for who, after such a period of daily toil, could sit down to the additional labour of learning anything. It is almost barbarity to propose it............ Nothing but the strong hand of government, and an assessment reaching the vast funds of the selfish and indifferent as well as the humane, is adequate to the remedy of the evil. Whether such a task will ever be undertaken by the legislature, or submitted to by the country, may be doubted; but this may be affirmed, without hesitation, that if this great duty is not discharged, and that, too, without delay, by the nation, the seeds of ruin are, by the laws of God, sown—and justly sown—in the community; and that such will be the depravity and wretchedness of the people on whom the visitation will fall, that even Timour, with his pyramids of ninety-thousand heads, would be deemed a messenger of mercy to mankind." (Ibid. p. 534.)

A factory commissioner, Mr. Tufnell, states in his report that the children that stand at the mules for twelve hours every day have little or nothing to do; and hints that it is quite practicable for them to indulge in literary pursuits! But Dr. Motard, in describing the external aspect

of Manchester, "de l'orgueilleuse Manchester," and its population, describes the results of infant labour. We transcribe his pen-and-ink sketch of this manufacturing metropolis:

"As you approach its atmosphere the elegant appearance peculiar to the English villages disappears, and in the suburbs noisy pot-houses take the place of the charming, quiet cottages. What road is that? Why those lugubrious, dull sounds—those blackened walls—that smoky atmosphere? What hell have we dropped into? Everywhere there is the livery of wretchedness, everywhere the degradation of vice: rags—livid, sinister-looking countenances—hoarse voices, mingling the accents of misery with the shouts of brutality. Men aged before they are old, rickety children, women no longer like their sex; everywhere an etiolated population, everywhere furious groups demanding bread. From whence come those vociferations? It is the riot of the famished, the most implacable of all, the prelude to pillage, and robbery, and incendiarism. Oh, Manchester! in whose palaces of industry which human genius has created within thy walls, how often has hideous famine polluted with its rags thy brilliant wonders! How often hast thou seen thy thousands of operatives seize with seditious hand the bread of the rich! In fact," &c.

And then Dr. Motard clinches his poetry with the prosaic dates of riots, sabreings, and soup-kitchens.

Whatever may be the cause, it is quite certain that extreme poverty and incredible misery are found in large manufacturing populations. Mr. Alison connects these evils with the unequal distribution of wealth, and mentions Ireland, an agricultural country, in illustration. Dr. Motard shows numerically that pauperism increases not in the ratio of the population, but in the ratio of the commercial and manufacturing prosperity. In ten years (from 1790 to 1800), the population increased at Philadelphia 29 per cent., the paupers 104 per cent.; from 1800 to 1809 the latter increased 79 per cent., the population only 36 per cent. (Tom. ii. p. 377.) But Glasgow itself is a much more striking example of this general fact.

The remedies for these evils are of a mixed character. Mr. Alison sees in the constant improvements in machinery a providential check upon the increase of the manufacturing masses and a stimulus to increased agricultural occupation. Education, but particularly a religious education, emigration, and a legal provision for the poor are the principal means he thinks necessary to be adopted. He also discusses the corn laws and the reciprocity system, but with these we have nothing to do. A system of medical police is one of his minor measures for the improvement of the population in large cities; and he would have it enforced by law.

"It is in vain to say that this matter (the building of streets) may, without danger, be left to the interest of individuals, and that every proprietor should be allowed to lay out his property to the best advantage. The interests of the higher and middling orders, indeed, may be safely intrusted to their own keeping; but it is neither just to the poor nor expedient for the public, to leave the indigent classes at the mercy of their superiors............ It is in protecting extreme indigence from the necessities to which it would otherwise be compelled to submit, and in enforcing police regulations—important alike to the health, the manners, and the morals of the lower orders—that the power of government is most beneficially exerted. Like the laws of quarantine or of public cleanliness, such regulations are necessary to enforce those salutary rules, which avarice is always ready to violate, and indigence too often unwilling to obey." (Vol. ii. p. 127 et seq.)

Mr. Alison founds an argument in support of a legal provision for the poor upon the general principle that mendicity increases where property is unequally distributed. We may add that Dr. Motard has similar views, but not being clearly worked out, they are not clearly expressed. This argument of Mr. Alison's being equally powerful in asserting the justice of a system of public hygiène, we are induced to quote it, premising, that it is not a favorable specimen of Mr. Alison's style:

"The accumulation of the labouring classes in great masses in the employment of their superiors is eminently favorable to the increase of wealth. The profits made by master-manufacturers or merchants who have numbers of the poor in their employment are prodigious............... All the other classes share in the advantages of the wealth which is thus created:—the agriculturists in the increased market for their produce which is thus created—the tradesmen and artificers in the augmented demand for their productions to which it gives rise. The operative workmen, perhaps, share least in the advantages of the wealth which their industry has created: degraded and demoralized by the habits which it induces, they are almost precluded from acquiring any permanent benefits from the skill which they possess. In these circumstances, the security of a provision in sickness or old age is nothing more than a fair compensation for the risks which they run and the contamination to which they are exposed: for health which is undermined, morals which are corrupted, foresight which is destroyed, sensuality which is disseminated. To resist such evils is as impossible, in certain circumstances, as for soldiers to preserve their health amidst the dysenteries of the camp, or the contagion of the hospital: to relieve them is the duty equally of a wise general and beneficent government." (Vol. ii. p. 186.)

The operative is in fact a serf (to speak plainly), without the protection of a master. He is not *adstrictus glebæ*, but he is bound by the iron band of his necessities to a course of life fraught with the evils Mr. Alison so correctly details. His employer, unlike the feudal baron, has no immediate interest in his welfare, is not directly dependent upon his arm for power, for protection, or for the necessaries of life. Labour is so abundant that he is virtually a slave, yet without the animal comforts of a slave; his necessities constituting a most efficient substitute for the driver's whip and collar. Education opens his eyes only to show him that he is irremediably naked; it gives him the desire to better his condition without the means; and what, we ask, will men, when goaded on by pride, rancorous envy, vicious sensuality, and the desperation of intolerable poverty, hesitate to do?

If we do not at once express our concurrence with the gloomy views of Dr. Motard and Mr. Alison, it is because they have altogether neglected to consider a most important, perhaps the most important product of modern civilization. For the first time in the history of mankind the medical profession stands alone, uncontrolled by the monarch or the priest. Hitherto it has never been powerful but as the servant of tyranny and superstition; now it depends on itself only whether it shall serve the cause of justice, mercy, and truth. Its individual members cultivate science for the benefit of humanity and to alleviate human misery in its detail; but the necessities of the age imperatively demand that they shall act as a body in the state, that collectively they shall place themselves in the same relations to society as, individually, they bear to their individual patients. The united profession must insist that its reasonable demand to be intrusted with the execution of plans for securing the health and ease

of the community be conceded. To be the appointed instruments of carrying out a system of general hygiène is its right; to constitute the machinery by which natural science may be applied to the amelioration of the evils of society is its duty. The natural sciences have been mainly developed by the profession; and, if natural science in aiding manufacturing industry has indirectly created a great amount of human misery, the profession which has nurtured it to its present pitch of power must apply that power to the removal of its own baneful results. We agree with Dr. Motard when he asserts that it is medicine alone which can untie the gordian knots of modern civilization. But medicine must be *applied* to society in all its ramifications; and it is the medical profession that ought to, indeed that only can apply it. It is the medical profession which can most effectually step in between capital and labour; and not simply show, theoretically, how the condition of the labourer may be improved, but so guide and control public opinion, that avarice itself shall be made subservient to that improvement. The modern slave may then possibly be placed in that position in which it will be well for himself and well for society; and a servile war in Europe, the most terrible blow that could be struck at the present or future happiness of mankind, would be prevented. We believe the political institutions of all other nations are such that this arduous duty will devolve upon the medical profession of the British empire. Religion apart, we can imagine no greater or nobler enterprise in which a body of educated men could engage than this. The concentration of moral and physical science on the physical and moral advancement of mankind must, confessedly, be a great object for even such a body of men to aim at; but it is a greater to become the rallying point and centre of action for that active and zealous philanthropy which happily grows with the evils of the age, and wants but an enlightened guidance to be equal to the task of counteracting them.

Self-sufficient shrewd people may laugh and say "there's nothing like leather;" but we appeal to the medical profession, and ask them if the relations they bear to society are not of surpassing importance. The study of modern medicine is the study of human nature, and being such there are no questions concerning humanity which it may not assist to solve. This may be generally inferred from our sketch of Dr. Motard's essay, and we have given a solitary instance of its application to one of the most profound political questions of modern times, namely, the founding of colonies. Our mission enterprises, diplomacy, and commerce have been admirably served by medical practitioners, and might be to a much greater extent. Forensic medicine, especially in its relations to the doctrines of free-will and moral responsibility, is as yet scarcely developed. Eleemosynary medicine or the relief of the sick poor is equally imperfect; and so is public hygiène, whether we consider the application of it to the arrest of epidemics, the purification of the air of cities, the construction of streets and public buildings, or the management of manufactories, workhouses, prisons, &c. Our quarantine laws are ridiculously absurd. No systematic attempt has been made to foresee bad harvests or the recurrence of epidemics, and so to ameliorate these unavoidable evils by anticipating them. We have no medical observatories, yet this good is within the scope of medicine. We need not state how vast is the im-

portance of a right system of medical education if the duties and power of the profession be such as we have stated. These are only a few and the most obvious of the important relations which medicine bears to society.

In giving a general estimate of the works before us, we would observe that the error in style so apparent in Dr. Virey's is also apparent in Mr. Alison's essay, and we may even add, in Dr. Motard's. The object of hygiène is a great and good object; it is in fact the great object of Christianity, and therefore is essentially noble and godlike. Writers on hygiène feeling this, are easily betrayed into a corresponding grandeur of diction which appears ridiculous to those whose thoughts have not been occupied with the subject. In short it looks like cant, and so tends to retard the practical application of the sciences. Great truths are most touching and most attractive when expressed in simple language; besides, writers who declaim much are apt to think when they have done that they have done enough. But that is not a sound opinion. "*Sola laus virtutis* in actione *consistit*." We must *act*, and act on a scale, and with an energy and a determination proportionate to the greatness of the object to be attained.

Dr. Alison's principal defect is diffuseness. His "Principles" are valuable; and we shall be happy if any remarks of ours induce the profession to read his work, for we think that they will learn from the perusal to appreciate their own importance and usefulness to the community. If the whole were condensed into a half-guinea volume (an easy task), it would be more within the reach of many of our readers.

Dr. Motard's essay exhibits all the comprehensiveness and system which so eminently characterize French literature in general, besides much of that practical merit we claim as peculiar to our own. It would almost bear translation. The book is imperfect; but it is imperfect mainly because it occupies only two octavo volumes widely printed. In the present state of the science general hygiène cannot be thoroughly discussed in two such volumes; it is too rudimentary and requires too many details.

We shall take an early opportunity to discuss our own system of public hygiène, and notice especially the reports which within the last few years have been made to parliament by commissioners appointed to inquire into the health of towns and the condition of the manufacturing and mining populations. We shall examine their contents and discuss the relations they bear to the medical profession,—to *physicians* in the true meaning of the term, and not to mere curers of aches and ailments. We indulge a faint hope that in discussing the questions connected with general hygiène, we shall convince Mr. Whewell that medicine is actually a science after all he has said to the contrary.

Art. XIII.

Recherches Physiologiques et Cliniques sur le Liquide Céphalo-Rachidien ou Cerebro-Spinal. Par F. Magendie.—*Paris*, 1842. 4to, pp. 164. With a Volume of Plates in Folio.

Physiological and Clinical Researches on the Cerebro-Spinal Fluid. By F. Magendie.—*Paris*, 1842.

In his preface to this essay, M. Magendie complains that justice has not been done to his former memoirs on this subject, and that the existence of the cerebro-spinal fluid, though mentioned by some writers, is passed over without any notice by most.

"This silence, however," says he, "does not surprise me, for the fluid to which I have given the name of *céphalo-rachidien* has always met with a singular fortune; everybody has seen it, it is impossible to open the skull without its flowing away under the eyes of the anatomist: it has served more than one as the basis of ingenious hypotheses, but nevertheless no one has yet devoted to it that serious attention which it deserves. Most medical men still regard it as a morbid product, while it is in reality one of the physiological conditions of life in man and in many animals.

"My intention, in choosing it as the subject of a special work, has been to place the fact of its normal existence beyond doubt, and to secure for it among the elements of the organism, that place which ought to be allotted to it." (Preface.)

M. Magendie appears to us to have greatly overstated the neglect with which his discovery has been treated. Before the publication indeed of his first paper in the *Journal de Physiologie* for 1825, the observations of Cotugno relative to the existence of a fluid after death in the cavities of the brain and spinal canal had been forgotten, and the fluid of the ventricles was generally regarded as the result of a morbid process. M. Magendie, to whom the investigations of Cotugno were at that time unknown, ascertained the normal existence of this fluid during life, and proved that it was not produced, as had been supposed, by the condensation of a vapour after death; he discovered its true situation to be between the arachnoid and pia mater, and satisfied himself of the existence of a distinct aperture of communication between the ventricles of the brain and the sub-arachnoid tissue of the spinal cord. The two former of these facts are now universally admitted, and the third is received by most anatomists, while all agree that for an acquaintance with them we are indebted to M. Magendie. The general adoption of his opinions, while no one sought to wrest from him the merit of having been the first to propound them, would have satisfied the craving for praise of most persons. Not so, however, with M. Magendie; from the date of his first communication in the Journal de Physiologie to the present time he has not ceased to reiterate his discovery; a great part of the first volume of his *Leçons sur les Fonctions et les Maladies du Système Nerveux*, published only three years ago, is occupied with the subject, and in the work now before us we have a *réchauffé* of former papers, without a single additional fact of importance. As, however, it is possible that many of our readers, though familiar with M. Magendie's results, are less perfectly acquainted with the grounds on which his conclusions rest, we have thought it desirable to prepare an analysis of the work before

us; and to them we think the following pages will not be without interest.

M. Magendie's first proposition is that the brain does not completely fill the cranium, while the spinal cord is very far from occupying the whole of the vertebral canal. With reference to the spinal canal this fact is extremely obvious, as is shown in plate iii. figs. 4, 5, 6, and 7. In old persons, particularly in such as have sunk into a state of dementia, a similar condition of the brain is very evident, which, shrunk from its natural proportions, is far from filling up its bony case. If, bearing in mind the appearance presented in old persons, the anatomist examines the head of the adult or child, he will perceive that in them the brain by no means fills the cranium, but that there are between them many interspaces: or, if he should chance to examine a case in which this state of things does not exist, the condition of the brain will strike him at once as an exception to the general rule and the effect of some morbid process. He will find that the cerebral convolutions are flattened, that the sulci have disappeared, and the hypertrophied brain, instead of its peculiar undulating surface, will present a smoothness and a polish like that of a plaster cast, giving an exact copy of the interior of the skull.

These facts, and the frequency with which we read in accounts of post-mortem examinations, mention of the escape of fluid on opening the cranium, may be admitted as proofs that the brain and spinal cord do not completely fill the cavities which contain them. To prove the existence of a fluid in the cranium and spinal canal is the next step of the enquiry. Cotugno, though aware of the existence of such a fluid in the dead body, doubted whether it was present in the living subject, and the experiments on dogs which he instituted for the purpose of ascertaining this point, led him, though erroneously, to decide the question in the negative. M. Magendie observes that if the arches of the vertebræ of a living animal are removed, and the dura mater of the cord is exposed, the membrane will not collapse on the admission of air, but will continue to fill up the spinal canal. If now a puncture is made into the membrane, a jet of fluid will take place, and the dura mater will gradually wrinkle, collapse, and become closely applied to the spinal cord. The same phenomena may be noticed in examining a body immediately after death, but after the lapse of some hours, the greater part of the fluid becomes imbibed by the surrounding tissues, and the dura mater presents the same wrinkled appearance as it does when the fluid has been evacuated by puncture.

The existence of this fluid admitted, its seat is the next question which suggests itself. The supposition that it is contained in the cavity of the arachnoid, though very natural, is erroneous. It is found in two distinct localities: first, in the interspace between the arachnoid and pia mater; second, in the cavities of the cerebrum and cerebellum in the human subject, and likewise in those of the olfactory nerves, optic lobes, and spinal cord of some animals.

Closely as the arachnoid in many points resembles other serous membranes, it yet presents some peculiarities of arrangement which have been overlooked, owing to the little attention that has been paid to the existence of the cerebro-spinal fluid. Instead of adhering closely to the organ it invests, as the pericardium does to the heart, it is separated

from the brain by a tolerably wide interspace, in which this fluid is situated. The whole spinal cord is thus bathed in the fluid, which forms a layer wider in some parts, narrower in others, according to the shape of the canal and the size of the cord in different places. One result of its presence, even in those situations where it is least abundant, is that the nerves float in it, and are thus kept separate from each other instead of being in close contact as they appear in the dead subject. In the skull the disposition of the fluid is similar, and the nerves are bathed in it to their exit from the cranium just as in the spinal canal. There are some situations, however, where it accumulates in such large quantity that they may with propriety be distinguished by the name of *confluences*.

"The first or posterior confluence, which is by far the largest, is situated below and behind the cerebellum. The second or inferior confluence is in front of the annular protuberance and between the crura cerebri, and lodges the basilar artery. The third or superior confluence is placed behind, above, and on the sides of the pineal gland. The fourth or anterior confluence exists above the decussation of the optic nerves, and below the pars cinereum which closes the third ventricle below and in front. Two other smaller confluences, lateral confluences, might be mentioned which bathe the ganglion of the fifth pair on each side." (p. 25.)

The same fluid which occupies the sub-arachnoid tissue penetrates, according to M. Magendie, into the ventricles. In the discovery that the fluid of the ventricles communicates with that in the spinal canal, the French physiologist was anticipated by Cotugno, but the question as to the way by which this communication is effected still remains undetermined. There is said to be an opening, called from its discoverer the *foramen of Bichat*, by which the arachnoid passes between the venæ Galeni, just where they enter into the straight sinus. The existence of this aperture is denied by Magendie, and in his scepticism on this point he is supported by many authorities, and by the researches of Martin St. Ange, which were specially directed to this subject. Some anatomists, however, whose opinion is entitled to great weight, admit the existence of the foramen of Bichat. Among their number is Arnold, whose *Annotationes de velamentis cerebri et medulla spinalis* deserve a different treatment than to be passed over in utter silence. But, even on the hypothesis that the foramen of Bichat is a natural and not an artificial opening, it would not at all answer the purpose of a means of communication between the ventricles and the subarachnoid tissue, since it leads, according, to Bichat, into the cavity of the arachnoid.

"But the real, constant, and normal opening, by which the cerebro-spinal fluid invariably passes in entering or in leaving the ventricles, is not at the place mentioned by the illustrious author of the *Anatomie générale;* it is to be found at the lower end of the fourth ventricle, at the spot called the *calamus scriptorius* by old anatomists. To convince one's self of the existence of this orifice, nothing more is necessary than to lift up and to separate slightly from each other the lobules of the inferior vermiform process of the cerebellum; when, without tearing any of the vascular adhesions, which connect this part of the cerebellum with the pia mater of the cord, the angular excavation which terminates the fourth ventricle is distinctly evident. Its surface is smooth and polished, and is continued into the ventricle of the cerebellum. Such is the anterior part of the opening: its lateral and superior parts are formed by the choroid plexuses of the organ, and by a lamina of medullary substance, the valve of Tarin, which varies much in size, and which is attached to the projecting part of the side of

the fourth ventricle. As to the form and dimensions of the opening, they vary much in different subjects and according to the quantity of the cerebro-spinal fluid, and when this fluid is very abundant, the opening will admit the end of the finger. Usually, and when no more than the normal quantity of fluid is present, the aperture scarcely exceeds two or three lines in diameter in every direction. It is also frequently divided into several compartments by vessels which pass from the medulla oblongata to the cerebellum. Sometimes it is contracted by one or both of the posterior arteries of the cerebellum which pass across it."* (p. 28.)

On a subject still open to doubt, as is most certainly the existence of this aperture, which is not admitted by Arnold, some notice of the arguments against it might be looked for, but are not found in this essay of M. Magendie. They are, however, very fairly stated by M. Cruveilhier in his Anatomie Descriptive under the following heads:

1st. The form and disposition of the opening do not present any of the characters of a natural aperture, but it is irregular in shape, and its edges have a torn appearance, while a little triangular tongue of membrane may be found attached to the inferior vermiform process of a size and shape such as exactly to close the aperture. We may also add that the description given by Martin St. Ange tallies far less with the characters of a natural opening, than does that which we have quoted above from M. Magendie. St. Ange says expressly that it is "a mere cleft, by no means such an opening as had been asserted by some, and it is impossible to assign to it any dimensions, since sometimes there is no appearance of an opening. In short it is not a free and distinct canal; but vessels and cellular filaments pass in front of it."

2d. In the dog and sheep the fibrous lamina which forms the floor of the fourth ventricle is not perforated, and M. Cruveilhier has met with a similar arrangement five or six times in the human subject, in cases where not the slightest trace of any morbid process was to be found either in the membranes or substance of the brain or spinal cord.

3d. In many cases of chronic hydrocephalus, the ventricles contained several pounds of fluid, while none whatever was present in the sub-arachnoid cellular tissue.

4th. In the brain of many children who have died with all the symptoms of acute hydrocephalus, M. Cruveilhier has found the lateral ventricles extremely capacious but empty, and the question has suggested itself to him whether in these instances the rupture of the membrane has not given passage to the fluid, while in ordinary cases it prevents its escape.

Notwithstanding these facts, M. Cruveilhier is induced to admit the normal existence of this aperture: 1st. From finding it, in the immense majority of cases both in the fœtus and in the adult, even when the brain has been removed with the greatest care. 2d. From the fact that in apoplexy of the ventricles bloody serum is always met with in the sub-arachnoid cellular tissue. 3d. From observing that if a coloured fluid is injected into the ventricles of the brain it always penetrates into the sub-arachnoid cellular tissue, and vice versâ.

This last occurrence might, we think, be adequately explained by endosmosis, and Martin St. Ange's investigations do not by any means

* Reference is made to drawings of the opening, but none have been published in the volume of plates.

clearly establish the existence of a distinct opening. He suspended bodies by the head or by the heels, and found that the fluid of the ventricles passed into the sub-arachnoid tissue, and that of the sub-arachnoid tissue into the ventricles, according to the position of the body. This did not, however, take place immediately, as it might be expected to do if it flowed through a distinct opening, but he says "*Cette transmission de liquide se faisait il est vrai, avec plus ou moins de facilité, et quelquefois même avec beaucoup de lenteur.*"*

In his estimate of the quantity of this fluid, M. Magendie agrees pretty closely with Cotugno. It varies according to the age and size of the patient, and usually bears an inverse proportion to the volume of the encephalon. It seldom, however, is less than two ounces, and often amounts to five ounces. M. Magendie regards the pia mater as the source of the secretion, which may be seen transuding from the membrane if it is exposed in a living animal. One striking point connected with its secretion is the rapidity with which, like the aqueous and vitreous humours, it is reproduced after having been evacuated. Within twenty-four hours after its removal, by puncture of the spinal canal, it will be found to have reaccumulated in as large a quantity as before, and this experiment may be repeated with the same result on several successive days.

The cerebro-spinal fluid is subject to movements synchronous with those of respiration, in which it advances towards the frontal extremity of the ventricles and then retires; but M. Magendie has never observed the fluid of the ventricles communicate in animals with that of the spinal canal. In the human subject, however, he does not doubt but this communication takes place, the opening of the fourth ventricle being so much more free in man than in animals, and in proof of this he appeals to the manifest distension of the brain produced by compressing the sac of a spina bifida. These movements are owing to the same causes as those of the brain, and like them, though very evident when the parietes of the cranium are removed, and the air exerts direct pressure on the organ, they are doubtless much less marked when the skull is entire.

The result of chemical analysis establishes, in M. Magendie's opinion, the fact that this fluid is of a peculiar nature, and that it differs essentially from mere serum. M. Lassaigne and M. Haldat found in it osmazome, albumen, various salts, and about 97 per cent. of water. M. Conerbe states that it contains the following elements, but has not mentioned the proportions in which they exist in it:

"1. Animal matter, insoluble in alchohol and ether—soluble in alkalis: it is analagous to the neurilema of the brain. 2. Albumen. 3. Cholesterine. 4. Cérébrote.† 5. Chloride of soda. 6. Phosphate of lime. 7. Salts of potash. 8. Salts of magnesia." (p. 50.)

M. Magendie regards the uses of the fluid as almost exclusively mechanical. It occupies all the irregularities of surface presented by the brain and spinal cord, it distends the membranes and transmits to the whole extent of the cranio-spinal parietes pressure made upon one part, and both from within and from without it presses on the encephalon. In other words, its uses are such as result from the known properties of

* Journal Hebdomadaire, Janvier, 1830, p. 104.

† The name applied by M. Conerbe to the white fatty matter of Vauquelin.

fluids. To these facts, which are stated with much unnecessary expenditure of words, we have no objection to make. But M. Magendie observes:

"The consequences of this pressure influence the general conformation of the head, and doubtless also of the spinal column from the earliest period of embryonic and fœtal existence, during which the parietes of the cranium and vertebral column are not yet solid. To what other cause are we to refer the spherical form of the head at a time when the brain is not yet in existence? The cerebro-spinal fluid protects the formation of the cerebro-spinal nervous axis, just as the fluids of the ovum, and especially the liquor amnii, defend and protect the whole fœtus in the cavity of the uterus. There must even exist a kind of antagonism between the cerebro-spinal fluid, which tends to distend the parieties of the cranium and the spine, and the fluid of the amnios, which compresses them on every side, and tends to make them assume the smallest possible volume." (p. 54.)

M. Magendie stated in a former part of this essay, that the fluid is secreted by the pia mater, but no trace of the membranous envelopes of the brain was found by Tiedemann before the eighth week, and the pia mater is not distinguished till even a later period. The earliest form which the brain presents is that of a number of vesicles filled with fluid, which gradually diminishes in quantity as the walls of the vesicles thicken, and at length entirely disappears in some parts, owing to the deposition of solid matter. Some relation may probably exist between the fluid of the cerebral vesicles and the sub-arachnoid fluid of after life. It would be interesting to ascertain the exact nature of this relation, and the solution of this question would necessarily throw light upon other still debateable subjects, and especially on that of the lining membrane of the ventricles—a point which Arnold has treated with less than his accustomed clearness. These are questions, however, the elucidation of which calls for patient and laborious investigation. Statements so loose and incorrect as those we have just quoted can advance neither the interests of science nor the reputation of their author.

The second part of the work treats of the cerebro-spinal fluid in disease, which M. Magendie refers to the two heads of alterations in quantity, or changes in character.

The quantity of the fluid may either be increased or diminished, and M. Magendie remarks, that "When once we become aware of the existence of the cerebro-spinal fluid the thought naturally suggests itself, that all accumulations of fluid in the interior of the cranium or spinal canal are merely accidental or morbid modifications of this fluid—a fact which it will be easy enough to establish." (p. 66.)

It has doubtless long before this struck our readers, that one question most intimately connected with the subject of this essay has been passed over by the author in utter silence. We allude to the nature and quantity of the secretion which takes place into the sac of the arachnoid—the differences between it and the cerebro-spinal fluid and the influences of disease upon each. Now, no person previously unacquainted with the subject could imagine, after a perusal of this work, that any secretion is ever formed in the sac of the arachnoid. But instead of investigating this matter, M. Magendie makes the broad assertion, that all accumulations of fluid within the cranium are produced by the liquo cerebo-spinalis, though there are cases of chronic hydrocephalus in

abundance in which the sac of the arachnoid has been found distended by fluid; and the plates of M. Cruveilhier furnish instances in which atrophy of the brain has coexisted with accumulation of fluid in the cavity of the arachnoid, as well as others in which this condition was associated with increase of the liquor cerebro-spinalis. There is, moreover, no invariable connexion between the accumulation of fluid in the ventricles and effusion into the sub-arachnoid tissue, as M. Cruveilhier candidly admits, though no allusion to such a fact is to be found in the pages of this essay. The observations of M. Cruveilhier refer only to cases of chronic hydrocephalus; but the same fact may be observed in the bodies of patients who have died of acute hydrocephalus—fluid being found in the ventricles when none is contained in the sub-arachnoid tissue, and the sub-arachnoid fluid being sometimes abundant in cases where the ventricles are found to be empty.

The obliteration of the aperture at the calamus scriptorius is mentioned by M. Magendie as a probable cause of the accumulation of a morbid quantity of this fluid in cases where it has been found confined to the ventricles. But after M. Cruveilhier's statement, that he has found this aperture obliterated without any morbid condition of the brain or spinal cord, we feel rather sceptical on the point. The influence of tubercles or other tumours of the brain in causing an accumulation of fluid in the ventricles is a fact with which the profession has long been familiar. The quantity of the cerebro-spinal fluid has been found increased in cases where some obstacle has existed to the venous circulation in the brain, or where there has been habitual congestion of the organ; also in instances where the brain is either partially or generally diminished in size. Under opposite circumstances it is found in less quantity than natural, as when the brain is hypertrophied or the cranial bones have acquired a preternatural thickness.

The diminution in the quantity of this fluid usually takes place slowly and by degrees: hence its symptoms are, for the most part, very obscure, and marked by the graver cerebral disease of which it is merely a consequence. In many cases, too, the increase in the fluid is gradual, and months or even years elapse before it attains its maximum. Neither are the cerebral functions, nor is the power of motion seriously impaired; or if such disturbance should take place, other lesions of the brain will be found to which they may be attributed. Instances, however, do occur in which a sudden accumulation of the fluid takes place, when symptoms of what has been called serous apoplexy take place, which closely resemble those produced by the injection of a large quantity of liquid into the cranium or spinal canal of animals.

Besides the increase or diminution in the quantity of the cerebro-spinal fluid, it is liable to modifications of its physical qualities owing to the admixture of blood or pus, or of cerebral matter, as is sometimes the case in softening of the brain. Of these changes that produced by the admixture of blood seems the most important; and the invariable admixture of blood with the sub-arachnoid fluid in cases where hemorrhage has taken place into the ventricles appears to be one of the strongest arguments in favour of a communication between the interior of the brain and the sub-arachnoid tissue.

The symptoms which characterize this occurrence are not very easy

to interpret, since in these cases there exist at the same time laceration and compression of the brain, coagula of blood, and blood mingled with the fluid. From a comparison of a good many cases, M. Magendie concludes that—

"1. When a partial admixture has taken place at the surface of the cerebro-spinal organ, as when the extravasated matter has completely assumed the place of the fluid, phenomena of compression exist in direct relation to the quantity of the effusion and the extent of surface compressed.

"2. When a partial admixture takes place in the ventricles, somnolence and sometimes profound sleep are observed—phenomena which likewise exist in cases of simple accumulation of fluid.

"3. Lastly, when this mixture is general, the same accidents occur, but they supervene more rapidly, like the cause which produced them, and there always exist various signs of compression, according to the quantity of the foreign matter, the points where it collects, and especially according to the greater or less rapidity of its effusion." (pp. 122-3.)

The work concludes with some interesting though confessedly incomplete observations on the cerebro-spinal fluid in the different classes of vertebrate animals. It exists in greatest abundance in the higher orders of animals, among whom the movements of the nervous system are most active, and diminishing gradually among the lower classes of animals, it finally disappears entirely in some species of fish. But, independently of its relation to the size of the nervous centres, its quantity is greatly influenced by the different modifications in the mechanism of the protecting envelopes and of the circulating system of the brain and spinal cord.

In man and the higher animals there exist an external sub-arachnoid and an internal, intra-ventricular fluid. The quantity of the former is in general greater in proportion to the fragility of the cranium, and in aged persons whose bones are very brittle this external fluid is usually very abundant. When some peculiar structural arrangement of the bones of the cranial arch provides the brain with another protection, the layer of fluid diminishes. This modification is already found in some ruminants; it is at its maximum in birds, reptiles, and fishes, in which there exist mere traces of the sub-arachnoid cerebral fluid.

The spinal cord is protected in the same manner as the brain by this fluid, which is especially abundant in situations such as the neck, where numerous and extensive movements take place. It is not, however, merely as a means of protection to the cerebro-spinal axis that this fluid deserves attention; but it fulfils an important office with reference to the circulation in those parts. All the vessels which supply the brain and spinal cord run beneath the arachnoid membrane; and we find the chief accumulations, the confluences of the sub-arachnoid fluid in the cranium around the largest vesicular trunks. Hence it follows that the circulation is carried on without interruption in the cerebral vessels, owing to the presence of the cerebro-spinal fluid, which prevents the brain from compressing them, as it must necessarily do if it exactly filled the whole of the cavity of the cranium. This fluid, moreover, by occupying the interspaces between the convolutions into which the pia mater dips, constantly keeps the two surfaces apart, and thus favours the circulation in the capillaries. In proof of this may be adduced the fact that in proportion as the cerebral convolutions disappear in different tribes of

animals the fluid on the surface of the brain diminishes in quantity; and in birds and reptiles, in which no convolutions exist, it has entirely left the surface of the organ to collect at the confluences which surround the large vessels. The spinal cord is surrounded by a layer of fluid wider in proportion than that which surrounds the brain—a provision called for by the enormous size which the vertebral sinuses are capable of attaining; since, unlike the sinuses of the brain, they are not contained in unyielding fibrous canals.

The various classes of animals exhibit another use of this fluid—namely, that of protecting the nerves from pressure up to the point of their exit from the cranium and vertebral canal. This use of it is illustrated by the fact, that while in the human subject the Gasserian ganglion is surrounded by a confluence of this fluid, there are some animals—as the horse and ox—in which it does not exist. In them, however, that portion of the dura mater which attaches the ganglion to the petrous portion of the temporal bone is ossified, and the ganglion is thus placed secure from pressure external to the cranial cavity.

Some historical notices, by a pupil of M. Magendie, of the opinions of the older anatomists concerning the fluid in the cranium, are given as an appendix to the work. The observations which we have just noticed on the cerebro-spinal fluid in animals form its only pretensions to novelty: all the other facts and opinions it contains will be found in the Journal de Physiologie, with the great advantage of not being expanded by a tedious verbosity to a bulk sufficient to occupy a quarto volume. Still the subject is one of interest and importance, and we are glad to have had the opportunity of placing a complete view of it before our readers. The consideration of it may be useful, both in a pathological and practical point of view; and to that numerous class of juvenile and senile theorists whose scanty philosophy is in the direct ratio of their knowledge, and whose feeble judgment is in the inverse ratio of their imagination, the facts and views we have recorded may prove a mine of boundless wealth.

Art. XIV.

The Climate of the South of Devon; and its Influence upon Health. By Thomas Shapter, m. d., &c.—*London*, 1842. 8vo. pp. 258.

The very admirable work, whose title we have just transcribed, and which, for its completeness, impartiality, and philosophical tone, may serve as a model for all future inquiries of the same nature, is in a great measure a reprint of papers already published in the Transactions of the Provincial Medical and Surgical Association, vols. vi, vii, and x. Such, however, is not its character throughout; many details have been omitted, others have undergone considerable condensation; the account of the geology of the district is rendered much more perfect, and there is an entirely new chapter, giving a short, but valuable sketch of the various towns and villages which are most commonly resorted to by invalids. We shall, therefore, treat it as a new book; and though, from the mass of materials, and, if we may so speak, the close-grained structure of the entire production, anything like a perfect analysis is imprac-

ticable; we shall endeavour to give a general outline of the volume, in as short a space as possible, advising such of our readers as desire further information, to procure and study the original for themselves.

The portion of the county noticed by Dr. Shapter, is the Southern district, including Exeter, and the well-known watering-places in its vicinity. Its general physiognomy is that of a succession of undulating high-grounds, with luxuriant vales, and small fertile valleys, for the most part in a state of high cultivation, and richly wooded by very lofty hedge rows; these, which are thickly studded with elm and oak trees, (the former, especially, in the vale of the Exe,) form boundaries of, generally speaking, very small inclosures. Besides the undulating high-grounds, there are hills and ridges of very considerable altitude, as Dartmoor, the mean height of which is 1782 feet, Haldon 800 feet, and Whitstone, 740 feet. The general beauty of the scenery is too well known to require notice. The *geological formation* of the district is very interesting, but can be barely touched upon within our limits. The series of rocks is extensive, ranging from the granite to the lower cretaceous group, but it is not complete in all its parts. The most important are, granite, grauwacke slate, carbonaceous rocks, schists, limestone, new red sandstone, (including *Exeter conglomerate*,) green sand, granite, green-stone, and trap-rocks. Coal, also, exists in one locality, Bovey heath, in conjunction with layers of Bovey clay. The coal is imperfectly formed, and is of little value as fuel.

The county abounds in rivers, streams, and springs. The chemical composition of the latter varies according to the geological formations in which they take their rise. Our author gives the analysis of several of each kind, for the particulars of which we must refer to the work itself (pp. 204-7); merely remarking, that where the mineral impregnations are in excess, which is particularly the case in some parts near the coast, as Torquay, it is advisable for invalids not to drink the water until it has been boiled, and thus freed of a portion of its solid ingredients, otherwise it is apt to occasion slight symptoms of a dyspeptic character. This observation, however, does not apply to those springs which are derived from the schist formation, the iron, and free carbonic acid, which many of them contain, rendering them, when drunk fresh, serviceable in cases of general debility and indigestion. The temperature of the springs is a little higher than the mean temperature of the climate.

The *flora* of South Devon is particularly rich; and the number of delicate exotics (see a list, p. 39) which flourish in the open air, and are not destroyed by exposure during the winter season, is of itself no mean proof of the mildness of the climate; to which subject we shall now direct our attention.

The most striking characteristic of the *climate* of Devon, generally, is that of being warm and moist; this is partly owing to its latitude, but also in a great measure to its position as regards the ocean, since it forms a portion of a large promontory, or imperfect peninsula, projecting westward into the Atlantic, and nearly half its circumference is sea coast. The mean annual temperature of South Devon, deduced from careful observations made at Exeter, is 51° 29′; this is nearly one degree higher than that of London, which is stated by Sir James Clark to be 50° 39′, though this is probably too high.

But it is not so much the actual warmth, as the equability of temperature, which characterizes the two south-western counties of England. The difference between the warmest and coldest of 10 years (from 1825 to 1834, both inclusive,) amounted, in South Devon, to no more than 4° (5° ?), and the mean difference, in succeeding years only to 1½°. The same relative superiority of equable temperature obtains, likewise, in the various months and seasons of each year, as is clearly manifested by Dr. Shapter's Tables, (p. 6,) in which the results of observation at Exeter are contrasted with records from the metropolis.

There is a curious circumstance connected with this subject, which we must notice, viz., that while the mean difference of temperature between day and night, for the whole year, amounts in this district to 30°, it is stated, by Luke Howard, not to exceed 14½° in London and the vicinity. This discrepancy obtains, likewise, in the mean diurnal variation which takes place during the several months and seasons. Dr. Shapter thinks the explanation of this singular anomaly must be sought, not only in the great quantity of heat artificially generated in London, "but also in the dense mass of houses radiating during the night the excess of temperature acquired during the day; the surrounding thick atmosphere constituting a medium which prevents this taking place as rapidly as if the sky were clear, and assisted by a free ventilation."

In South Devon, January is the coldest month; the temperature then progressively increases until July, when its mean height is 63°·64: after this it declines again to its minimum average 39°. The skies are, for the most part, disposed to be clouded and overcast, especially in January, December, November, February, March, and October; the above order indicating the relative proportion of shade proper to each month. April and May are the brightest and most sunshiny; July and August have only a moderate proportion.

The *hygrometrical constitution of the atmosphere* is a subject of much importance in the history of a climate, for slight variations in moisture are well known to exert a most powerful effect upon many forms of disease; and our author has, therefore, done wisely in devoting some space to its consideration. The only way in which the true condition of atmospheric moisture can be appreciated is by noting the difference between the temperature of the atmosphere, and the temperature at which dew is deposited; for when both are nearly the same, the climate is moist, and when they are widely separated, the climate is dry. Now, the results of a daily observation made in this way, at 9 A.M., for the space of five years (from 1832 to 1836 inclusive,) fully prove that the proverbial moisture of Devon is not imaginary; the mean temperature of the dew point being about 46° (45·9), while the temperature of the atmosphere itself is 53°, so that there is only a mean difference of four degrees and a half (4·4) between them. The winter season is the most damp, the differences of temperature amounting to but two degrees and a third (2·3); nor is the autumn much superior in this respect, the difference being very little more than three degrees (3·1.) The summer and spring are comparatively dry seasons, the mean difference amounting in the former to nearly eight degrees (7·9), and in the latter to nearly six (5·9). November is the dampest month, and July the dryest.

Common report gives Devon the character of being a particularly rainy county; but this opinion admits of some qualification. The annual fall of rain, at Exeter, amounts to very nearly 32 inches (31·90), being about 7 inches more than in London. But, notwithstanding this, the number of wet days is not so great, the average for Exeter being 162·4, while in London it is 178. By a *wet day* is understood one on which a fall of rain, however slight, takes place. It should also be observed that some places on the coast are both less liable to rain and fog, and have generally a dryer atmosphere than Exeter and other parts of the district. This is especially the case with Torquay, Exmouth, and Budleigh Salterton (vide pp. 142, 153.)

Frost is not uncommon in this district, but the atmosphere very rarely maintains, for any length of time, a temperature below the freezing point. *Snow* seldom falls in any great quantity, or remains upon the ground above two or three days. The fall nearly always takes place in January and February. *Hail* is a little more frequent than snow. In ten years the number of days in which it fell amounted to 71. It is most prevalent in April and December. *Thunder* and *lightning* are, comparatively, unfrequent, and the storms are very rarely attended with serious consequences.

The most prevailing *winds* of this district are the w. and n. w. They occur chiefly in June, November, February, December, and March. The e. and s. e. prevail most in May, July, and October. s. and s. w. in September, and n. and n. e. in January. Taking the average of the whole year, the s. and s. w. are attended by the highest temperature, and the n. and n. e. by the lowest: but this does not apply to individual months, for in June the n. and n. e. are the warmest winds, and the same is also true of the e. and s. e. in May. During the winter season the s. w. wind is often accompanied by a warm thick mist, which is peculiarly relaxing, and from its frequency is not unaptly styled Devonshire weather. A similar phenomenon occasionally attends the s. e. wind during summer. We must refer to the work itself for a number of tables illustrating these various points (pp. 31, 32.)

Upon the whole, then, it would appear "that the chief characteristic of the climate of this district is that of being warm, soft, mild, equable, calm, and free from storms; though subject to a large share of rain, yet it seldom occurs that a whole day is so unceasingly wet, as not to afford some hours for out-door exercise..... The air is usually damp, but from the general prevalence of warm westerly winds, the moisture which it contains is not cold and chilling."

The most important part of Dr. Shapter's volume is that in which the effects of the climate upon the constitutions of its inhabitants are illustrated. The data, from which the results are deduced, are the cases treated, during the same ten years over which the former observations extend, at the Exeter Dispensary: and amounting as they do to no less a number than 11,258, (4535 males and 6723 females,) the inferences deduced are worthy of all confidence.

We would gladly transfer to our pages one of the tables given by our author, (No. iii,) showing the relative frequency of various diseases in each month, but could not do so without occupying more space than can well be spared, and must therefore content ourselves with a short ab-

stract of the results. From this table it appears, that the more sickly period of the year extends from February to July, but that the greatest amount of sickness prevails during April and May: and the climate of these two months is characterized by some peculiarities which are worthy of notice. It is between them that the rise in temperature is most considerable, amounting to more than one half of the whole difference between spring and summer; the difference between the diurnal maximum and minimum is also more marked than in any of the other months, amounting in April to nearly 39°, and in May to 34½°, 30° being the mean during the year. The mean barometric height is lower than in either of the two months, which immediately precede or succeed them; the most prevalent winds are the N. N. E., and S. E., and the number of sunshiny days is greater than during other parts of the year. This sickly period is, therefore, characterized by a sudden increase of temperature, by warm days and cold nights, by a dry atmosphere, and seductive sunshine; and the diseases which make up the increase are, in April, fever, rheumatism, dropsy, diseases of the brain, bronchitis and pneumonia, dyspepsia, gastritis, and diseases peculiar to females;—in May, eruptive fevers, scrofula, diseases of the brain and heart, bronchitis, dyspepsia, and eruptive complaints. It should be observed, however, that the mortality in these same months is rather below the average, as we shall see presently.

The period of the year in which there is the least amount of disease extends from August to January, September and October being the most healthy months. These two months are characterized by equability of temperature, the mean diurnal difference being no more than 29°. The prevailing winds are from the west; the air is moist when the temperature is high, and dry when it is low; and there is a good deal of rain. This bears out the statement made above, that the thick, warm, and commonly-styled unwholesome weather, is in reality conducive to health in this district. The diseases which more particularly diminish in frequency during this period are dropsy, diseases of the chest and brain, those peculiar to females, and rheumatism; fever, on the contrary, begins to increase.

Fever constitutes 12 per cent. of the whole amount of disease in the 11,258 cases observed. This average, though large in itself, is lower than that found in other great towns, as London, Edinburgh, Glasgow, &c. Its frequency, of course, varies in different years. The greatest number of cases, during the ten years, occurred in 1827, but there was nothing peculiar in the climate to account for this increase. February appears to be the month most prone to fever, and the fewest cases occur in August and September. The average age of the patients was 31; this is below the average given by Dr. Craigie,* which was 45, and above that recorded by Dr. Tweedie,† who observed the greatest number of cases between 20 and 25. The most common types are simple *synochus* and *typhus*. In most cases some degree of local inflammation has been noticed with the general fever; the adynamic typhus being most frequently complicated with affections of the mucous membrane of the stomach and bowels, then of the viscera of the chest, and more rarely of the brain, (this latter assertion is only true of the early stages.) In the other, and

* Edinb. Med. and Surg. Journal, vol. xlvi. † Cyclop. of Pract. Med., vol. ii. p. 189.

more simple fevers of the district, the chest is most frequently attacked, then the abdomen, and then the brain. It would not appear that these diseases are accompanied by a high degree of mortality. Dr. Shapter inclines to the opinion that fever sometimes originates tuberculous deposits, in patients who are predisposed to them (p. 59). The peculiar eruptions of fever are not, generally speaking, of frequent occurrence, though at times they prevail extensively, the most usual form being that of petechiæ. In 1836 a peculiar form of *spotted fever* was epidemic. The spots appeared within the first twenty-four hours. They were of a brownish red, sometimes passing into a purplish colour, larger than petechiæ, and gradually shaded off into the deepened colour of the surrounding integuments. Some of them passed into vesicles, and they generally terminated by desquamation of the cuticle. The fever was of an adynamic character, and was most successfully treated with salines and aromatics. In 1839 and 1840, fever assumed a hemorrhagic tendency, and proved fatal in many cases, by bloody discharges from the bowels. Twice or three times in the last ten years the external mucous surfaces in children, during a scarcely appreciable attack of fever, showed a great liability to take on inflammation of a bad character, attended by copious muco-puriform discharges. In 1834 this was peculiarly the case with the female organs of generation, and in an ignorance of the circumstances suspicion might have been excited that disease had been communicated, attended with violence.

Intermittent fever is of very rare occurrence. The ordinary *eruptive* fevers occur in this district in about their usual proportion, generally assuming an epidemic character. With reference to these we have little to notice, excepting that in cases of *malignant scarlatina*, in which all other plans of treatment entirely failed, Dr. Shapter has obtained the most signal benefit from the free exhibition of ammonia, according to Dr. Peart's method. "Not only," he says, "were present symptoms relieved, and danger almost invariably arrested by its use, but I observed that the patients stepped, as it were, immediately from the sick bed into health; there was nothing like a protracted convalescence," and disagreeable sequelæ were far from frequent.

Rheumatism, *lumbago*, *gout*, &c., though not infrequent disorders, are by no means so prevalent as in many other parts of the kingdom—the number of cases forming about 4 per cent., or 1 in 24 of the whole registered diseases; while in Penzance they constitute 1 in 17·6; in Plymouth, 1 in 18·4; in London, 1 in 14·7; and in the north of England, 1 in 22. The cases which occur consist chiefly of subacute rheumatism, rheumatic gout, and more especially chronic rheumatism. Acute rheumatism is only occasionally met with. These various forms are most prevalent in March, April, May, and June.

Dropsy is more frequent in this district than in many other places, forming $3\frac{1}{2}$ per cent., or 1 in 28 of the whole diseases. How far this may depend upon the moisture of the climate, is a question we do not feel ourselves competent to answer.

Diabetes mellitus and *insipidus* are by no means uncommon. They occur chiefly in males. Every case of true diabetes which has come under Dr. Shapter's notice, terminated in tubercular phthisis. "This has been so uniformly the case," he says, "that the conviction forces itself

upon me of its being essentially a scrofulous disease, and that the kidney is made to be an emunctory of those matters otherwise colliquatively discharged by the skin."

Scrofula is more frequent than in the midland counties; it constitutes nearly 2 per cent. of the whole number of cases. It appears in all its various forms. Mr. James, of Exeter, has treated *noli me tangere* with the most complete success, by a compound of the chloride of zinc (1 part) with sulphate of lime (3 parts). This is moistened, and applied on lint over a small portion of the diseased surface; after about five days it falls off, exposing a healthy-looking sore, which readily heals on the application of simple dressing. Other portions are then similarly treated, until the cure is complete.

Scirrhus occurs in about the usual average, i. e. about 1 in 244.

We pass over the diseases of the brain and nerves, and also those of the heart and great vessels, which present nothing to detain us, and proceed to consider—*Diseases of the lungs*, which form 17 per cent. or 1 in 6 of the whole amount. Of these a large proportion consists of *simple bronchitis*, which is most prevalent during the winter and spring months. *Chronic catarrh* is not uncommon among the lower orders, but is comparatively rare among the upper classes. *Influenza* has been epidemic several times, and proved, as in other places, very fatal. *Asthma* is rarely met with. *Croup* is at times frequent, generally making its appearance as an epidemic. *Laryngismus stridulus* also occasionally occurs; and so does *hooping-cough*, but only as an epidemic. *Consumption* forms a large proportion (4 per cent.) of the diseases of the chest in this district; it occurs in all its various forms, though, most usually, the cases are protracted and lingering. *Pneumonia* is by no means frequent. *Pleuritis* is more common. *Diseases of the abdomen* amount to $14\frac{1}{4}$ per cent. of the whole. *Dysentery* is rare: *diarrhœa* is very prevalent, especially among children. *Acute gastritis* seldom occurs; the *chronic* form is more frequent. *Peritonitis* was very common in 1827. The *lead*, or as it has been called, the *Devonshire colic*, has disappeared (excepting among painters, &c.), with the removal of its cause, viz., the employment of leaden vessels in the making of cider. *Dyspepsia* prevails to a considerable extent, but presents no remarkable features. The largest proportion of cases takes place in May. *Functional disorders of the liver* are not infrequent; but what is generally understood by the term "*liver disease*," is by no means common. Diseases of the *urinary organs* are not frequent. *Uterine diseases* occur in about the ordinary proportion. *Eruptive diseases* form a considerable proportion of the disorders of the district, about 1 in 14. The most common are scabies, herpes, impetigo, psoriasis, lepra, eczema, prurigo, and acne.

Having given a summary of the indigenous diseases our author proceeds, in the next chapter, to specify those affections in which a change to the climate of South Devon appears beneficial. Here we shall follow him, with all possible brevity. Much advantage is frequently derived from a residence in this climate, at those periods of life when the system undergoes changes, which, without the presence of any active disease, often give rise to so much weakness and depression as to cause great anxiety in the minds both of the patient and his friends. This is especially the case in early youth, when the growth of the body is disproportioned

to the strength,—at the commencement of decay, when various symptoms arise, to which the name of *climateric disease* has been applied,—and in the general failing of advanced years. In all these cases, a change to this locality is often marked by a speedy alleviation of distress. It is also to be particularly recommended in those cases where a tendency to scrofulous disease is manifested, but not to the exclusion of other dietetic and medicinal preventive means, as is so frequently supposed by parents and patients.

As a residence for consumptive patients the southern parts of Devonshire have long been celebrated. Dr. Shapter speaks on this subject with the reserve of a cautious and experienced physician. "This district," he says, " possesses many of the qualifications which are usually thought requisite for counteracting the consumptive tendency, such as, its contiguity to the sea ; its protective amphitheatre of hills, forming an adequate barrier against the north and east winds ; together with an atmosphere soft, warm, and charged with aqueous moisture, a condition eminently useful towards alleviating irritation of the lungs." (p. 122.) Torquay and Dawlish are the places most recommended. Of the precise character of the climate of the former place Dr. Shapter speaks doubtingly, notwithstanding the numerous observations on its temperature, &c., that have been put on record. Dr. S. thinks these observations have been made "at such irregular times, and for so short a period at each time, that it is out of the question deducing from them anything like satisfactory averages." There can be little doubt, however, that of all the places on the south-western coast, Torquay affords the driest and most sheltered position during the winter months.

The climate of South Devon is also calculated to give much relief in *bronchitic affections ;* in *dyspepsia*, unless associated with, or depending upon, general debility ; in *gout* and *rheumatism ;* in *dysentery*, and in *cutaneous affections.* It is, moreover, very useful in that form of *amenorrhœa* which is characterized by the bloodless cheek and lip, shortness of breath, nervous palpitation, quick small pulse, pain referred to a spot beneath the left breast, and general tendency to constipation.

The next four chapters contain accounts of the different watering-places, the geology and natural productions of the district (already considered by us), and the civil and economical history of the inhabitants ; and the last, and certainly not the least important section of the work, gives the vital statistics of Exeter and the immediate neighbourhood.

Of this last division of the work we regret that our limits will not allow us to give any account ; but we can safely recommend it as a model for those engaged in similar inquiries. The data, of the most authentic kind, have been weighed with the most scrupulous accuracy, and the results, therefore, may be thoroughly depended on.

In concluding this very imperfect sketch of Dr. Shapter's valuable and interesting work, we must once more recommend it to the attention of our readers. Independently of the important information of various kinds contained in it, it may safely be studied as a model for those who are desirous of pursuing a similar line of inquiry, and who wish to see the medical topography of a district treated with that singleness of purpose, and philosophical candour, which should characterize the writings of every member of a liberal profession.

Art. XV.

1. *Allgemeine Anatomie. Lehre von den Mischungs-und Formbestandtheilen des menschlichen Körpers.* Von J. Henle.—*Leipzig*, 1841. 8vo, pp. 1032. Mit funf Tafeln, &c.

General Anatomy. The doctrine of the elementary composition and structure of the Human Body. By J. Henle.—*Leipzic*, 1841. 8vo, pp. 1032. With Five Plates, &c.

2. *Handbuch der allgemeinen Anatomie des Menschen und der Haussäugethiere.* Von Fr. Gerber, Professor der Thierheilkunde, &c. in Bern.—*Bern*, 1840. 8vo, pp. 213. Mit 7 Steindrucktafeln in Folio.

Manual of the General Anatomy of Man and the Domestic Mammalia. By Fr. Gerber, Professor of Veterinary Medicine, &c. at Bern.—*Bern*, 1840. 8vo, pp. 213. With 7 Lithographic Plates in Folio.

3. *Elements of the General and Minute Anatomy of Man and the Mammalia.* By Fr. Gerber. *To which are added Notes and an Appendix, by* George Gulliver, f.r.s.—*London*, 1842. 8vo, pp. 390-106. With an Atlas, containing 34 Plates.—*London*, 1842. pp. 71.

4. *Lehrbuch der allgemeinen Anatomie des Menschen.* Von Victor Bruns, Doctor der Medicin und Chirurgie, &c.—*Braunschweig*, 1841. 8vo, pp. 398.

Elements of the General Anatomy of Man. By Victor Bruns, Doctor of Medicine and Surgery &c.—*Brunswick*, 1841.

5. *Die Mikroskopischen Forschungen im Gebiete der menschlichen Physiologie, dargestellt von* Otto Köstlin.—*Stuttgart*, 1840. 8vo, pp. 304.

The Microscopic Researches in the department of Human Physiology, set forth by Otto Köstlin.—*Stuttgart*, 1840.

6. *Traité pratique de Microscope et de son emploi dans l'étude des corps organisés.* Par le Docteur L. Mandl; *suivi de Recherches sur l'Organisation des Animaux Infusoires.* Par Dr. D. C. G. Ehrenberg. *Paris*, 1839. 8vo, pp. 486. Avec quatorze Planches.

Practical Treatise on the Microscope, and its employment in the study of organized bodies. By Dr. L. Mandl; *followed by Researches on the Organization of Infusory Animals.* By D. C. G. Ehrenberg.—*Paris*, 1839. With Fourteen Plates.

7. *Anatomie Microscopique.* Par le Docteur L. Mandl. Parts I-V. —*Paris*, 1838-9. Fol. maj.

Microscopic Anatomy. By Dr. L. Mandl. Parts I-V.—*Paris*, 1838-9.

8. *Entwurf eines neuen genetischen Systems der Histologie, zugleich als Grundriss einer philosophischen Anatomie.* Von Dr. Hermann Klencke.—*Leipzig*, 1841. 12mo, pp. 230.

Attempt at a new genetic system of Histology, for a basis to a philosophical Anatomy. By Dr. Hermann Klencke.—*Leipzic*, 1841.

9. *On the Employment of the Microscope in Medical Studies. A Lecture introductory to a course of Histiology.* By John Hughes Bennett, m.d.—*Edinburgh*, 1841. 8vo, pp. 27.

I. The report on minute anatomy which we lately published, renders it unnecessary to analyse the contents of the works whose titles are pre-

fixed. Our present intention is to speak briefly of the characteristics of each of them, and to offer some remarks on the history of their common subject, and on the spirit and method in which it is now investigated.

1. The work of Professor Henle is a complete system of general anatomy, and such an one as probably no other man in Europe could have written. Its author is already known to our readers as one of the most sharp-sighted and industrious anatomists of Germany; as the most successful investigator of the nature of mucus, and of the structure and arrangement of the epithelia, the hairs, the blood-vessels, the villi, and the true elementary parts of the secernent glands. By his discoveries in these and many other particular objects of inquiry, he has contributed to the knowledge of physiology scarcely less than his fellow-pupil, Schwann, has by his great generalization of development through cells. For the present work Henle seems to have laboured in the whole field of structural anatomy both by actual investigation and by reading; and he has thus produced a book which is not only superior to all others on the same subject, but has few equals in merit among the text-books of other sciences. It is written honestly, accurately, and with common sense, and, though far too large for the use of students, it would be hard to say what part could be omitted without lessening its value to those who are already acquainted with the general principles of the science.

Of its two chief sections the first, on the chemistry of the body, is compiled from the works of Berzelius, Löwig, and F. Simon; the second, which forms nine tenths of the whole, is devoted to structural anatomy. There is in it a happy combination without confusion of the facts of observation with those of history. In each chapter, a tissue is first described with no more references than are absolutely necessary, and this is followed by the history of the progressive discoveries in its anatomy, and an analysis of the opinions of different authors on questions yet unsettled. In all this historical part, Henle's accuracy and impartiality are truly admirable: we have scrupulously compared page after page with extracts from the quoted works and have hardly discerned an error.

2. The value of Dr. Gerber's Manual, which at its first appearance was the best of the modern works on general anatomy, has been greatly depreciated by the publication of those of Henle and Bruns. Not that it was admirable even when almost unrivalled; for its style is obscure, and the descriptions of all the tissues are confused or imperfect. It contains, indeed, many original facts from the investigations of its author and his friend Valentin; and these give it considerable value. But its general usefulness is prevented by its faults, not only of style, but of arrangement; the author seeming to have mistaken minuteness of division for accuracy of plan.

Of the translation the best that can be said is that it holds in English literature the same place which the original work once did abroad; it is unrivalled; it is executed with a fair amount of accuracy, but the writer seems often to have stuck half-way between the languages, and the translation of many words has to be carried some distance further before they are fairly rendered into English. The plates are, in both, hard and unnatural: they are, indeed, little more than diagrams of the objects they are in-

tended to represent. Much value is added to the translation by Mr. Gulliver's notes and appendix, in which he has embodied the results of his laborious investigations of the blood-globules, the chyle, lymph, &c.

3. The characters of Bruns' work are equally different from those of both the preceding. It contains but little original matter, but its descriptions are clear, brief, and, according to the generally received views, complete; no extraneous questions are introduced, and the simple tale is told as fluently as if it were all true and fit to be applied to any extent in practice. By all these qualities it is as well adapted for the use of the student as Henle's work is for that of the professor, or of one who is already acquainted with all that Bruns could tell him.

4. Otto Köstlin's book is a compilation of abstracts of all that, at the time of its publication, had been learnt by the use of the microscope, and merits nearly all the praise that can be given to success in such an undertaking; for it is complete and accurate. There are very few parts in which the author has either omitted or misunderstood the labours of those who have worked on the subject. Its defects are the attempts to graft ideal physiology upon substantial anatomy. It is hard to conceive how a man, having learnt so many facts, could believe that there is anything but nonsense in the notions which he tries to make them illustrate. If he had wished to prove how little connexion there is between the two things, he could not have succeeded better than by thus merely putting them side by side.

5. The treatise of Dr. Mandl is the best which has been produced in France upon microscopic anatomy. But it is inferior to all the preceding. It contains a well-written history of the microscope; but the few pages which follow on some of the tissues are imperfect, and, in the present state of microscopic science, valueless. The whole style of the work is too light for a subject which, at present, consists of little more than facts. The value of the researches of Ehrenberg is well known: but Dr. Mandl has been unwise to add them to his work, for they not only have no natural connexion with what precedes them, but, by their completeness, they render the imperfections of the rest more obvious.

6. The Anatomie Microscopique of the same author is scarcely more than an enlarged and better illustrated edition of the treatise. It includes only the tissues which Dr. Mandl has particularly studied; but his investigations fall far short of the accuracy and completeness of the German anatomists. The plates have the faults common to so many of those intended to represent microscopic objects; in the attempt to make them distinct, they are made stiff, diagram-like, and too definite. Nothing but the rare association of an artist who is an anatomist, with an engraver of the first degree of excellence, can ensure an accurate representation of nature in works of this class; and in none of those before us has such a combination of talents been employed. Indeed we know not where they are to be found in the service of general anatomy, except in the works of Berres and Wagner.

7. The title of Dr. Klencke's work at once reveals something of its character. It is one of those confident attempts which are commonly made in Germany to establish an imaginative system, in the notion that it is the offspring of the pure reason. The author says of it in another

book,* that Carus has honoured it by the assertion that, "by this work the histotomy of former days has been for the first time carried out to a real histology." Our readers may judge how far it has effected an advance in science by the following extracts, which it is necessary to give as an example of the style to which we shall have again to refer. As one of the principles on which physiology is to be placed on an imperturbable basis he sets down this;

"Since every formation is the plastic expression of the vital-idea working in it, and this idea is, in every organism, a focal point of the general universal-idea, and this universal-idea is the very Godhead impressed in nature; it follows that the world and all forming existence must go on with the completest reason and adaptation to purpose; and there must be manifested in every phenomenon a thinking which points back to reason. There lives, then, in all nature a thinking without consciousness, a dreaming intelligence, which is alluded to by natural philosophers as a *nisus formativus*, instinct, &c. But in man this thinking bursts into consciousness; and the same idea which is formative in his organism, and, without being the subject of consciousness, unfolds the living members from the germ, itself becomes perceptible; and now, since in itself it represents the universe, it reflects as from a mirror, in the light of the conscious reason, all nature and the eternal foundation-thought of the macrocosm; and it inflames spiritually the general intelligence in the realm of spirits. Consciousness, and unconscious organizing are therefore *one* undivided and idealized principle." (p. 15.)

In other words, and this might be the first article in the creed of this sect, "thinking is knowledge." Another passage will show the kind of knowledge attained by this employment of the pure reason in questions in the solution of which the understanding has its proper work.

"What," asks the author, "is this seemingly watery substance with which all the tissues are moistened?—and he answers in the words of his masters, Carus, who calls it the 'parenchymatous primary formative fluid,' and Oken, who says, 'It is the true nervous substance.' With the nervous matter has the animal substance commenced; with that which is the highest, and which physiologists have regarded as the last. The origin of the animal is from nerve, and all anatomical systems are only unwindings or expansions from the nervous mass. The animal is nothing but nerve; what it is further is nerve-metamorphosis. The mucus of the medusæ and polyps is nerve-substance at the lowest stage, in which the inherent and confused substances are not yet developed in isolation. The nervous matter is the absolute Indifferent in the animal; that which by the lightest breath, even by a thought, is polarizable." (Klencke, p. 44.)

8. Dr. Bennett's Introductory Lecture is an unconditional, and therefore an unwise, panegyric on the microscope. All that is stated on microscopic evidence is advanced as if it were certain truth; yet in the lapse of a year doubt has been cast on a great moiety of the *facts* which it contains. It includes, however, some interesting cases, in which the microscope afforded assistance in the diagnosis of disease.

II. Having said thus much of these works, of which the mere existence is a remarkable evidence of the advance and progress of the anatomy of structure, let us now examine briefly the stages through which it has passed in its course hitherward; an inquiry which, as far as we know, has not hitherto been made, and which, by directing it chiefly towards that part

* Neue Anat. und Phys. Untersuchungen über die Primitivnervenfasern und das Wesen der Innervation, p. xl.

of anatomy which is studied by the microscope, may serve to illustrate some of the faults of the spirit in which it is pursued.

The beginning of the study of structural anatomy was probably coincident with the first strict inquiries into the form and nature of the human body. As soon as men began to examine how the parts of their frame were put together, they would naturally investigate also the structure of each part; and, with scarcely an exception, we find that, to the time of Bichat, the anatomy of structure and that of construction were treated as one science. The latter made, as may be supposed, by far the more striking and rapid progress; and it is not possible to trace any stages of discovery in structural anatomy till the time of Malpighi, who first, about the year 1660, employed magnifying glasses in the study. The first fruits of microscopic research were rich and abundant. Not to mention Malpighi's discoveries in vegetable physiology; the discovery of corpuscles in the blood, and of the capillary circulation, the main fact of the cellular structure of the lungs, and the principles of the formation of secernent glands, the import of the sensitive papillæ, the lobular and cellular structure of the fat, the mode of the growth of hair, as much as can be believed of the structure of the spleen, the more important part of the anatomy of the kidney, the threefold constituents of the teeth, and many facts of less interest were all fully established by him, and to all the microscope was an important, if not an essential, aid. It is remarkable that Malpighi's were almost the only facts discerned in the investigations of the first microscopic anatomists which stood the test of time. Though often denied, yet scarcely one of them has been even temporarily obscured; they were at once established as truths, or as very probable opinions; and no change of doctrine could remove them from the accepted records of medical science. The explanation of so striking a fact is obviously this; Malpighi reasoned rightly, or but little on his facts, and, possessing a thorough knowledge of the physiology of his day, he was able at once to point out many of the relations which his discoveries bore in the explanation of processes in which all were interested. In this he was widely distinguished from those who immediately followed him in similar investigations, and of whom scarcely one knew more of physiology than of many other sciences. It is, indeed, difficult to speak too highly of the merits of Malpighi. He was in every respect a great physiologist; and in the history of microscopic anatomy he stands pre-eminently among the few who have combined acuteness in minute researches with clearness of reasoning upon the results which he obtained.

After Malpighi, the next great name in the history of minute anatomy is Leeuwenhoeck; whose earliest investigations appeared in 1673, in a communication made to our Royal Society through Regner de Graaf. There is a striking contrast between the two; Malpighi, we have said, was a genuine and accomplished physiologist; Leeuwenhoeck was an unlettered mere microscopist; the one used his microscope with a truly philosophic view, the other's microscope may be said rather to have used him. Leeuwenhoeck looked at everything that was small enough to escape the eyes of others; without order, and as if without purpose, he examined objects from every source. His instruments and his eye were, however, alike marvellously accurate; and employing them with extra-

ordinary industry, he was enabled to perfect and discover more facts than perhaps any other anatomist of any time, so that it is still hard to find any thing which he has not discerned, and it is unwise of any man to appropriate a discovery with the microscope until he has assured himself that Leeuwenhoeck has not a prior claim. No man of his day would have been greater, had he been able to make a scientific use of all the facts that he collected; but unhappily, when he left the regions of sense he almost always went into those of error; his imagination was apt to see in every wonder something still more wonderful, in every diversity some strange and false analogy.

Among Leeuwenhoeck's discoveries in minute anatomy may be enumerated those of the bone and cartilage corpuscles, the spermatic animalcules, the tubules of dentine, and the fibres of enamel, the scales of the cuticle, and of the coarser epithelia, the chyle and milk corpuscles, the muscular fibrillæ, and the transverse striæ of their fasciculi; the tendinous and nervous fibres, and the fibrous and laminar structure of the lens and corneæ. These, and numerous other structures were seen by him, and, with few exceptions, all were described with singular accuracy. But nearly all were misapprehended; and hence it is that though Leeuwenhoeck's discoveries were ten times more numerous, and many of them were not less important than Malpighi's, yet his good influence upon science was comparatively nothing. Indeed, in later times, his frivolous and false interpretations of the structures which he saw tended to bring the microscope into disrepute; and while all Malpighi's observations had become a part of the real property of science, Leeuwenhoeck's remained unproductive for 150 years, and were rarely noticed except that the deductions from them might be laughed at. For it was not by their discoverer alone that all these facts of structure were misinterpreted. He had the misfortune to live in and before the last great period of system-making in medicine; and some of his facts, as pliant as they were striking, were admirable materials on which to found hypotheses. To what volumes of fancy did his mulberry-like blood-globules, and his globules of various orders give some semblance of truth!—All pathology, in the Boerhaavian doctrine of "error loci," seemed for a time to rest on a few of his facts, and when that pathology was proved false, the foundation was hidden beneath the fallen superstructure.

The investigations of both Malpighi and Leeuwenhoeck were made with simple lenses, and they afford a high testimony to their accuracy to the furthest limit of the field in which they can be used. Robert Hooke, the author of the "Micrographia, or physiological descriptions of minute bodies made by magnifying glasses," and the contemporary of Leeuwenhoeck, seems to have been the first who used, in the study of anatomy, the compound microscope, which about that time was invented by Jansen, or by some one even the name of whom has been forgotten. Hooke was a man of strange eccentric mind; one who spent more time in thinking than in observing, and was therefore little able to cope in discovery with the active Leeuwenhoeck. He made, however, many good observations; among them the first of the ultimate structure of muscle, which he seems to have published a year before Leeuwenhoeck.

Another contemporary, to whom minute anatomy is indebted, was John Swammerdam. Devoted to natural history, and a truly scientific

naturalist, he studied structures, like Malpighi, only as the means to the knowledge of processes. His minute investigations of the anatomy of insects, to which he had been led by a taste inherited from his father, a thorough Dutch collector, was a good preliminary exercise to his more widely extended researches. At Leyden and at Paris he was the best minute dissector of his day; and his ingenuity constantly supplied him with aid when his hands and eyes were insufficient. He supplied many facts of structural anatomy, but he himself threw them into obscurity by the brilliancy of his invention of the arts of making dry inflated preparations, and of permanently injecting blood-vessels, which, with the constant assistance of Van Horne, he perfected about the year 1666.

One cannot now adequately conceive the value of such a boon as this. What weariness and labour did it spare to those who before had been content to dissect vessels filled, if at all, with air or liquids that oozed through every puncture! No wonder that the first structure whose vascularity was thus demonstrated, was called "a miracle of nature."* The beauty, as well as the importance of the structures which are thus exhibited and made permanent, was at once, as it has been ever since, a constant excitement to industry in the employment of the art. Ruysch, though he does not confess it, learned the method from Swammerdam, and he soon so far outstripped his master, that, among injectors, none has ever had so high a reputation; though, in truth, his preparations cannot bear comparison with those of Lieberkuhn, Prochaska, Barth, Sömmering, or many of our own time.

Nehemiah Grew, who lived at nearly the same time, was another who avoided the merely microscopic use of the microscope; and, had he given as much attention to animal as he did to vegetable anatomy, he must have been one of the greatest contributors to our science. As it was, we owe to him the first hints of the structure and arrangement of the perspiratory glands,† some accurate notions of the structure of the lens, and a very clear account of the intestines, in which he describes the Peyer's bodies;‡ of these last, indeed, he must be regarded as the discoverer, since his account of them was read to the Royal Society in 1676, and Peyer's inferior description of them was not published till 1677.

But before the close of the seventeenth century much of the fashion of the microscope seems to have passed away, and in the eighteenth, though anatomy was diligently cultivated, we find the facts yielded to it by the microscope scattered at very distant periods. Much of this decline is due, no doubt, to the difficulty of obtaining good instruments; for the observer had, in general, to make his own, and much of Leeuwenhoeck's success was certainly due to the superior care and dexterity with which he formed and polished his lenses; for in this, as well as in using them, he was unrivalled. But much more, probably, was due to the impossibility of making the results of microscopic researches square, as they were interpreted, with the current physiological laws and hypotheses; for by these the error of some could be detected, and suspicion could be cast on all. We say "as they were interpreted;" because, as mere facts, Leeuwenhoeck's descriptions were, through nearly all his life, notorious and popular; the number of his papers inserted in the Philosophical Trans-

* "Miraculum naturæ, seu uteri muliebris fabrica." † Phil. Trans. No. 159.

‡ The Comparative Anatomy of the Guts.—London, 1681.

actions between 1673 and 1723 sufficiently proves their notoriety; and the exaggerated praise, in which they were compared to Delphic oracles, establishes their popularity; nothing, therefore, but misinterpretation of them could make them seem valueless.

The history of minute anatomy after the death of Leeuwenhoeck may be traced in the works of a very few observers, and these not the most renowned of their day; for as contemporaries of Ruysch, Albinus, Valsalva, Morgagni, Winslow, Lieutaud, Portal, Cowper, Cheselden, and Haller, those whom we have now to mention were certainly of inferior renown.

One of the first was Wier Muys, the author of a ponderous treatise on the Muscles;* a dull man, with a great love of divisions in threes, whose discoveries bore no proportion to his researches. He seems, however, to have had a clear view of the structure of muscle, and to have been the first to examine closely the tissue of the blood-vessels, and the inter-muscular fibrils. He made also several examinations of the blood-corpuscles; but, on the whole, he contributed little to the advance of science.

One of his contemporaries, J. N. Lieberkuhn, was a man of fewer words and more facts. He was the inventor of the plan of corroding preparations, about the year 1748, and as a minute injector was never equalled, unless it were by Soemmering, to whose preparations, examined in comparison with many by Lieberkuhn, Barth, and others, Prochaska† has awarded the highest praise. To Lieberkuhn we owe the first good account of the anatomy of the villi, and of the minute tubular glands of the small intestine, which still bear his name,‡ as well as of the arrangement of the blood-vessels supplying and the epithelium covering the villi.

At this time lived Ledermüller. He also looked at everything that was small; but after Leeuwenhoeck he could make few discoveries. He published numerous observations on the seminal animalcules, and there was a dispute between him and Dr. Asche, who maintained that they were mere cells, and had no tails. Recent investigations have made it probable that each had truth on his side, and that they only saw the same bodies in different stages of development. But we do not find that Ledermüller added much to the facts of science. He was content that his highest ambition should be to give others all that he had in great labour pursued, namely, "microscopic amusements for the mind and eyes," for so he called his last and longest work.§

We must not pass over Stephen Hales, the "vir pius et solers, nemini que invisus," as Haller called him, who lived at this time, and whose microscopic observations, though few, were good, and were carried on in such a truly physiological spirit, as might have made many of his medical contemporaries blush. In relation to general anatomy, Hales|| contributed some of the earliest and best experiments of the dyeing of bones by madder, observations on the blood-corpuscles, and on the velocity of the

* Musculorum artificiosa fabrica. Lugd. Bat. 1751. The first part appeared in 1738.
† Disq. anat. phys organismi corporis humani.—Viennæ, 1812.
‡ De fabrica et actione villorum intest. tenuium.—Leid. 1745.
§ Microscopische Gemüths-und Augenergötzungen.—Nuremberg, 1763.
|| In his Statistical Essays, vol. ii. 1740.

capillary circulation, and the account of the zig-zag contraction of the muscular fibres, which was rediscovered by Prevost and Dumas.

A contemporary ecclesiastic, Antonio della Torre, though a more laborious microscopist, accomplished less than Hales; for, like several of those already mentioned, he lacked a scientific mind to give life and force to his facts. Dr. Martin Barry (Philos. Trans. 1841) supposes that Della Torre really saw in the blood the rings which he described, (Philos. Trans. 1765, vol. lv.) and which others had mistaken, as he believed, for globules or discs; but the greater probability is, that this was but one of the many instances in which error has chanced to express itself in the language of truth; for the blood-corpuscles have central apertures only rarely, and the apertures are very hard to see: they could not have been discerned with such lenses as Della Torre used. It seems probable, however, that he saw both the ganglion-globules and the tubes in the structure of the brain; but his view of them was too obscure to add any likelihood to the supposition that he saw clearly any structure yet more minute, such as that of the blood-corpuscles.

After the middle of the eighteenth century the decline of microscopic anatomy continued to be rapid, in spite of the labours of two of the best physiologists that ever cultivated it; we mean William Hewson and Felix Fontana.

Hewson must ever be remembered for the accuracy of his observations on the blood and lymph corpuscles; and we are rejoiced to find Mr. Gulliver, who has worked with so much industry in the same field, claiming due honour for his predecessor;* for Hewson's merit as an observer has not hitherto been fairly appreciated. As far as his descriptions were carried they were almost without error; and, though the blood-corpuscles had been so often investigated, it was he who was first able to prove their flatness, (for at his time they were commonly supposed to be globular,) and the existence of a distinct central nucleus.† But he had the imprudence to append an improbable hypothesis to his facts, and they both fell into the discredit which only one deserved.

The investigations of Fontana were spread over a wider field, and many of them were singularly accurate, though at one time he seems to have had some bad glasses, which, like the celebrated lenses of Dr. Monro, made everything look like a set of spirals. Among his better observations are those of the structure of tendon, which was before nearly unknown; of the true import of the transverse striæ on the muscular fasciculi, which he first ascribed to the fibrils within the sheath; of the central substance of the nervous fibre, in which if he erred he did no more than Remak and some of the best modern observers do; and some accurate accounts of the structure of hair and of the fat-cells. But Fontana was not a mere microscopist. It has happened unfortunately for him that the only work of his which is generally known, or, as far as we are aware, has been translated from the Italian, is the treatise on the poison of the viper, to which his researches in structural anatomy form a kind of appendix. This is, indeed, a good book; but those alone which fully represent Fontana's labours and merits as an experimentalist are the several works

* See Gerber's General Anatomy, Appendix, p. 100.

† Experimental Inquiries, &c.—London, 1771-4, and Phil. Trans. vol. lxiii.

which he published on irritability, by which we are sure that a prior claim might be established for him as the author of many discoveries of which his successors have enjoyed the reputation.

With Fontana we may close the history of the first period of the study of structural anatomy by the microscope. Chiefly by the great influence of Haller, who was but a cold friend of microscopic examinations, the minds of physiologists were now turned to an almost exclusive study by experiment; and how completely all confidence in the microscope was lost at the beginning of this century may be judged from the fact, that the researches of which we have just traced the history were almost excluded by Bichat from his system.

The promulgation of Bichat's system is commonly spoken of as the birth-time of structural anatomy. But this is not strictly true. The origin of structural anatomy was, as we have already said, at least contemporaneous with that of descriptive anatomy; and for three centuries before the time of Bichat it had made a slow but steady progress. He did but add a vast amount of facts, systematize all that had been accumulated, and render all attractive by a physiological theory as fascinating as it was false: in a word, he completed, out of structural anatomy, that which had been before but a partial system of general anatomy. Not that Bichat's merit in general anatomy has been hitherto rivalled; on the contrary, so far as a system could be founded on the structure and properties discernible by the unaided senses, his was perfect; and for nearly twenty years afterwards all who wrote on the same subjects did but condense or comment on his writings. Nay, more, (and this is an unanswerable argument against those who pretend that all investigation of structure is futile which is not microscopic,) Bichat's arrangement of the tissues still stands good; and the microscope has ratified the general correctness of the divisions of tissues which he founded on their obvious and general characters. The truth is, that in the tissues, as in all other objects in nature, where there is a similarity in minute structure there is also a general resemblance. Just as the botanist, for example, can discern at a glance by its general aspect the species or variety of each of the minutest mosses or confervæ, and needs not, except as an ultimate test in difficult cases, to examine the minute arrangement of their parts; so in anatomy, each tissue has an obvious aspect, a gross physical constitution and distinct physiological properties, as well as a certain minute structure, and, for all practical purposes, can be distinguished by the one as well as by the other. In some cases, indeed, these coarser characters can be used for means of diagnosis where the microscope finds nearly identity; as, for instance, between the contractile and non-contractile fibro-cellular tissues, in the cellular tissue in all its varieties, and between the periosteum, and all other (so-called) fibrous tissues.

Upon this truth, we say, it is that Bichat's system still stands almost unaltered, and that much of his labour will still have a place in every prudently-written work on general anatomy. And therefore, although he was no microscopic observer, he contributed a great boon to microscopic anatomy, by showing how animal structure might be studied in an orderly and scientific scheme, instead of the hap-hazard manner in which they were investigated by Leeuwenhoeck and all his imitators.

Many years, however, elapsed before the microscope was again carefully or extensively employed in structural anatomy. Among the first who used it was Sir Everard Home, with the assistance of Mr. Bauer. But to Sir Everard no more praise can be assigned for this than for any other part which he bears in the history of physiology. It does not appear that he was the author of one clear or correct description; and though many of his mistakes may be ascribed to his heedless mode of preparing his objects for examination, yet one is compelled to suspect something more than unintentional error in his accounts, when they cannot by the same careless examination be confirmed; and still more when, in all the investigations which the same Mr. Bauer carried on under the superintendence of Robert Brown, (those, for example, on the fungi and insects infecting corn,) the accuracy of his delineations has never been surpassed.

Others of the earliest microscopic physiologists of this second period were Rudolphi, and Prevost and Dumas; but the first to whom an important place can be given is Gottfried Reinhold Treviranus, one of the most intellectual physiologists of this century, who first in 1816 (in his Vermischte Schriften,) endeavoured, with a philosophic view, to unravel all the tissues into their minutest structures. From this time, and with a regularly-accelerated velocity, the microscope has made progress in anatomy; till now, the rate of discovery is scarcely measurable, and the mind can hardly apprehend the greatness and variety of the results which it each day presents. But at this period of the history we may stop; for most of that which is in this regard important may be with some pains traced in the Report by Mr. Paget, which was published in the last Number. A better task will be to point out by the light of the past history what should be the spirit and tendency of future researches; for at the same time that we thoroughly admire the industry and honest emulation in which the microscopic investigations of the last period have been carried on, and rejoice in the accession to medical science of their brilliant and important results, we cannot help observing that there is now a tendency to error in the entire confidence which many give to statements drawn from microscopic evidence, and in the exclusiveness with which many rely upon the microscope, as if it were the only source of learning now left us for our use, or as if the facts it yields need only be collected in order that physiology may be perfected.

III. The fate of Leeuwenhoeck's discoveries, compared with that of Malpighi's plainly enough showed what every succeeding period has confirmed, namely, that facts of minute anatomy are useless for the advance of science unless they are at once, with a right judgment of their import and their relations, set in the place which they should occupy in the current physiology. No example can show more plainly than Leeuwenhoeck's that labour can make a discoverer, but can never, alone, make a philosopher; and that a microscope, however good and however well used, can never make a physiologist. With all his knowledge of facts, Leeuwenhoeck was neither an anatomist nor a botanist, nor, in a word, anything but a mere microscopist; and the same may be said of Ledermüller, Della Torre, and a number of his less eminent imitators. On the other hand, we see by the example of Malpighi of how

great avail the microscope may be made, even in its less perfect state. But it requires a physiologist to interpret its facts for physiology, a botanist to make good use of its results for botany: each science in which it can be employed must use it as an assistant, and in subordination to the laws and the facts obtained by other means; it cannot, in any science, take peculiar preeminence, much less can it in any accomplish so much that either thought or any other method of investigation can be dispensed with.

In the present day there are many Leeuwenhoecks in desire, if not in fact: that is, there are many who think their time well occupied in seeing, without understanding or explaining, the various objects of microscopic investigation. They are unknown in the world of science, because they do not, like their prototype, stand nearly alone; and one can only regret that so much time and labour of industrious, though not brilliant men should be lost. In the same spirit which actuates them separately the Microscopic Society has been founded. A number of persons—anatomists, botanists, geologists, chemists—are here combined, as if to form one large and complex Leeuwenhoeck, with nothing more in common in their disorderly pursuits than that they all study little things with the same apparatus. With as much propriety might a society be formed of those who will examine none but large things; for although the improvement of the microscope be stated as one of the purposes of this association, we cannot find by their proceedings that they have in the smallest degree contributed to this object. We have no doubt whatever that this society is even mischievous, by giving to an apparatus the highest place in the investigation of knowledge; and by inducing industrious men to study little pieces of every science, because they have the means of *seeing* the objects concerned in them. It is in this way that the microscope may be again, as it once already has been, employed to no good purpose. The truth is, the microscopic society exactly fits the foolish spirit of these times, in which men seem to think that a society and transactions are essential to the progress of every artificial division of science; and to compass the formation of a society, will take anything for a bond of union.

There is another reason also why the spirit of the times is peculiarly favorable to microscopic investigations, namely, because their fruits are mere facts, and often very small and unimportant ones. Now, since in the last Number we paid a very matter-of-fact compliment to this spirit by giving a large space to facts in their most condensed and unalleviated form, it must not be thought amiss if, in the present, we protest somewhat against the influence which those derived from the microscope are allowed to exercise; for we hold that facts never yet made a science, and especially that microscopic facts and those which divide the fashion with them—statistical facts—will never make a true physiology. They may grow in size and number, but such facts never improve in nature; they have, if we may so speak, no vital principle, no *nisus formativus* in them; they increase by super-position into a huge amorphous mass; but they never, by any force of their own, shape themselves into the symmetry of a science. For this they must be subjected to the formative influence of the understanding, which cannot use them rightly unless it have been better educated and exercised than in

these times it usually is. For in the present day, men, thinking it enough if they pursue and discover facts, and exercising those lower faculties of the mind which can apprehend isolated truths, are too apt to neglect the cultivation of their nobler law-discerning powers.

It is certain that, in proportion to the labour bestowed upon it, the real progress of physiology through microscopic observation, great as it is, has, on the whole, been slow. If, for example, we compare the advancement of the knowledge of the physiology of the nervous system with that of the circulation or secretion or any function of which the apparatus is clear to the assisted sight, we may at once discern the inferior value of mere observation. And it may be safely predicted that if the present love for isolated facts continue long, the progress of the higher truths of physiology will be still slower: for men are already educated for the study of facts rather than for the improvement of the thinking faculty; and with each succeeding year the number of soundly-thinking men must be diminished. It is strange indeed to see how men's minds are bent from that to which they were adapted by nature, by the fashion of the times they live in. Already we can see many suffering their highest faculties to lie in idleness, while they betake themselves to mere looking, and descend to the common low-level of the day; falling under the spell of fashion—becoming microscopists when they might and should be physiologists.

Another circumstance well calculated to make microscopic researches popular in the present day is, that much of the responsibility for accuracy in making them is thrown from the observer upon the apparatus; and that though much labour is requisite for success, it is, for the most part, labour of the dull, mechanical kind, for which energy and intellect are hardly necessary, if, indeed, they can be employed in it. This is truly the age for the glorification of apparatus; magnifying and counting instruments seem to be regarded by many as if they were fitted to do the work of the reason and the understanding; and we fear that the assertion of one of the best German anatomists, that "the future progress of physiology depends upon Schiek and Pistor, the microscope-makers," is but the expression of the general feeling of the majority of those who are pursuing medical science; for a man is now-a-days looked on coldly who advances an opinion which is not made to seem true by a great array of figures of calculation or of measurement.

As a proof that physiology, under the influence of this love of facts, may be studied too exclusively by the microscope, it should be remembered that there are several instances already detected in which similarity of microscopic structure is by no means safe evidence of a similarity of function. An epithelium-cell is very like a gland-cell, a hair-fibre like a smooth muscular fibre, and the physiologically different varieties of fibro-cellular tissue are not discernibly different in structure. These, then, are clearly subjects which can be completely or safely investigated only by experiment or by some of those other methods which the strict adherents of the microscope would throw into disrepute.

Again, at the very best, the microscope teaches only the coarse outlines of the forms of the apparatus in which the processes of the living body are carried on; and the apparatus which we thus see are not the parts by the mutual influences of which the processes are effected. These

processes depend, without doubt, upon the mutual relations of the elementary particles, the atoms of the body; and the distance by which we are by the microscope brought nearer to these is probably in comparison with the distance at which they are still removed from our view, infinitely small. The best knowledge, therefore, which the microscope can afford may be compared with the kind of acquaintance with chemical science which a man might acquire from an examination of the apparatus in a laboratory. In thinking otherwise men confound, as they are very apt to do, relative smallness with absolute simplicity; whereas, in truth, there is no reason to believe that the processes effected in a single organic cell are in their essential nature less complex or less obscure than those which go on in a large mass, or in a whole organ, and which are often only the same processes on a yet larger scale.

To whatever extent, then, microscopic investigations may be carried, and whatever improvements may be effected in the instrument, physiology, we think, will make but slow progress if all other means of study be not as diligently employed as if no microscope existed; and, above all, if the understanding, well trained and exercised, be not continually exerted to give form and vitality to the facts which the senses can discern.

In maintaining this we are far from desirous of seeing the ideal systems pursued by such men as Dr. Klencke gain ground in England. Nothing can show better how futile they are for advancing physiology than the history of general anatomy which we have just traced. For we see in it, as in the history of many other sciences, that, after making a certain progress in direct proportion to the industry with which the senses and the understanding were exercised upon it, it was arrested till an art enabled men to employ, not their reason, but their senses, with a supernatural force. At the time of Bichat a full measure of facts had been filled up; the unaided senses had perceived nearly all that lay within their range; and the understanding, by a fair induction, had discerned many laws. After this time there was, as we have shown, a pause for nearly twenty years; a period of calmness, in which the reason, placed as it was upon a high eminence of facts, and undisturbed, might have discerned (had it been sufficient, as idealists imagine, for the unveiling of the great truths of physical science,) many things that were suprasensuous and many still hidden laws. But the reason was employed in vain; and not one step did the science of histology advance till men again resorted to the microscope to assist their senses. If truth, therefore, of any sort be the object of pursuit, we have an unanswerable argument against idealism in the comparison of the results obtained in the last ten years of microscopic observation, with those of the preceding twenty years of meditation. With every inclination which self-love can excite in favour of the belief that man can think the truths of physical science, it is impossible to resist the fact that in this, as in so many other instances, the truths and laws have been discovered by the senses and the understanding, and by them alone. In general anatomy there is not one good argument for making idealism the rival of induction.

We believe, then, that a love of mere facts and idealism, or the love of exercising the imagination in the delusion that the reason is being employed, are the two evil spirits of the day, by which the progress of

physiology is retarded. Both work with the highest of their power in Germany, and they are maintained there by their mutual opposition. Both, too, are alike opposed to induction: which, as it is of English origin, ought to become each day more characteristic of English physiology. Microscopists are constantly saying that the microscope is to them what the telescope is to the astronomer. We wish they would make it so: if they would learn how much of astronomy is a mere matter of assisted sight, and how much is composed of laws worked out by the understanding, they could not but be led to make a great change in their mode of study. If astronomers had been merely telescopists, astronomy, properly so called, had certainly never existed: by the same rule, a century of Leeuwenhoecks would never have made a physiology, nor would all their senses have discerned so much as did the understanding of John Hunter.

We believe that if the understanding be exercised with an energy proportioned to the industry with which facts are pursued, the present will be a more brilliant period in the history of physiology than ever yet was known; for never were so many engaged in the pursuit, and never was there so much labour bestowed upon it; and already, by the few who combine clearness of thinking with accuracy of observation, some most striking and important results have been attained. Only, when we see an apparatus exalted so much above its due state of subserviency, we cannot help fearing lest much of that which is being done should be done in vain; and lest that which is gathered in disorder, and often with a heedless curiosity, should end, as it did once before, in mere obscurity.

ART. XVI.

Animal Chemistry; or Organic Chemistry in its Applications to Physiology and Pathology. By JUSTUS LIEBIG, M.D. PH.D. F.R.S. M.R.I.A., Professor of Chemistry in the University of Giessen. Edited from the Author's Manuscript by WILLIAM GREGORY, M.D. F.R.S. M.R.I.A., Professor of Medicine and Chemistry in the University and King's College, Aberdeen.—*London*, 1842. 8vo, pp. 354.

BRIEF as is the period that has elapsed since the publication of this work, we believe that there are few scientific men to whom its name, at least, is not familiar; for few publications of modern date have so strongly excited public attention. We feel called on, therefore, to take the earliest opportunity of introducing our readers to its valuable contents; and this we shall do by presenting to them as full an analysis as our limits will allow, interspersed with a few critical remarks on some points in which we are disposed to think that the author has fallen into error; referring to the work itself all such as desire fuller information as to its details. If we do not unhesitatingly subscribe to all the commendations which have been bestowed upon his labours, it is because, after an attentive consideration of them, we feel assured that in some particulars they will require modification; but, on the other hand, the same careful consideration has led us to attach very great importance to many views which have not struck others in the same light.

That Professor Liebig is *facile princeps* among the Organic Chemists of

the day, we believe that no one will deny. To say that the present flourishing state of the science to which he has devoted himself is mainly due to his labours—that every subject he has touched upon has been elucidated by his inquiries—and that there is none, however obscure, upon which it may not be expected that he (and the fellow-labourers who are following in his path) will throw some light—is to give him but his due meed of praise. And to say, further, that the present work by no means disappoints the expectations we had formed of it, but in many respects goes beyond them, will be admitted, we think, as a sufficiently high expression of our opinion of it. We regard its value as rather consisting in the novelty of the path of inquiry which it opens up, and in the numerous suggestions which arise as branches of that path, than in the results actually obtained. This is the author's own appreciation of his labours; and we regard it as a merit rather than as a fault, that he has thus early made them public, since he thereby enlists as his assistants in the prosecution of his inquiries all those who are at present devoting themselves to investigations of a more desultory character.

The chief fault which we have to find with the work is, that the author does not confine himself sufficiently within the limits of chemistry—the subject of which he is confessedly the greatest master—but connects his chemical statements with speculations in physiology and pathology—in which, as it seems to us, he generally fails. He appears to have done this somewhat unwittingly; for the following remark, which occurs in the dedication to the British Association, seems to show that he is well aware of his true province:

"When the chemist shows, for example, that the elements of bile, added to those of the urate of ammonia, correspond exactly to those of the blood, he presents to us a fact which is independent of all hypothesis. It remains for the physiologist to determine, by experiment, whether the conclusions drawn by the chemist from such a fact be accurate or erroneous. And whether this question be answered in the affirmative or in the negative, the fact remains, and will some day find its true explanation."

Our author is evidently possessed with the idea, that by chemistry alone can physiology be now advanced. It is certainly well for science that its labourers should restrict themselves to particular departments: and it is pleasant to meet with enthusiasts, who, by their exclusive devotion to one set of ideas, are pretty sure to work out all that is valuable in them. But we regret to see a man who occupies a position so eminent in the scientific world as that on which Liebig stands, committing himself to the following extraordinary statements, which imply a degree of exclusiveness that we should not have expected from him:

"During the last five-and-twenty years physiology has acquired new ways and methods of investigation within her own province; and it is only *the exhaustion of these sources of discovery* which has enabled us to look forward to a change in the direction of the labours of physiologists. The time for such a change is now at hand; and a perseverance in the methods lately followed in physiology would now, from the want which must soon be felt of fresh points of departure for researches, render physiology more extensive, but neither more profound or more solid.. .... The study of the uses and functions of the different organs, and of their mutual connexion in the animal body, was formerly the chief object of physiological researches; but lately this study has fallen into the back ground. These researches have yielded us the most valuable results, in relation to the recognition of the dissimilar forms and con-

ditions to be found in the healthy and in the diseased organism; but they have yielded no conclusions calculated to give us a more profound insight into the essence of the vital phenomena." (Preface, pp. 11-12.)

Now we fearlessly assert that at no time has the advance of pure physiology (from which we exclude chemistry) been so rapid, as during the last five-and-twenty years; and that at no part of that period has this rapidity been so extraordinary as during the last five years. So that, so far from apprehending an exhaustion of our methods of research, we ourselves anticipate a continually-increasing harvest of new results. The true relation of chemistry to physiology appears to us to be entirely misunderstood by Liebig; and we shall endeavour to explain it in a few words. Chemistry may fairly take cognizance of all those processes, by which the vegetative functions of the living organism are brought into relation with the world around. It may, on the one hand, trace the conversion of the alimentary materials into organizable products; but there it ends: it can throw no light upon the process of organization, or upon the new (vital) properties which are called forth by it. On the other hand, it can take up the same materials, in the act of being restored to the inorganic world by the metamorphosis of the tissues; and it may give an account of all the changes which these materials undergo, up to the time when they are finally cast off by some of the excretory processes. But of the *vital* functions of the organized tissues, which it is the peculiar province of the physiologist to investigate, chemistry can give no account whatever; and these constitute a field which may well find room for the united labours of any number of investigators.

In thus limiting the domain of chemistry, however, we shall not be found to underrate its practical importance; for we leave to it the investigation of the whole range of effects produced on the living organism by external agents, whether alimentary materials, morbific causes, or remedial substances; and it will be found that all the results obtained by Liebig have reference to one or other of these departments.

We shall not follow, in our analysis, exactly the order of the work, because we should thereby be led into much repetition. It might be wished that the editor had reduced it to a form better adapted for being readily comprehended by the student. We do not regard it as by any means a readable book. The arrangement seems to us to be far from good. The train of thought is often interrupted by digressions; and it is frequently difficult to keep a clear view of the main argument. This must be our excuse, if we should prove to have sometimes misunderstood our author.

On many most important questions we have not ventured to offer an opinion, because they must be regarded as still *sub judice*, and as to be decided rather by chemical than by physiological evidence. And we may here especially advert to one of our author's fundamental assumptions—that of the absence of power in the animal body to convert the non-azotized elements of food into azotized compounds—as requiring confirmation. It is well known that the vegetable, by a supply of ammonia, can form gluten out of what would otherwise have been deposited as starch; and it is not enough to assert dogmatically that the animal is destitute of this power. It has been distinctly stated by Dr. Prout, that he has found albumen in the duodenum when none was formed in the stomach; and

this circumstance has led him to the conclusion that a highly-azotized substance may be secreted into the duodenum, for the purpose of being united with the non-azotized constituents of the food, to form a compound adapted for the nutrition of the tissues. If this assertion be substantiated, a large part of Liebig's doctrine of nutrition and secretion will fall to the ground.

The discussion of the details of these functions is appropriately introduced by a few remarks on the vital force, to which a large proportion of the phenomena exhibited by living beings must be attributed; our author's views as to the nature of this force are more clearly set forth, however, in a subsequent part of the work, from which we derive the following account of them.

1. *Vital force.* The various changes exhibited in the growth of a plant or animal—involving as they do an immense variety of individual actions, no one of which can be entirely explained by any application of the known laws of physics or chemistry, whilst many of them take place in direct opposition to these laws—cannot be philosophically viewed in any other light, than as dependent on a cause or set of causes peculiar to themselves; but having strong analogies with the agents which operate on inorganic matter. "If the vital phenomena be considered as manifestations of a peculiar force, then the effects of this force must be regulated by certain laws, which laws may be investigated" like those of physics or chemistry. It manifests itself in the growth of the organism, and in its resistance to those external agencies which would tend to disintegrate the mass. It further presents itself as a cause of motion, and of change in the form and structure of the material substances which serve as food, disturbing and abolishing the chemical equilibrium in which they previously were, and causing their elements to arrange themselves in new forms, so as to produce new compounds. Of these compounds, some are identical in composition with the living tissue, and are made to become a part of that tissue by the attractive power of the vital force; whilst others, whose composition differs from that of the living tissue, are removed from the situation in which they are found, and are passed off as excretions by other parts of the body.

"The manifestations of the vital force are dependent on a certain form of the tissue in which it resides, as well as on a fixed composition in the substance of the living tissue. The capacity of growth in a living tissue is determined by the immediate contact with matters adapted to a certain decomposition, or the elements of which are capable of becoming component parts of the tissue in which vitality resides. The phenomenon of growth or increase in the mass presupposes that the acting vital force is more powerful than the resistance which the chemical force opposes to the decomposition or transformation of the elements of the food." (p. 198.)

Our author then goes on to point out that, in order to attain a clear conception of the manifestations of the vital force, differing as they do so remarkably amongst themselves, it is necessary to bear in mind that every known force is recognised by two conditions of activity, which present different phenomena to the attention of the observer. Force exerts itself in matter either in the form of resistance to external causes of motion or of change in form and structure, or as a moving force when no resistance is opposed to it or when that resistance is overcome by it. In the former case there is a state of equilibrium; and we only know of the existence

of these forces by the result of the disturbance of the equilibrium through any external agency. In the latter case we recognise the force by its obvious effects.

"Both these manifestations of activity may be observed in that force which gives to the living tissues their peculiar properties. The vital force appears as a moving force or cause of motion, when it overcomes the chemical forces (cohesion and affinity) which act between the constituents of food, and when it changes the position or place in which their elements occur; it is manifested as a cause of motion in overcoming the chemical attraction of the constituents of food, and is, further, the cause which compels them to combine in a new arrangement, and to assume new forms." (p. 204.)

Its existence in the passive condition is evinced by the power of resisting those forces, which would otherwise disintegrate the structure.

We are not left to suppose, however, that this force has any existence distinct from the matter on which it acts. The *analogy* between vital force and chemical force is extremely strong; although every idea of their *identity* is contravened by the fact of their continually-opposing action. No philosopher has ever proclaimed the existence of a chemical force or principle as an entity separate from matter, and controlling its operations *ab externo;* since all are agreed that it is nothing more than a result of certain properties of matter, which are involved in our fundamental conception of it. Equally unphilosophical would it be to endeavour to abstract a vital force or principle from those peculiar forms of matter on which it operates. It is most justly urged by Liebig that—

"As the manifestations of chemical forces seem to depend on a certain order in which the elementary particles are united together, so experience tells us that the vital phenomena are inseparable from matter; that the manifestations of the vital force in a living part are determined by a certain form of that part and by a certain arrangement of its elementary particles. If we destroy the form or alter the composition of the organ, all manifestations of vitality disappear. *There is nothing to prevent us from considering the vital force as a peculiar property, which is possessed by certain material bodies, and becomes sensible when their elementary particles are combined in a certain arrangement or form.* This supposition takes from the vital phenomena nothing of their wonderful peculiarity; it may therefore be considered as a resting-point, from which an investigation into these phenomena and the laws which regulate them may be commenced, exactly as we consider the properties and laws of light to be dependent on a certain luminiferous matter or ether, which has no further connexion with the laws ascertained by investigation............A living part acquires, on the above supposition, the capacity of offering and overcoming resistance, by the combination of its elementary particles in a certain form; and as long as its form and composition are not destroyed by opposing forces, it must retain its energy uninterrupted and unimpaired. When by the act of manifestation of this energy in a living part, the elements of food are made to unite in the same form and structure as the living organ possesses, then these elements acquire the same powers. By this combination the vital force inherent in them is enabled to manifest itself freely, and may be applied in the same way as that of the previously existing tissue." (pp. 209-10.)

The views here upheld are *identically* the same with those which were some years since maintained by Dr. Carpenter, in his essay "On the Laws regulating Vital and Physical Phenomena," and since embodied in his Principles of Physiology. The doctrine contained in the sentence in italics is that which it was the whole object of the essay to establish;

and even the verbal correspondence is extremely striking, as the following quotation will show:

"But there is nothing inconsistent with our knowlege of the physical properties of matter, in the belief that all matter (or each at least of those forms of it capable of being assimilated) is also endowed with vital properties; which remained undisplayed or occult, until brought into action by being subjected to those conditions which a living organized system can alone afford." (Edinb. New Phil. Journal, April, 1838.)

Whatever merit, therefore, is due to the idea, (which, if correct, is indisputably an important step in generalization,) it may be fairly claimed by Dr. Carpenter; but we feel sure that, far from attempting to fix upon Liebig a charge of literary plagiarism, he will rejoice to find his position strengthened by the independent testimony of so eminent a philosopher. A more valuable example could scarcely be adduced of the fact, that fellow-labourers in the pursuit of truth, engaged in the same inquiries, and with the same spirit, are likely to arrive at the same conclusions, and may express them in language almost identical.

II. *The phenomena of motion in the animal organism.* Some speculations upon this subject are connected by Liebig with his propositions regarding the vital force; and though, as we conceive, they are weakened in their application by his neglect of certain fundamental physiological truths, yet they are of so interesting a character that we cannot pass them over in silence. The guesses of a Liebig, even when proved to be partly erroneous, are more worthy of attention than the pretending dogmata of shallower minds; and we shall find that these contain many ideas of high value, which, when more correctly applied, may be found to afford the true expression of the causes of the phenomena in question. The following passage is continuous with that last quoted:

"If now we bear in mind that all matters which serve as food to living organisms are compounds of two or more elements, which are kept together by certain chemical forces—if we reflect that, in the act of manifestation of force in a living tissue, the elements of the food are made to combine in a new order—it is quite certain that the momentum of force or of motion in the vital force was more powerful than the chemical attraction existing between the elements of the food. The chemical force which kept the elements together acted as a resistance, which was overcome by the active vital force. Had both forces been equal, no kind of sensible effect would have ensued. Had the chemical force been the stronger, the living part would have undergone a change. If we now suppose that a certain amount of vital force must have been expended in bringing to an equilibrium the chemical force, there must still remain an excess of force by which the decomposition was effected. This excess constitutes the momentum of force in the living part, by means of which the change was produced; by means of this excess the part acquires a permanent power of causing further decompositions, and of retaining its condition, form, and structure, in opposition to external agencies. We may imagine this excess to be removed, and employed in some other form. This would not of itself endanger the existence of the living part, because the opposing forces would be left in equilibrio; but by the removal of the excess of force the part would lose its capacity of growth, its power of causing further decompositions, and its ability to resist external causes of change. If, in this state of equilibrium, oxygen (a chemical agent) should be brought in contact with it, then there would be no resistance to the tendency of the oxygen to combine with some element of the living part, because its power of resistance has been taken away by some other application of its excess of vital force." (p. 212.)

In plants there is no demand upon the vital force for the maintenance of the form and structure of their non-azotized constituents; since these, when once formed, have little or no tendency to disintegration, except when affected by external agents. Their azotized principles, however, exhibit the same tendency to spontaneous decomposition as do those of animals, and need, therefore, in Liebig's opinion, a constant exertion of the vital force for their preservation. During every period of the life of a plant, the available vital force (that which is not neutralized by resistance,) is expended only in one form of vital manifestation—that of growth or increase of mass, or the overcoming of resistance; and it is interesting to observe, that those species which have the longest life and the greatest capacity for extension, are those which form the least amount of azotized principles, whilst those that produce the greatest quantity of these (as the cerealia and the fungi) have but a brief individual duration. But in animals, besides this source of expenditure, there is another, of still greater importance—the production of motion, whether this be employed in the economy of the body itself, or in changing its place in relation to the external world. In order to illustrate his views of the mode in which the vital force is concerned in animal motion, Liebig has recourse to the various remarkable phenomena of the galvanic battery. By a chemical action going on in one spot between acid and metal, a force is created which circulates through a wire of any extent, and which may be employed in any part of it to produce decomposition of bodies held together by the most energetic affinities, or to overcome the most powerful mechanical resistance. Of the nature of the force which circulates in the wire, of the mode of its propagation, and of the manner in which it operates in producing the separation or union of chemical elements, or the movement of particles or masses of matter, we really know not one iota more, than we know regarding the corresponding manifestations of the vital force.

The nerves are thought by Liebig to resemble the wire of the galvanic circuit in their mode of producing motion:

"By means of the nerves, all the parts of the body, all the limbs, receive the moving force which is indispensable to their functions, to change of place, to the production of mechanical effects. *Where nerves are not found motion does not occur.* The excess of force generated in one place is conducted to other parts by the nerves. The force which one organ cannot produce in itself is conveyed to it from other quarters; and the vital force which is wanting to it, in order to furnish resistance to external causes of disturbance, it receives in the form of excess from another organ, an excess which that organ can not consume in itself." (p. 220.)

The readers of this Journal scarcely require to be reminded, that the doctrine contained in this quotation is one which we have on all occasions felt it our duty to protest against. If it were true, how could the fact be explained, that there is an extensive system of muscles, which can with great difficulty be acted on by any stimulus conveyed through the nerves, whilst they are readily excitable by stimuli applied directly to themselves; or that, in all instances, the nerves lose their power of communicating stimuli after the circulation has ceased, long before the muscles lose their irritability? Equally erroneous is the statement in the former part of the work, that, whilst in the animal organism everything to which the name of *motion* can be applied proceeds from the

nervous apparatus, the phenomena of motion in vegetables, the circulation of fluid, for example, in the characeæ, and the closing of the flowers and leaves depend on physical and mechanical causes. (p. 3.) We do not apprehend that any valid distinction can be drawn between the folding of the leaves of the mimosa or the closure of the trap of the dionœa, on the one hand, and the action of the heart and alimentary canal of the highest animal on the other. Both are equally dependent upon stimuli external to them, acting on a contractility inherent in them; and this contractility is, in one case as in the other, a manifestation of vital force. In his next proposition, however, we feel assured that our author is correct; and we have a strong opinion of its importance as a physiological principle:

"We observe further, that the voluntary and involuntary motions, in other words, all mechanical effects in the animal organism, are accompanied by, nay, are dependent on, a peculiar change of form and structure in the substance of certain living parts, the increase or diminution of which change stands in the very closest relation to the measure of motion, or the amount of force consumed in the motions performed. As an immediate effect of the manifestation of mechanical force, we see that a part of the muscular substance loses its vital properties, its character of life; that this portion separates from the living part, and loses its capacity of growth and its power of resistance. We find that this change of properties is accompanied by the entrance of a foreign body (oxygen) into the composition of the muscular fibre (just as the acid lost its chemical character by combining with the zinc;) and all experiment proves that this conversion of living muscular fibre into compounds destitute of vitality, is accelerated or retarded, according to the amount of force employed to produce motion. Nay, it may safely be affirmed that they are mutually proportional; that a rapid transformation of muscular fibre, or, as it may be called, a rapid change of matter, determines a greater amount of mechanical force; and conversely, that a greater amount of mechanical motion (of mechanical force expended in motion) determines a more rapid change of matter. From this decided relation between the change of matter in the animal body and the force consumed in mechanical motion, no other conclusion can be drawn but this, that the active or available vital force in certain living parts is the cause of the mechanical phenomena in the living organism . . . . How, indeed, could we conceive that a living part should lose the condition of life, should become incapable of resisting the action of the oxygen conveyed to it by the arterial blood, and should be deprived of the power to overcome chemical resistance, unless the momentum of the vital force, which had given to it all these properties, had been expended for other purposes?" (p. 221.)

Thus the change of matter, the manifestation of mechanical force, and the absorption of oxygen, are, in the animal body, so closely connected with each other, that we may consider the amount of motion and the quantity of living tissue transformed, as proportional to the quantity of oxygen inspired and consumed in a given time by the individual. The nerves are regarded by Liebig in the light of conductors, which convey the vital force from the muscles, and apply it to the purposes of animal motion; and he thinks it a sufficient explanation of the absence of motor properties in the gelatinous tissues, mucous membranes, &c., to advert to the absence of nerves in these tissues. On this hypothesis, the muscles must be regarded as so many generators of vital force; which, when their product has been removed, die away like an annual that has borne its seed. Fully agreeing with him as to the existence of the connexion which he points out between the action of a muscle and the

decomposition of its substance, we yet think that the vital force which is employed to produce the motion, so far from being carried away by the nerve, is expended on the spot; and that the function of the nerve is to convey from its central organ some influence which excites the change, just as the wire proceeding from a galvanic battery excites an ordinary chemical decomposition. We doubt not that this simple and obvious analogy would have been adduced by our author himself, if his mind had not been possessed by those erroneous notions respecting the connexion between nervous agency and muscular movement which still prevail amongst his countrymen.* The gist of his argument, however, is not affected by this error; since his purpose is chiefly to establish that relation between muscular action and the waste of the structure, which we with him regard as indisputable. Of this waste a part is continuous and almost invariable, being dependent upon those regular movements which are necessary to the maintenance of life, such as those of the heart, respiratory muscles, &c.; whilst another portion is altogether regulated in amount by the exercise to which the general muscular apparatus has been subjected. The equilibrium between the supply and the waste of matter can only occur, when the portion separated or expelled in a lifeless form is, at the same instant in which it loses its vital condition, restored in another part. The same arterial blood which brings the oxygen to disintegrate, conveys the fibrin to nourish; and the determination of blood to a particular muscle or set of muscles, which occurs as a consequence of their functional activity, becomes the cause of their increased nutrition. But for this replacement *time* is required; the power of growth is much more limited than the waste; and it may be doubted whether the increase can take place at the same moment at which the disintegration is occurring. In order, therefore, to repair the loss which is occasioned by muscular action, it is necessary that there should be a frequent cessation of that action; and this seems to be the object of that rest which is provided for all the voluntary muscles in *sleep*. In this condition the animal is reduced almost to the level of a plant; for, as in the latter, all the vital force is employed upon the nutritive functions. It is, in the opinion of Liebig, from the absence of nerves, which he regards as conducting away the force generated in the several parts of the system, that the plant derives its capacity of almost unlimited growth, and its power of overcoming the strongest chemical attractions in the substances which serve as its food; for instance, that of the oxygen and carbon in the carbonic acid which it decomposes. In the animal, which is supplied with food already assimilated in chemical nature to its own constitution, the vital force is employed as moving power. "In what form or in what manner the vital force produces mechanical effects in the animal body is altogether unknown, and is as little to be ascertained by experiment as the connexion of chemical action with the phenomena of motion which we can produce with the galvanic battery." But we really know no more than this of the mode in which "a certain something, invisible and imponderable in itself (heat), gives to certain bodies the power of exerting an enormous pressure on surrounding objects; we

* It is not a little remarkable that the Hallerian doctrine should now be most violently opposed in the country of its author; whilst it is most strongly upheld in Britain, especially in the Edinburgh School, which at first most stoutly combated its reception.

know not even how this something itself is produced when we burn wood or coals. So it is with the vital force, and with the phenomena exhibited by living bodies. The cause of these phenomena is not chemical force; it is not electricity nor magnetism—it is a force which has certain properties in common with all causes of motion and of change of form and structure in material substances. It is a peculiar force, because it exhibits manifestations which are found in no other known force." (p. 232.)

In the preceding very beautiful train of speculation, there is one important point left out of view; namely, the *constant* action of the involuntary muscles concerned in the functions of respiration and circulation. However true Liebig's doctrine may be in regard to the necessity of a period of repose for the renewal of the muscular substance which has been disintegrated by action, it obviously applies only to the voluntary muscles; and the nutrition of the heart and of the muscles of respiration must be as continual as their loss. Still his general theory,—*that the source of mechanical power in the organism, is the conversion of living parts into lifeless amorphous compounds*, is not in the least invalidated by such objections; which have reference only to the details of its application.

The doctrine here advanced by Liebig—that the rapidity of transformation of muscular tissue is closely connected with the demand made upon its functional activity—has been previously stated, though in a less precise form, by several physiologists. Thus, in Müller's Prolegomena we meet with the following passages:

"As the excretions are constant, even when the supply of nutriment is stopped, it necessarily follows that a constant decomposition of the substance of the body is essentially connected with life. It cannot, indeed, be otherwise, if it be true, as it has been already proved to be, that the vital force is manifested in an animal only while certain stimuli produce in the living tissues constant material changes, of which the phenomena of life are merely the external signs, just as flame is the appearance resulting from the material changes effected in combustion. The impulse to these material changes is given by respiration." (Baly's Translation, p. 38.)

"Exhaustion ensues when the action of an organ is increased without any external stimulus, if the organic force is not increased at the same time. *It appears, therefore, that the very action of organs produces a change in their composition.* It may be that the constant change which is produced in the organic substance by the action of the arterial blood, and which is as necessary to life as the decomposition of the burning matter is to the phenomenon of combustion, is accelerated or increased by the action of the organ, while the renovation from new nutritive matter does not take place with proportionate rapidity, and can only be effected gradually during rest. Generally, however, the more exertion a man uses, the more active seems to be the decomposition of the matters of his body, and the more need has he for nutriment. Both men and brutes that have died after very violent exertion, as in the instance of a stag hunted to death, undergo putrefaction much sooner than animals bled to death." (Ibid. p. 52.)

The same doctrine, modified by the recent discoveries regarding the independent vitality of the several parts of the organism, has been expressed by Dr. Carpenter in his last work, as shown by the following extract:

"Hence it may be stated as a general law, that the vital activity of the cells (and of the parts produced by their transformation) diminishes in proportion

to the prolongation of their life; and this law exactly corresponds with what may be observed in comparing the tissues of different kinds which are present in the same body. For we uniformly find that those in which the most active vital changes are going on (such as the nervous and muscular tissues,) are those in which the duration of the individual component portions is the least, as is shown by the rapidity of the changes of removal and reposition which are continually taking place in them. The converse holds good also. Further, it may be remarked—and this is a matter of much practical importance—that anything which increases the functional activity of any particular tissue, thus causing its cells to live faster, diminishes the duration of their lives, as is shown in the increased demand for nourishment which is set up as a consequence of the continued exercise of the muscular or nervous system, and which, being far greater than can be required for such increase of their amount as results from that exercise, necessarily indicates that a corresponding removal of effete matter resulting from the death of the cells, has taken place." (Principles of Human Physiology, p. 535.)

There is, then, in the living animal body, a constant waste of the muscular substance which constitutes a large proportion of its mass; a part of this waste, occasioned by the action of the heart and respiratory muscles, is independent of the will, and goes on at a nearly equal rate during the period of rest; the other part is entirely dependent for its amount on the degree of exertion which has been required from the voluntary muscles during a given time. The degree of this waste may be measured by the quantity of azotized matter in the urine; since, as will be shown hereafter, this matter is entirely derived from the decomposition of the azotized tissues, of which the muscular must supply by far the largest proportion. Its amount, therefore, is proportional to the mechanical effects produced by an individual in a given time; whether the mechanical force has been employed in voluntary or involuntary motions; whether it has been consumed by the limbs, or by the heart or other viscera. In order to maintain the healthy condition of the body, an equivalent amount of azotized food will be required to restore this waste; and in the adult, a perfect balance will be maintained between the consumption of vital force for supply of matter, and that for mechanical effects. But in the infant, whose nutrition is very active, the mechanical power is very low; the vital force being expended, as in the vegetable, on the increase of the organism. In the old man, on the other hand, there is a larger proportion of mechanical power to the amount of food consumed; but a continual waste is going on. The amount of sleep necessary to restore, in the adult labouring man, the loss which has been sustained by activity, may be stated as seven hours out of the twenty-four. An old man is satisfied with only half this amount of sleep, consequently his expenditure of mechanical power should not be more than half. If an adult man expends more mechanical power than can be restored by seven hours' sleep, or diminishes his quantity of sleep so as not to produce the requisite equilibrium, he grows old prematurely. But an infant at the breast sleeps twenty hours and wakes only four; and all the vital force not expended in mechanical efforts goes to generate new tissue. According to Liebig, if we consider the expenditure of vital force in mechanical effects as counterbalanced, in the adult man, by that which produces the formation of new parts,—the ratio being therefore as 100 : 100,—the ratio of mechanical effects in the infant to the formative effects may be stated as 25 : 250; and in the old man as 125 : 50.

All these effects, however, are greatly modified by temperature. As the vital power of the plant, by which it decomposes the carbonic acid of the air, is dependent upon light, so is the energy of the animal dependent upon heat. For the maintenance of this heat, it is provided with a source within itself; namely, the combination of oxygen with the combustible elements of its structure, or with substances contained in its food. The lower the temperature of the surrounding medium, the more oxygen is introduced into the system for this purpose, the faster does the change of its substance go on, the more force is available for mechanical purposes; consequently, if the supply of food is sufficient, and the system has the power of maintaining its proper standard of heat, a moderate external temperature will be favorable to muscular energy; and this common experience teaches. But if the supply of food be deficient, or the loss of heat by external cooling be greater than that which the system can replace, so that the temperature of the body itself be depressed, then the power to produce mechanical effects is diminished, a condition of sleep ensues, and at last even the vital and involuntary motions cease, so that syncope or death supervenes. The quantity of power available for mechanical purposes is also diminished by causes which prevent a due supply of oxygen to the muscular structure; hence it is that in ascending high mountains, the muscular power is greatly diminished by the rarity of the air. Again, the introduction of easily oxidizable substances into the blood, by the union of which with oxygen the animal temperature is kept up without any disintegration of its tissues, may be imagined to prevent the action of oxygen on the muscular substance, and the consequent liberation of mechanical power. This is considered by Liebig to be the mode in which alcohol increases the animal temperature, whilst it diminishes muscular power. He affirms that it is not passed off by any of the excretions, and that there seems no channel by which it can be got rid of, unless it is thus consumed within the body. We cannot help thinking, however, that his statements on this point are too exclusive; for his assertion that alcohol is not separated in the expired air would seem to be contradicted by ordinary observation, the breath often affording pretty *strong* testimony to a fact which the speech denies; and the experiments of Dr. Percy leave no room for doubt on the matter. Moreover, we feel sure that alcohol must have a specific action on the nervous system; else why the extraordinary modification on the functions of the brain produced by it; and whence the passage of this fluid into the ventricles of the brain, which was proved some years since by the ingenious experiments of Dr. Percy? This is another of the many errors in physiological detail, which will, we fear, prevent the value of the author's labours from being justly appreciated by some of those who possess the capability of discerning them.

In the hybernating animal the temperature of the body falls to a low standard, and the vital energy of the whole system is thereby greatly diminished. Only enough remains to perform the involuntary motions necessary for the support of life; but the waste of the muscular apparatus is checked in the same proportion; and there is little expenditure of material, except of that which goes to keep up the temperature of the body, which is chiefly supported by the combustion of the fatty principles. In other cases the active force of the body may be entirely consumed in

producing voluntary mechanical effects, in such wise that no force shall remain available for the involuntary motions. Hence the depressed state of the circulation, which is a familiar consequence of excessive fatigue; and hence the death by syncope of an animal urged to a still longer continuance of muscular exertion. It is remarked by Liebig that, when a stag is hunted to death its flesh becomes uneatable in consequence of the metamorphosis which its muscular tissue has undergone; and the badness of the meat of overdriven cattle is a fact familiar to every one. We have seen, however, that this fact had been previously noticed and correctly applied by Müller.

III. *Food of animals, and its applications in their economy.* Referring to our former report (Br. & For. Med. Rev. No. XXVII, p. 298-308,) for the details contained in the portion of the work which bears on this subject, we shall here give such a brief abstract of our author's views as may enable his general argument to be understood. The food of plants entirely consists of the inorganic binary compounds—carbonic acid, water, and ammonia; and their vital force is employed in the decomposition of these, and in the union of their elements into the peculiar proximate principles which are formed by vegetables. The food of animals consists either of the flesh of animals, resembling their own in composition; or of the proximate principles formed by vegetables. Now it is a recent discovery of the highest importance, which has been entirely substantiated by the analyses of Mulder, that the chief azotized proximate principles of plants, namely, vegetable albumen, fibrin, and casein, not only bear a very strong analogy to the corresponding principles, which enter into the composition of animal bodies, but are absolutely identical with them in the proportion of their elements. Hence the problem of the nutrition of herbivorous animals is much simplified; since it is not requisite to imagine that there is any more elaborate process of conversion exercised upon these articles derived from the vegetable kingdom, than is required for those immediately obtained from the animal body. The digestive process separates the azotized vegetable principles from those which are merely hydrates of carbon, such as starch and sugar; and applies the former to the maintenance of the azotized tissues, whilst the latter are destined for another purpose. All parts of the body which have a decided shape, which form parts of organs, contain nitrogen: water and common fat are the only constituents which are destitute of that element; both these are amorphous and unorganized, and only so far take part in the vital process, as that their presence is required for the due performance of the vital functions. The azotized parts are those most liable to decomposition, and require the most constant regeneration. The materials for this regeneration must, in Liebig's opinion, be entirely and immediately derived from the food; and accordingly animals cannot be supported for any length of time upon matters destitute of nitrogenized constituents; whilst they require a less amount of vegetable substances for their nutrition, in proportion as these abound in such constituents.

The nutritive process in carnivorous animals may be advantageously studied in detail, as affording a simple means of determining the mode in which the elements of the food are disposed of. In the adult animal, the sum of the excretions must equal that of the ingesta, the bulk of the

body itself undergoing no change. The fæces of such an animal are dry, and contain only the indigestible parts of its food; and the only proper excrement is that expelled by the urinary passage. This consists, in the carnivorous serpents, of urate of ammonia, which contains only two equivalents of carbon for every equivalent of nitrogen; and in the carnivorous mammalia, of urea, in which there is but one equivalent of carbon to one of nitrogen. But in the flesh which forms the food of these animals, and in that of which their own body is composed, the ratio of carbon to nitrogen is at least as eight to one; consequently the amount of carbon to be excreted by some other channel, is six or seven times that of the nitrogen passed off through the kidney. For this purpose, there is no other channel than the respiratory apparatus,—the skin and lungs; by which the carbon is thrown off as an oxidized product. In this mode the hydrogen of the food is also disposed of. Still, nothing is more certain, than that the carbon, hydrogen, and nitrogen given out, though equal in amount to what is supplied in that form, do not directly proceed from the blood. In the adult animal the food serves to restore the waste of matter; certain parts of its fabric have lost the state of vitality, have been expelled from the substance of the organs, and have been metamorphosed into new compounds which are amorphous and unorganized. Every action in the body involves such a waste. It has been already shown to accompany every exertion of muscular power. Recent discoveries regarding the nature of the secreting process have shown that it takes place in that function to a large amount. And we have no hesitation in extending the doctrine of Liebig from the muscular to the nervous system; and in affirming that every act of sensation, every exertion of voluntary power, every operation of the intellect, every play of the emotions,—in short, every functional exercise of the nervous system, involves a similar passage of nervous tissue from the organized to the inorganic condition. There is, therefore, a constant withdrawal from the blood of the elements which are required to supply this waste and to form the tissues; and the elements thus withdrawn must be supplied by the food, all the nutritious part of which is first converted into blood, and then into organized tissue. After having done its work, the nitrogenized portions, of which no further use can be made in the system, are separated by the kidneys; whilst the remaining carbon and hydrogen are oxidized, so as to liberate the necessary supply of animal heat, and are then carried off through the lungs. We have no reason to believe that, in carnivorous animals, a particle of the flesh or blood once received within the body is thrown off by the excretory process, until it has become the living flesh and blood of the being nourished by it. The secretion of bile and of urine goes on in animals that are in a state of hybernation, or are undergoing starvation; and it has been found that the amount of urea formed by dogs fed upon pure sugar for three weeks was equal to that excreted in the normal condition. The formation of these secretions, therefore, has *no direct* connexion with the ingestion of food, and takes place entirely at the expense of the living tissues which are continually in process of disintegration.

Between these two products, however, there is an important difference. Urine is to be regarded in the light of a simple excretion, having no ulterior purpose in the body, and destined simply to carry off what it

would be injurious to retain. On the other hand, bile would seem to have a specific use in the digestive process; and it does not pass off as an excretion, until its state has been completely changed. The latter part of this doctrine is founded on the very startling but positive assertions of Liebig, that the constituents of the bile cannot be recognized in the fæces, at least among carnivorous animals, and that we must suppose the whole of the secreted fluid to be reabsorbed, and its hydro-carbon to pass off by the lungs. We must take the liberty of questioning whether, in the present state of chemical knowledge, such an assertion is justifiable. It is well known that the constituents of the bile are recognized with so much difficulty by chemical tests, and are so readily altered in their character by various reagents, that there has been great difficulty in establishing what its proximate principles really are. We do not, therefore, deem the chemist's assertion as to the absence of biliary matter in the fæces altogether valid; more especially with the obvious fact before us, that their colour, at least, is due to it, as is shown by the effects of obstructions to the flow of bile, occurring in the course of disease, or artifically induced, in decolorizing them. But we can readily imagine that the doctrine of Liebig, as to the reabsorption of the biliary secretion, is more correct when applied to the carnivora, than to omnivorous or herbivorous animals; since in the former the fæces are white and dry; and it is only when they approach the condition of the others (as in the case of the domesticated dog or cat) that the fæces seem tinged with bile. Indeed the supposition appears to be required by the remarkable fact, that, whilst the proportion of elements in the entire mass of blood is equal to that of the average of flesh (the formulæ for both, as Liebig has shown, being the same), the sum of the elements of bile and urine amounts to almost precisely the same; so that there seems to be no superfluous carbon for respiration. All the carbon used for this purpose in the carnivora appears to have passed through their tissues, and to have been separated from the general current of the circulation, by the liver. In the herbivora, however, it is admitted by Liebig that a certain proportion of the elements of bile is discoverable in the fæces; and we shall presently see that in them there is another source for the carbon of respiration. In what manner, or in what state, the elements of the bile are reabsorbed and conveyed to the lungs, we are not informed; they must certainly undergo a change previously to their again entering the current of the circulation; since we know that some of the constituents of the secretion are absolute poisons if retained in the blood, and could not, therefore, be re-introduced into it without injury.

The active movements which are so characteristic of the life of the adult carnivora, and which we see them vainly attempting to perform when in captivity, occasion a waste of matter, which affords an ample supply for the respiratory process. In the young animal, however, the movements are not so energetic; the waste (according to our author) is less; and the maintenance of animal heat could not be provided for, unless more carbon were oxidized than has formed a part of the tissues of the body. It is for this purpose that, in the milk which serves for its nutrition, there is not only a supply of azotized matter, the caseine,—but also a store of matter rich in hydrogen and carbon, the buttery and

saccharine portions of it, for the purpose of being oxidized in such a manner as to generate the requisite amount of caloric. We cannot but think, however, that there is here an imperfection in the mode of our author's reasoning, although we agree in the general correctness of its result. Upon his own frequently-repeated statement, the proportion of urea excreted is an accurate measure of the waste of the tissues; and this proportion is, with reference to the bulk of the body, much greater in the child than in the adult, as Lecanu's observations have proved. The facility with which the effects of injuries are repaired is another indication that the processes of disintegration, as well as those of renewal, are active in children; since the reparative process often requires not merely the formation of new parts, but the removal of those that have become effete. We are inclined to believe, therefore, that the change of matter goes on *more*, instead of *less*, rapidly in the young animal than in the adult. But its loss of heat, in consequence of the much larger surface it exposes in proportion to its bulk, is *much* greater than that of the adult; and it is to provide against this, that Nature has superadded to the calorific means possessed by the adult a store of combustible matter for the use of the tender nursling. The question may not seem to all our readers to be important enough to sanction this digression; but we hope we shall not be suspected of a captious spirit in stopping to correct physiological errors, even of a trifling kind, whereever they are likely to be preferred to truth, through the influence of so high an authority.

In the herbivorous animal, the process of nutrition is represented by Liebig as very different from that which has been described in the adult carnivora, and as more nearly resembling that which takes place in the young of the latter group. From the comparative indolence of their habits, the waste of their tissues is far less, and the amount of azotized nutriment which they require to support it diminishes in the same proportion. Hence the quantity of carbon thus set free for the maintenance of the heat of the system, is not nearly sufficient for the purpose. But Nature has provided the requisite supply in their food. Whilst the nitrogenized constituents of the food of the carnivorous animal afford, when they return to the state of dead matter after having passed through the living tissues, all or nearly all the carbon required for respiration, those which are sufficient to maintain the normal structure of the herbivora do not yield above one fifth of the carbon which is consumed for the purpose of generating caloric; but the sugar, starch, and other non-azotized principles taken in as food appear to be made *directly* subservient to this function, without ever becoming part of the living fabric. Hence an animal thus constituted, if restricted to a diet consisting entirely of azotized substances, must be supplied with five times the amount of these that would otherwise be necessary; and the very instructive fact is pointed out by Liebig, that since 15 lbs. of flesh contain no more carbon than 4 lbs. of starch, a savage, with one animal, and an equal weight of starch, could support life for the same length of time, in which another restricted to animal food would require five such animals, in order to procure the carbon necessary for respiration. When confined to animal food, man is made to resemble the carnivora in his nutrition; and as, like them, he derives the carbon for his respiration from

the waste of his tissues, like them he is obliged to expedite that waste by continual activity. Cultivation, therefore, by which a large amount of non-azotized matter is supplied for respiration, is not merely the economy of nutrition, but the economy of force. This statement requires some modification, however, when it is applied to races of men like the Esquimaux, who, though restricted to animal food, are chiefly supported upon that which resembles the non-azotized elements of plants, in its capacity for being at once oxidized, and for thereby sustaining the temperature of the system; for all the fatty constituents of the fabric must be regarded as serving this purpose; and these abound in the animals upon which such races feed. Hence the restless activity which is observable among the hunters of the agile deer or the swift-footed antelope of southern climes, gives place among the inhabitants of the frigid zone to a stupid inertness, which is only occasionally interrupted by the necessity of securing the supply of food afforded by the massive tenants of their seas. Here, as in other instances, the exception adds probability to the rule; and we see, in the kind of food most readily obtainable by the inhabitants of different regions, a beneficient provision of the Creator for their respective wants.

Since the preceding paragraph was written, our attention has been directed to the fact that the portion of Liebig's doctrines, which concerns the direct application of the non-azotized elements of the food to the maintenance of animal heat by a species of combustion, has been long taught in this country, by Dr. Collier. In the surgeon's Report, appended to Sir John Ross's Narrative of his Arctic Voyage, (published in 1835), which Report was written by Dr. Collier from data verbally supplied by the surgeon, we find the following passages.—" If the preservation of a uniform temperature by external means be of the highest importance, it must be admitted that the due and vigorous generation of caloric by a proper selection of food is not less so. The natural food of this climate seems well adapted to the purpose. Every one knows that solar caloric, caloric by combustion, and that generated by animal life, are the three chief sources by which our temperature is sustained. Now, it seems but reasonable that in a region where our supply from the first two is so exceedingly limited, the more active evolution from the last source should compensate for the deficiency..... There are three modes by which heat is probably generated within the body, by the chemical decomposition which takes place in respiration, by the influence of the brain and nervous system, in some degree analogous, perhaps, to its development by galvanic influence, and by the process of digestion and nutrition. If it be acknowledged that combustion goes on more rapidly in cold weather, and that this is wisely pre-ordained, the same remark applies to respiration, in which the imaginative poet and the philosopher alike recognize the resemblance. The heat generated will partly depend on the rapidity of union of the impurities of the blood and the consequent liberation of caloric. *But it will partly depend on the quantity of carbon and hydrogen contained and taken in with the food.* On this ground alone, I expect the patience of my readers; for it will follow, if this be admitted, that such provisions should be selected for these expeditions as may have been found to contain these elements in the largest possible excess, loosely combined, and in the most favor-

able state for elimination. On reference to the food destined by nature for the support of the Esquimaux, we find it almost exclusively hydro-carbonaceous oil, blubber, fish, and flesh, the latter two of which cannot be too fat for them. Here we see a strong analogy between their process of nutrition and that of combustion; nearly the same materials, the same play of affinities, the same results, the same change of latent into sensible caloric. If I am rightly understood, my readers must see that I contend that the gross diet of northern tribes is not a matter of chance, but in harmony with the slow but constant changes which are continually going on around them; and intended to enable them to resist cold, and to vigorously generate heat." (Appendix to Sir J. Ross's Voyage, pp. cxxv-cxxvii.)

We do not think that Liebig would lay any great stress upon this doctrine as peculiar to himself. The point on which he evidently most insists, is the subserviency of the carbon of the bile to the maintenance of animal heat by the respiratory process; as evinced by its re-absorption, and disappearance through that channel. We doubt not that scattered hints could be found, in the writings of chemists and physiologists long anterior to Dr. Collier, respecting the use of the hydro-carbon of the food in maintaining animal heat through respiration; since the analogy of this process to that of combustion has been, as Dr. C. himself remarks, long generally recognized.

The slowness of the metamorphosis of the tissues of the herbivora is considered by Liebig as sufficiently indicated by the very small amount of the alkaline phosphates contained in their urine; since the presence of phosphorus appears essential to the existence of fibrin and albumen as such, and it must therefore be set free when they are decomposed. But they lose much more heat than the carnivora, in consequence of the large amount of perspiratory glands in their skin; and therefore, whilst they require far less azotized matter for their food, they have need of much more carbon and hydrogen. But if the process of cooling and exhalation be checked, as it is in stall-fed animals, the non-azotized constituents, instead of being thrown off by respiration, in the form of carbonic acid and water, are retained in the system, and metamorphosed into *fat*. This transformation is effected simply by the withdrawal of a part of the oxygen; and as the change of the azotized principles into fat would be a much more complex process, it may be safely assumed that this product is formed from sugar, starch, and other substances of the same kind. We have in the bee, as correctly stated by Liebig, a very satisfactory example of the transformation of a saccharine into a fatty body; since it has been demonstrated that wax may be formed, to any amount, when bees are fed upon pure sugar. A deposit of fat will take place, therefore, whenever the amount of carbon in the food is greater than can be carried off by the oxygen introduced by respiration; and this may be due to a superabundance of carbon, or to a deficiency of oxygen. Anything which increases the supply of oxygen, such as bodily exercise, will tend to keep down the accumulation. But in the very production of fat, oxygen is set free in the form of carbonic acid; and this process itself becomes a source of the disengagement of heat.

The doctrine thus expounded enables us to give a much more precise account of many pathological changes which could only be previously

explained in a vague and general manner. Thus, when the respiratory process is permanently interrupted by chronic disease of the lungs, the condition is produced which is favorable to the generation of fatty matter; there seems, however, to be a general condition of the system which is unfavorable to its deposit in the tissues; and the office of separating it is then thrown on the liver, which, as Mr. Bowman has recently shown, has a tendency to present the fatty condition, simply from being called upon to separate an unusual quantity of fatty matter from the blood. Again, in some other diseases, according to Liebig, the starch, sugar, &c., of the food obviously do not undergo the changes which enable them to assist in respiration, and consequently to be converted into fat; thus in diabetes mellitus, the starch is only converted into grape sugar, which is expelled from the body without further change. We are not sure, however, that this explanation of the disease is not too directly chemical; and we are more inclined to take Dr. Prout's view of its nature, in referring it to a disordered condition of the process of decomposition of the tissues at large. The craving appetite, which is so remarkable a symptom in an early stage of it, and which generally continues through its whole course, is a sufficient indication to our minds that the *waste* (as it is termed by Liebig) is going on with unusual rapidity; and the enormous amount of the azotized constituents of food which can be appropriated without bodily exercise, seems to show that there is something wrong in the process of their assimilation, as well as in the treatment of the non-azotized compounds. The influence which the total withdrawal of sugar, starch, &c. from the food has in controlling the disease, is certainly in favour of Liebig's view; but, on the other hand, if it were altogether correct, the formation of sugar ought to cease entirely as soon as the supply of non-azotized matter is stopped, which is certainly not the case; and it ought to bear a constant proportion to the supply of non-azotized matter, which is also far from being true, as the practical physician well knows. We do not believe that we are yet in a condition to understand the real nature of this interesting disease; and we would put our readers on their guard against accepting the dogmas even of a Liebig with respect to it, especially since (as we shall presently find) he entertains a conception which has been *proved* to be erroneous. The practical physician well knows, that the excretion of sugar by the urine,—the point which is naturally most striking to the chemist,—is really but a small part of the whole matter.

This division ends with a few observations on the part performed by gelatine in nutrition. That principle is regarded by Liebig as entirely incapable of affording the means of reparation to the fibrous and albuminous tissues; but he considers that, when taken into the system, it may serve to replace the waste of the gelatinous tissues themselves,—cartilage, bone, cellular membrane, &c. A certain power must be requisite, in the state of health, to form such from the fibrin or albumen of the blood; hence it may be considered as an economy of this power to employ a certain amount of ready-prepared gelatine as food; and this economy will be the more decided, when the body has been enfeebled by sickness. But all experience shows that on gelatine alone the system cannot be supported.

IV. *Metamorphosis of tissues.* Under this head our author combines a large number of highly interesting facts relating to the changes to which the alimentary principles are subservient within the body. He by no means intends to offer a complete history of these changes; but simply to indicate the path of inquiry by which their nature may be elucidated. He commences by pointing out the important fact, that experiment has now demonstrated the existence of numerous compounds of a simply chemical nature, which, with the greatest diversity in external characters, possess the same composition in 100 parts, or even the same absolute amount of equivalents in each element.

"Cyanuric acid, for example, is a nitrogenized compound which crystallizes in beautiful transparent octahedrons, easily soluble in water and in acids, and very permanent. Cyamelide is a second body, absolutely insoluble in water and acids, white and opaque, like porcelain or magnesia. Hydrated cyanic acid is a third compound, which is a liquid, more volatile than pure acetic acid, blisters the skin, and cannot be brought in contact with water without being instantaneously resolved into new products. These three substances not only yield, on analysis, absolutely the same relative weight of the same elements, but they may be converted and reconverted into one another, even in hermetically closed vessels; that is, without the aid of any foreign matter." (p. 104.)

A similar group of three occurs in the case of albumen, fibrin, and casein. When these substances are dissolved in a moderately-strong solution of caustic potash, and the solution is exposed for some time to a high temperature, they are decomposed. The addition of acetic acid to the solution causes in all three the separation of a gelatinous translucent precipitate, which has exactly the same characters and composition, from whichever of the three substances above mentioned it has been obtained. This compound has been termed *protein* by Mulder, its discoverer; and he has found by precise and careful analysis, that it contains exactly the same elements and exactly in the same proportion as the animal matters from which it is prepared; whilst these last may be regarded as compounds of proteine with definite proportions of sulphur and phosphorus. The same product may be obtained from the azotized principles of plants. Hence we may take this as a common foundation or point of departure in investigating the metamorphoses which occur in the animal body; since all its organic nitrogenized constituents, how different soever in composition, may be regarded as formed from proteine by the addition or subtraction of the elements of water or of oxygen, and by resolution into two or more compounds. Of this fact the development of the chick *in ovo* may be considered a sufficient demonstration; since the egg contains nothing but albumen, with a small proportion of fatty matter. The carbon of the latter may partly contribute to form the nerves and brain, of which this element constitutes a large proportion, and is partly thrown off by respiration; whilst the fibrin, blood-globules, muscles, vessels, membranes, feathers, claws, &c. cannot be formed in any other way than from the albumen.

We think it necessary to point out, however, that in various passages of his work, a want of acquaintance is shown by Liebig as to what we think may be considered to be the true relations of albumen and fibrin. The former we regard as a mere chemical compound, incapable of undergoing organization, until its condition has been entirely changed, and

possessing only a granular structure, when it is made to coagulate. The latter, on the other hand, we believe to be the *pabulum*, at the expense of which, not only muscular fibre, but all the tissues of the protein series are immediately formed; it possesses truly vital properties, as evinced by its tendency to assume a regularly-organized form, when it coagulates in a thin layer on a living surface (as in organizable lymph); and it may altogether be considered as the intermediate form between albumen and the living tissues.

The process of chymification is viewed by Liebig as a purely chemical action, analagous to those processes of transformation which were described in his former work as putrefaction, fermentation, and eremacausis. These changes depend upon the influence which an azotized substance, itself in a state of transformation, is capable of exerting upon other matters whose constituents are held together somewhat loosely.

"The clear gastric juice contains a substance in a state of transformation, by the contact of which with those constituents of the food which, by themselves, are insoluble in water, the latter acquire, in virtue of a new grouping of their atoms, the property of dissolving in that fluid................. It can hardly be doubted that this substance is a product of the transformation of the stomach itself. No substances possess in so high a degree as those arising from the progressive decomposition of the tissues containing gelatine or chondrine, the property of exciting a change in the arrangement of the elements of other compounds. When the lining membrane of the stomach of any animal—as, for example, that of the calf—is cleaned by continual washing in water, it produces no effect whatever if brought into contact with a solution of sugar with milk, or other substances. But if the same membrane be exposed for some time to the air, or dried and then placed in contact with the same substances, the sugar is changed according to the state of decomposition of the animal matter, either into lactic acid, into mannite and mucilage, or into alcohol and carbonic acid; while milk is instantly coagulated. An ordinary animal bladder retains, when dry, all its properties unchanged; but when exposed to air and moisture it undergoes a change not indicated by any obvious external signs. If, in this state, it be placed in a solution of sugar of milk, that substance is quickly changed into lactic acid." (p. 110.)

"The fresh lining membrane of the stomach of a calf, digested with weak muriatic acid, gives to this fluid no power of dissolving boiled flesh or coagulated white of egg. But if previously allowed to dry, or if left for a time in water, it then yields, to water acidulated with muriatic acid, a substance in minute quantity, the decomposition of which has already commenced and is completed in the solution. If coagulated albumen be placed in this solution, the state of decomposition is communicated to it, first at the edges, which become translucent, pass into a mucilage, and finally dissolve. The same change gradually affects the whole mass, and at last it is entirely dissolved, with the exception of fatty particles, which render the solution turbid." (p. 111.)

It is scarcely possible, in our opinion, for any demonstration to be clearer; and if anything were wanting to confirm it, it is the fact recently established by the labours of German physiologists, that the whole epithelium of the stomach is thrown off at every meal, thus furnishing the very supply of animal matter in a state of decomposition which is theoretically required. In the action of the gastric juice on the food, the oxygen of the atmosphere and the elements of water take an important share; and it is considered by Liebig that the chief use of the saliva is to entangle and carry down, by means of its peculiar viscidity, a large quantity of air-bubbles to aid in digestion. The nitrogen of the

air thus introduced is believed by him to be taken into the blood, and to be the source of that which is exhaled from the skin and lungs. We cannot but regard this supposition as rather far-fetched, and as founded on somewhat insufficient evidence; still we have no ground for denying it; and any account of the peculiar objects of the salivary secretion has the merit of assigning a purpose for that which is evidently an important function, but which has never yet been sufficiently elucidated.

The exact state and proportion in which sulphur and phosphorus exist in fibrin and albumen has not been exactly determined; and Liebig is not sanguine in his expectations as to the possibility of any such determination; since a variation in the sulphur or phosphorus, smaller in extent than the usual limit of errors of observation, will affect the number of atoms of carbon, hydrogen, or oxygen, to the extent of ten atoms or more. "A formula for protein," he further remarks, "is nothing more than the nearest and most exact expression in equivalents of the result of the best analyses; it is a fact established so far, free from doubt, and this alone is for the present valuable to us." This formula is 48 C, 6 N, 36 H, and 14 O. In albumen the proportionals of sulphur and phosphorus are equal; in fibrin the amount of sulphur is double that of the phosphorus; whilst in casein there is no phosphorus, but only sulphur. The following table represents the chemical alterations to which the protein is subjected in its transformation into various organized tissues:

	Protein.	*Ammonia.*	*Water.*	*Oxygen.*
Fibrin, Albumen	Pr.	. . .	. .	. .
Arterial membrane	Pr.	. . .	+ 2 H.O.	. .
Chondrine .	Pr.	. . .	+ 4 H.O.	+ 2 O.
Hair, Horn .	Pr.	+ $N.H_3$	. .	+ 3 O.
Gelatinous Tissues	2 Pr.	+ 3 $N.H_3$	+ H.O.	+ 7 O.

"From this general statement it appears, that all the tissues of the body contain, for the same amount of carbon, more oxygen than the constituents of the blood. During their formation oxygen also, either from the air or from the elements of water, has been added to the elements of protein. In hair and gelatinous membrane we observe, further, an excess of nitrogen and hydrogen, and that in the proportions to form ammonia.

"Chemists are not yet agreed on the question, in what manner the elements of the sulphate of potash are arranged: it would therefore be going too far, were they to pronounce arterial membrane to be a hydrate of protein; chondrine, a hydrated oxide of protein; and hair and membranes, compounds of ammonia with oxides of protein." (p. 127.)

The greatest difficulty in the comprehension of these transformations is connected with the production of gelatinous and horny substances. To imagine that, in their formation, ammonia as well as oxygen and water are added to the protein, appears a very improbable supposition; and the idea that they are elaborated from protein chiefly by a disengagement of certain equivalents of carbon and hydrogen, appears much more consistent with the established fact, that gelatine and its allied principles are entirely removed from the compounds of the protein series, and never can be reconverted into them. That a separation of carbon takes place in the conversion of fibrin or albumen into gelatine, was long ago suggested by Dr. Prout; and he indicated this process, which must be continually going on, as one source of the carbon which is set

free in the lungs by the function of respiration—an idea which would now seem to require modification.

If the view propounded by Liebig with regard to the respiratory process—that the carbon and hydrogen set free by it in an oxidized form are rather derived from the reabsorption of bile, or from the non-azotized portion of the food, than immediately from the decomposition of the tissues—should prove correct, we should expect that the two principal excretions, the bile and urine, would contain nearly all the products of the waste of the system ; and that the sum of their constituents should be equivalent to the sum of the elements of the living flesh, or of the circulating blood, which has been well designated as liquid flesh (*chair coulante*). However ill founded such an expectation might seem, the analyses of Liebig and his associates leave us no room for doubt on the subject; and to his general theory we can scarcely refuse our assent, although when it comes to be applied to other classes of animals, we apprehend that it will need considerable modification. Thus in insects, whose respiration is so peculiarly energetic when they are in a state of activity, we find but the merest traces of a biliary apparatus; and the mode in which atmospheric air is introduced into the minutest portions of the body, together with the extraordinary increase in the quantity of carbon thrown off during violent muscular movement, leads us to think that in this class very nearly the whole carbon thrown off by respiration is at once derived from the decomposition of the tissues, and does not pass through the biliary apparatus at all. On the other hand, the mollusca and crustacea, which agree in aquatic residence, and in inertness of habits, agree also in possessing an immense liver, combined with a very feeble respiratory apparatus; and we can scarcely avoid the belief that in them more of the carbonaceous matter of the bile passes off with the excrements, than in warm-blooded animals, where it is needed for the support of the temperature.

The analyses of Playfair and Boeckmann give for flesh (fibrin, albumen, cellular tissue, and nerves,) and for blood, as the most exact expression of their numerical results, one and the same formula, namely, 48 C, 6 N, 39 H, 15 O. This may be called the empirical formula of blood. Now the most characteristic ingredient of bile appears to be a substance of an acid nature, termed *choleic* acid, which is capable of being readily converted into other forms (such as *choloidic* acid) by the operation of various agents. The results of its analysis are best expressed by the following empirical formula; 76 C, 2 N, 66 H, 22 O. Thus we see that a predominance of carbon is the peculiar character of the biliary secretion. Now if we add to half this formula the elements of the urate of ammonia, which is the characteristic ingredient of the urine of serpents, we get as the sum the following formula: 48 C, 6 N, 40 H, 17 O, which is exactly that of blood, with the addition of one equivalent of water and one of oxygen—the very elements which we have independent reason to know are added in the course of the transformations in question. In the higher classes of animals uric acid is no longer found in the urine, but is replaced by urea. This disappearance of uric acid and production of urea plainly stand in very close relation to the quantity of oxygen absorbed in respiration and to the quantity of water consumed in a given time; for when uric acid is subjected to the action of oxygen

it is at first resolved, as is well known, into urea and alloxan; whilst a further supply of oxygen acting on the alloxan causes it to resolve itself into urea and either oxalic or carbonic acid, the latter being formed when there is most oxygen. Hence it is remarked by Liebig, that the tendency to the formation of calculi consisting of uric acid will be greatest in those in whom, from want of exercise, the supply of oxygen is deficient; if in such cases the amount of exercise be increased, oxalates will be formed instead of urates—a result which he affirms to be conformable to experience. With a still greater supply of oxygen the same elements would have yielded in healthy subjects only the last product of the oxidation of uric acid, namely, carbonic acid and urea. The action of oxygen upon uric acid is favoured by the ingestion of a large quantity of water, by means of which the sparingly soluble uric acid is kept dissolved, so that the inspired oxygen can act upon it: in birds, which drink but little, uric acid predominates in the urine, in spite of their active respiration. In the urine of the horse when at rest, hippuric acid is found; and this is converted, by increased oxygenation, into benzoate of ammonia and carbonic acid as soon as the animal is compelled to labour. In the fœtal calf the urine contains a peculiar product—allantoin; and Liebig points out that two proportionals of protein, with the addition only of two atoms of water, contain the elements of six proportionals of allantoin and one of choloidic acid, which seems to be the chief constituent of the meconium. These six atoms of allantoin contain the elements of two atoms of uric acid, two atoms of urea, and two atoms of water. So that in the fœtal calf, as in the former instances, the urinary and biliary secretions together account for those elements which were once united in the protein of the tissues.

Such comparisons further lead to a curious view of the connexion between the formation of these secretions, and the metamorphoses of protein into gelatine. For if we suppose one of three atoms of protein to be decomposed into its nitrogenized products urea and uric acid, and the non-azotized choloidic acid, and consider the latter to be withdrawn, whilst the former remains united with the other two atoms of protein, we obtain a formula closely approaching to the composition of gelatinous tissues. In regard to the speculations as to the connexion between the secretion of bile and the formation of gelatine, to which we might be thus conducted, our author expresses himself with philosophic caution:

"We must, however, attach to such formula, and to the considerations arising from them, no more importance than justly belongs to them. I would constantly remind the reader that their use is to serve as points of connexion, which may enable us to acquire more accurate views as to the production and decomposition of those compounds which form the animal tissues. They are the first attempts to discover the path which we must follow in order to attain the object of our researches; and this object, the goal we strive to reach is, and must be, attainable. The experience of all those who have occupied themselves with researches into natural phenomena leads to this general result—that these phenomena are caused, or produced, by means far more simple than was previously supposed, or than we even now imagine; and it is precisely their simplicity which should most powerfully excite our wonder and admiration." (p. 143.)

The preceding remarks on the bile have reference only to the Carnivora, in which alone its whole solid matter can be traced to the metamorphoses

of tissues; in the Herbivora there seems good reason to believe, that a large part of the non-azotized compounds which abound in it are derived immediately from the corresponding principles of their food. Thus it has recently been pointed out by Berzelius (though we do not find the fact adverted to by Liebig) that the green colouring matter of the bile of ruminants seems identical with the chlorophyll of the herbage on which they feed. The transformation of the animal tissues will be probably influenced by the presence of such other substances; just as, according to the interesting experiments of Dr. A. Ure, the administration of benzoic acid causes the usual constituents of the urine to be replaced by hippuric acid. Thus Liebig shows that if the elements of starch and protein, oxygen and water being also present, undergo transformation together and mutually affect each other, we obtain, as the products of this metamorphosis, urea, choleic acid, ammonia, and carbonic acid, and besides these no other product whatever. He then goes on to point out some very curious relations between the azotized constituents of the bile and those of the urine; which in his opinion show that the azotized products of the transformation of tissues in the herbivora do not, as in carnivora, reach the kidneys immediately or directly; but that, before their expulsion from the body in the form of urine, they take a share in certain other processes, especially in the formation of the bile; after which they are reintroduced into the circulation, and are not finally carried out of it by the urine until they have undergone their final metamorphosis. There would seem to be in the herbivora a sort of economy of azotized matter, arising out of the small degree of waste of their tissues, and the small amount of azotized principles which are taken into the system to replace this. The opinion that the non-azotized compounds of the food at once pass off by the various channels of excretion, especially the liver, is regarded by Liebig as confirmed by the fact that not a trace of starch or sugar has been detected in arterial blood, not even in animals which had been fed exclusively with these substances; but we are inclined to question the truth of this assertion, since it is succeeded by one that has been proved to be erroneous,—the total absence of sugar in the blood of diabetic patients. It is well known that the presence of sugar in diabetic blood has been of late established, not only by chemical tests, but by the perhaps still surer agency of polarized light.

v. *Influence of medicinal agents on vital transformations.* The statement of Dr. A. Ure, already referred to, respecting the influence of benzoic acid taken into the system in producing hippuric acid, has received satisfactory confirmation from the experiments of M. Keller, in the laboratory of Professor Wohler at Göttingen, as well as in this country by Mr. Garrod. These experiments are considered by Liebig as placing beyond all doubt the fact "that a non-azotized substance taken in the food can take a share, by means of its elements, in the act of transformation of the animal tissues, and in the formation of a secretion." If this principle be admitted, it at once affords us a key to the *modus operandi* of a great number of remedies; and, although the details of their action have not yet been investigated, it is a great thing to know in what direction to look for them. We consider this portion of Liebig's work as the one which is of the highest value to the scientific pathologist, although extremely scanty in its details. In fact, the purpose of

the author is evidently to give but examples of the kind of investigation which he proposes; examples, however, which are in themselves of the greatest interest, as well as of the highest promise. Reverting to the speculations which he has previously offered as to the sources of the biliary secretion in the herbivorous animals, he shows that the presence of a nitrogenized compound is essential to the formation of that fluid; and that this compound, like the other constituents, may be obtained directly from the food, instead of being produced by the metamorphosis of tissues. Now, it is among the most curious results of late analyses that *theine*, the peculiar principle of tea, should prove to be identical in composition with *caffeine*, the active principle of coffee; and that both these should contain the same proportions of carbon and nitrogen as exist in *taurine*, the azotized principle of bile, so as to be convertible into it by the addition of water and oxygen. The same may be said of asparagine, the characteristic principle of asparagus and althæa; and by a similar addition of water and oxygen to theobromine, the characteristic principle of the cocoa bean, it will yield the elements of taurine and urea, of taurine, carbonic acid, and ammonia,—or of taurine and uric acid. However small the quantity of solid matter contained in an infusion of tea or coffee, still it will be sufficient to form a corresponding amount of the azotized ingredient of bile, without which those ingredients that constitute by far the largest portion of the secretion will not be separated. In the natural condition of man such artificial aids are not required. But it cannot be denied that,—

"In the case of an excess of non-azotized food, and a deficiency of motion (which is required to cause the change of matter in the tissues, and thus to yield the nitrogenized product which enters into the composition of bile), the health may be benefited by the use of compounds which are capable of supplying the place of the nitrogenized substance produced in the healthy state of the body, and essential to the production of an important element of respiration. In a chemical sense,—and it is this alone which the preceding remarks are intended to show,—caffeine or theine, asparagine, and theobromine are, in virtue of their composition, better adapted to this purpose than all other nitrogenized vegetable principles. The action of these substances, in ordinary circumstances, is not obvious, but it unquestionably exists." (p. 182.)

Passing from these to the truly medicinal agents, it is remarked that these may be subdivided into three groups: the first (including the metallic poisons) consists of substances which enter into direct chemical combination with certain parts or constituents of the body, while the vital force is insufficient to destroy the compounds thus formed; the second, consisting of the essential oils, camphor, empyreumatic substances, antiseptics, &c., possesses the property of impeding or retarding those kinds of transformation to which certain very complex organic molecules are liable, transformations which, when they take place out of the body, are usually designated by the terms fermentation and putrefaction; the third division is composed of bodies the elements of which take a direct share in the changes going on in the animal body, although not furnishing materials for the formation of blood, and therefore not, strictly speaking, nutritive in their character. It is with this last group alone that we are at present concerned; and the example just given affords a beautiful illustration of the mode in which an important process of *secretion* may be influenced by the ingestion of substances which cannot

be converted into blood. There is no reason why the *nutrition* of certain parts which differ widely from the protein compounds in the proportion of their elements, should not be influenced in like manner; and this would seem by far the most probable view of the *modus operandi* of certain medicines, which have a specific and direct effect upon the nervous system. The nitrogenized vegetable principles, whose composition differs from that of the proper nitrogenized elements of nutrition also produced by the vegetable organism, are distinguished, beyond all others, for their powerful action on the animal economy. The effects of these substances are singularly varied; from the mildest form of the action of aloes, to that most terrible poison, strychnia, we observe an endless variety of different actions. It is asserted by Liebig that no substance devoid of nitrogen possesses a poisonous action in a similar dose; and he states that from this suggestion M. Francis was led to examine more accurately the composition of picrotoxine, the active principle of cocculus indicus, and to discover in it the presence of nitrogen which had been previously overlooked. It is certainly a most valuable test of the validity of a law, that it is able to correct observations which appeared to contradict it. Now the primary operation of these powerful agents is universally referred to the nervous system.

"This action is commonly said to be dynamic, that is, it accelerates, or retards, or alters in some way the phenomena of motion in animal life. If we reflect that this action is exerted by substances that are material, tangible, and ponderable—that they disappear in the organism—that a double dose acts more powerfully than a single one—that after a time a fresh dose must be given if we wish to produce the action a second time; all these considerations, viewed chemically, permit only one form of explanation; the supposition, namely, that these compounds, by means of their elements, take a share in the formation of new, or the transformation of existing brain and nervous matter.

"However strange the idea may at first sight appear, that the alkaloids of opium or of cinchona bark, the elements of codeine, morphia, quinine, &c., may be converted into constituents of brain and nervous matter, into organs of vital energy, from which the organic motions of the body derive their origin; that these substances form a constituent of that matter, by the removal of which the seat of intellectual life, of sensation, and of consciousness is annihilated; it is, nevertheless, certain that all these forms of power and activity are most closely dependent, not only on the existence, but also on a certain quality of the substance of the brain, spinal marrow, and nerves; insomuch that all the manifestations of the life or vital energy of these modifications of nervous matter, which are recognised as the phenomena of motion, sensation, or feeling, assume another form, as soon as their composition is altered. The animal organism has produced the brain and nerves out of compounds furnished to it by vegetables; it is the constituents of the food of the animal which, in consequence of a series of changes, have assumed the properties and the structure which we find in the brain and nerves." (p. 183.)

It is not, then, more improbable that the formation of the nervous matter should be influenced by the ingestion of azotized vegetable principles of one kind, than that it should be entirely dependent, as we know that it normally is, on the ingestion of other principles furnished by the same kingdom. But why, it may be asked, should we seek for such an influence rather in the nervous tissue than in any other? The reply furnished by Liebig is pregnant with meaning. Of all the tissues in the body, the matter of the nerves is that to which these powerful principles

bear the nearest resemblance in chemical composition. We have seen that vegetable fibrin, albumen, and casein are transformed into muscle and other fibrous tissues, because there is no essential difference in their chemical composition. We have seen that the starch and sugar of plants are transformed, under certain conditions, into the fat of animals; the required chemical change being one easily comprehended. Now, the peculiar matter of the nervous tissue is of a nature chemically intermediate between protein and fat; for it is a fatty acid, containing a small proportion of nitrogen, and must be formed either by the separation of a highly-azotized compound from the elements of the blood, or by the combination of a nitrogenized product of the vital process with a non-azotized compound, probably a fatty body. This process of conversion is probably not a simple one, but consists of several stages, in each of which a protein-compound undergoes a certain alteration. It is easy to conceive, then, that an azotized principle introduced from without may take the place of these altered compounds; just as we have reason to believe that the ingestion of ready-prepared gelatine may supersede the necessity of its formation in the system, or that caffein may supply the nitrogenized constituent required for the bile. And thus we have what is, to say the least, a feasible explanation of the powerful action of minute quantities of these vegetable principles upon the nervous system.

"It would serve no purpose to give these considerations a greater extension at present. However hypothetical they may appear, they only deserve attention in so far as they point out the way which chemistry pursues, and which she ought not to quit, if she would really be of service to physiology and pathology. The combinations of the chemist relate to the change of matter, forwards and backwards, to the conversion of food into the various tissues and secretions, and to their metamorphosis into lifeless compounds; his investigations ought to tell us what has taken place and what can take place in the body. It is singular that we find medicinal agencies all dependent on certain matters, which differ in composition; and if, by the introduction of a substance, certain abnormal conditions are rendered normal, it will be impossible to reject the opinion that this phenomenon depends on a change in the composition of the constituents of the diseased organism, a change in which the elements of the remedy take a share; a share similar to that which the vegetable elements of food have taken in the formation of fat, of membranes, of the saliva, of the seminal fluid, &c. Their carbon, hydrogen, or nitrogen, or whatever else belongs to their composition, are derived from the vegetable organism; and after all, the action and effects of quinine, morphia, and the vegetable poisons in general, are no hypotheses.

"Thus, as we may say in a certain sense, of caffeine, or theine and asparagine, &c., as well as of the non-azotized elements of the food, that they are food for the liver, since they contain the elements by the presence of which that organ is enabled to perform its functions, so we may consider these nitrogenized compounds, so remarkable for their action on the brain, and on the substance of the organs of motion, as elements of food for the organs as yet unknown, which are destined for the metamorphosis of the constituents of the blood into nervous substance and brain. Such organs there must be in the animal body; and if, in the diseased state, an abnormal process of production or transformation of the constituents of cerebral and nervous matter has been established; if, in the organs intended for this purpose, the power of forming that matter out of the constituents of the blood, or the power of resisting an abnormal degree of activity in its decomposition or transformation, has been diminished; then, in a chemical sense, there is no objection to the opinion, that substances of a composition analogous to that of nervous and cerebral matter, and consequently

adapted to form that matter, may be employed, instead of the substances produced from the blood, either to furnish the necessary resistance, or to restore the normal condition." (pp. 188-9.)

It is a strong confirmation of any doctrine, to find several inquirers arriving at the same result by entirely different paths of investigation. Thus we have seen that Liebig has been led, by inferential evidence of an entirely chemical nature, to the conclusion that poisonous and remedial agents exert a specific influence over particular tissues, by modifying the processes which normally take place in them, in virtue of a certain chemical relation subsisting between the agent on the one hand, and the tissue or some of its products on the other. The recent experiments of Mr. Blake on the action of poisons lead to analogous results; for he has given strong reason to believe that all poisons, save those which have an immediate and violent local operation (such as the mineral acids) are absorbed into the blood, and carried to some particular tissue on which they have a specific effect; the time which is required for the influence of oxalic acid on the heart, or strychnia on the spinal cord, being strictly proportional in different animals to the time required for the conveyance of other agents along the same course. Again, the very same doctrine, expressed almost in the words of Liebig, was propounded in a paper read some months since in the Medico-Chirurgical Society (to appear, we understand, in the forthcoming volume of the Transactions) by Dr. W. Budd; in which the author brings together a number of facts regarding the local action of remedies, and the *symmetrical* changes produced in the body by many diseases, in proof of the opinion, that all such local actions must be the result of the presence of the agents in the circulating blood, and of an "elective affinity" between each and the tissue or organ on which it operates. The views of Liebig render more precise our ideas of this local action; and take from Dr. Budd's supposition the uncertain character of an hypothesis, conferring upon it the definiteness of a theory.

VI. *Theory of Disease.* Our limits prevent us from adding more than a very few observations on the two remaining divisions of the work before us. We the less regret this, since it appears to us that our author's theory of disease is the least successful part of it. He attempts to reduce to one general expression that which we believe no real pathologist would regard as capable of such generalization; although the "unity of disease" may be a very specious dogma for a practitioner to trumpet forth, who wishes to attract public attention; and the curability of all maladies by one nostrum may well serve the purposes of the empiric. As the normal or physiological state of *health* depends on an immense variety of conditions, so an alteration in any one of these conditions will produce a pathological state—that of *disease;* and we do not see that we are much aided in our conception of it by any such definition of it as the following: "Disease occurs when the sum of vital force, which tends to neutralize all causes of disturbance (in other words, when the resistance offered by the vital force) is weaker than the acting cause of disturbance." In pursuing this idea into its details, it appears to us that our author's want of acquaintance with scientific pathology renders him inadequate to the object he has proposed; and we may especially advert to his explanation of a *febrile paroxysm,* as exhibiting an utter ignorance of the nature of that disturbance.

vii. The *Theory of Respiration*, with which the text concludes, appears to us to contain little novelty. The chief point on which the author dwells, is the agency of the globules in conveying oxygen from the lungs to the systemic capillaries, and in bringing back carbonic acid from these to be discharged at the lungs, an idea which has long been familiar to physiologists. He considers this to be the exclusive function of the globules, affirming that physiology has proved that the globules take no share in the function of nutrition; in which statement Dr. Barry would consider him as somewhat premature. He brings forward, however, an extremely interesting view of the function of the *iron*, which is well known to exist in the blood-corpuscles. If it be regarded as originally in the state of protoxide, it will be converted into peroxide by exposure to atmospheric air in the lungs; in the systemic capillaries it will give off half of its oxygen, and be reduced to the state of protoxide; this protoxide will combine with the carbonic acid set free in the same situation, and the iron will return to the lungs in the state of carbonate. In the lungs it will change its equivalent of carbonic acid for one of oxygen, which will be conveyed to the tissues as before. Now although the fibrin and albumen of venous blood undergo a change by exposure to air in the lungs, and undergo a converse change in the systemic capillaries, it cannot be questioned that the chief alteration takes place in the contents of the blood-corpuscles; and the idea of Liebig as to the nature of that change appears to us peculiarly satisfactory. It fully accounts for the phenomenon; since the result of analyses and calculations proves that the amount of iron present in the blood, if in a state of protoxide, is sufficient to furnish the means of carrying or transporting twice as much carbonic acid, as can possibly be formed by the oxygen absorbed in the lungs. It also explains other well-known facts, in regard to the asphyxiating influence of minute quantities of certain gases and vapours, such as sulphuretted hydrogen and prussic acid, of which no other account has been offered; since these agents have a powerful effect upon iron, when alkalies are present; and free alkali is never absent in the blood.

The latter part of the volume is occupied by a copious appendix, containing a vast amount of important analytical evidence, referred to in the body of the work.

In conclusion, we have only to add to the observations with which we commenced, our tribute of praise to the able translator, Dr. W. Gregory, for the admirable manner in which he has placed this most important treatise before the English reader; and our renewed recommendation to all who desire to share in the honour of themselves advancing science, or who merely wish the credit of keeping pace with it, to "read, mark, learn, and inwardly digest" its valuable contents.

Art. XVII.

Observations on Life, as the Cause of the Vital Phenomena.—London, 1842. 8vo, pp. 16.

This little pamphlet is intended, as we learn from its conclusion, but as the herald of a larger work, which, though nearly ready for publication, its author has been obliged to abandon for the present. The brochure has two claims on our attention: first, on account of the source whence

it is understood to originate; second, as containing a new, and what will be considered by many a plausible, view of the nature of life, which professes to be founded upon experiments to be detailed. This view we shall present to our readers in the author's own language:

"He believes that the following inferences may now be considered as physiological facts:

"First, That there are a thousand things done in the body that cannot possibly be done by the mind, or by any other agent that resides in the brain. There must, therefore, be some other cause to produce these effects, otherwise the body could not be organized, neither could a single movement be made in the animal frame.

"Second, That there is in the living body, independently of the mind, an internal vital agent, which is endowed with an innate power of action; and that this vital agent, or in other words LIFE, is the direct cause of every movement, both voluntary and involuntary, that is made in the living animal.

"Third, That in man, and all the higher order of animals, the great solar ganglion is the seat of life; and the ganglionic nerves, which have their insertion in, and derive their origin from, this central organ, are the nervous machinery with which it works. The principle of action which resides in the solar ganglion has many motor nerves at its command, by which it performs its own voluntary movements; but the great sympathetic nerves, or the internal spinal cords, are the true motor tracts." (pp. 5-6.)

Now, in regard to the first of these propositions, we imagine that there can be no difference of opinion; since the old Stahlian doctrine, that *the soul* is concerned in all the operations of the organic system has been long since abandoned. But we need scarcely tell our readers that we utterly dissent from the second; and that consequently the third cannot in our opinion be true. We have on many occasions pointed out the absence of necessity for any such hypothesis as that offered by our author, in the explanation of the phenomena of the living organized body; and have shown that in the *microcosmus*, no less than in the *macrocosmus*, all sound philosophy would lead us to refer the phenomena which come under our notice, to *laws* dependent on the *properties* with which matter was originally endowed by its Creator. If in the one we require the hypothesis of a distinct entity or "organic agent,"—whether endowed, as Dr. Prout supposes, with faculties "little short of intelligence," or as our author more boldly states with "innate intelligence,"—we do in the other also. Intelligence implies consciousness. Does our author mean that this agent is conscious of its operations? In that case every animal must have within it two conscious entities and separate volitions—his mind, and his life; although his own consciousness only leads him to infer the presence of one. There seems to us to be no end to the absurd contradictions into which the assumption of any such doctrine must lead us. Our author attempts to get rid of these, however, by a kind of inversion of the Stahlian hypothesis; for, instead of considering the various operations of organic life as a sort of branch-office of the soul, he regards the mind itself as only one manifestation of his "vital agent."

"Life," he says, "has its faculties as well as the mind; and the mind is but one of the many faculties of the internal vital agent. The mind is only a part; life, or the vital entity, is the whole. It is the mind that enables the vital entity to keep up a connexion with the external world. It is the mind to which the vital agent is indebted for all its acquired knowledge. Selective absorption,

digestion, circulation, assimilation, organization, respiration, secretion, reparation, and reproduction, are all vital properties. Sensibility is common to both the vital and mental powers; but even this is decidedly a vital property. Consciousness, memory, thought, reason, judgment, and volition, are mental powers, but these powers are merely superadded to life. In one word, the mind in man is the mind of the soul." (pp. 13-14.)

We do not think it necessary for us to expose the jumble of ideas contained in this quotation; the looseness of the author's mode of thinking is indicated by his designation of the *actions* of digestion, circulation, respiration, &c. as *properties.* No one can hope to reason successfully on abstract subjects like these, who does not rigidly adhere to the correct use of terms.

The only illustration given of the mode in which the vital agent, residing in the "solar ganglia," (we presume the author means either the semilunar ganglia or the solar plexus,) may be regarded as operating on the body, has reference to muscular movement; by the consideration of which the author states himself to have been led to the views he has adopted. The following is the assumption on which he founds his argument:

"As the mind is not endowed with any innate knowledge of the organized structures, it is very evident that it cannot be the direct cause of any of the muscular movements that are made in the living body. When the mind wills a movement to be made, it may give the stimulus of volition; and the movement is instantly made in obedience to the will. But the movement is not made by the mind, for it can only will, it cannot act or regulate the actions of the muscular fibres. In the involuntary movements it is very clear that the mind cannot be the motor power, and we may rest assured that there is only one principle of action in the animal frame. Even the voluntary actions cannot be made by the mind; for the mind of itself can no more select the proper nerves or move a muscle than it can move a mountain." (p. 7.)

Now on this we have only to remark that, fully admitting our ignorance of the mode in which the will operates upon the nervous system so as to produce muscular motion, we cannot see anything more mysterious about it than in the converse operation, by which a change propagated through a nerve renders the mind conscious of certain sensations; and that neither in the one case nor in the other do we see that any assistance is to be derived from the interposition of a new agent between the mind and the nerve. This vital agent we suppose is considered by its upholder as immaterial; and the difficulty of explaining *its* action on the nerve is, therefore, just as great as in the case of the mind.

The power of the cerebro-spinal system of nerves to excite either voluntary or automatic movements, is entirely derived, according to our author, from its connexion with the sympathetic system; this being, in his estimation, the guide of *all* the actions that take place in the body. Certainly no physiologist, within our knowledge at least, has assigned to it so eminent a function; and until we have some better evidence for this opinion than that at present before us, we must take leave to withhold our assent. We cannot say that we are prepossessed in favour of our author's views, when we find them partly based upon the idea a hundred times refuted, that the ganglionic system of the lower animals is analogous to the sympathetic system of vertebrata; or when we find him stating that "even in the human subject, sensation and the power of

motion may continue in the lower extremities, even when the spinal cord has been completely severed from its centre." *Were this true*, he would certainly have some ground for his inference, that "in these cases the muscles in the lower extremities could only be made to obey the will of the mind by some vital power in the ganglionic system." (p. 10.)

The very numerous instances in which experimental physiologists have seriously injured or entirely removed the semilunar ganglia, without the occurrence of any marked results, might have sufficed, we should imagine, to prove the fallacy of the whole theory. In fact we can scarcely suppose that our physiological readers will think it deserving of a serious refutation. The author's total misapprehension of the nature of the reflex function of the spinal cord, which may now take rank as one of the best-established facts in physiology, is shown by the following remark:

"Dr. M. Hall attributes the respiratory movements, when the cerebrum and cerebellum are removed, in mammalia, principally to the reflex action of the pneumogastric nerves. This is evidently an error; for, as the pneumogastric has neither its origin nor termination in, or any connexion with, either the diaphragm or the intercostal muscles, it cannot possibly, under any circumstances, be the direct cause of the respiratory movements." (p. 12, note.)

It is scarcely necessary for us to point out the author's singular mistake in attributing to Dr. M. Hall any such anatomical error; the well-known idea of that physiologist being, that the pneumogastric is the chief *excitor* of the respiratory movements, conveying to the medulla oblongata the stimulus originating in the lungs, which produces a reflex motor impulse along the phrenic and intercostal nerves. It is from this mistake that our author cannot understand why, on Dr. M. Hall's system, the respiratory movements of mammalia should come to a stand, or nearly so, when the cerebrum and cerebellum are removed, and the pneumogastrics and spinal accessory divided. He asks—"Now, if the medulla oblongata or the spinal cord has any inherent power, or if either be the seat of the principle of action, why is it, that under these circumstances neither the one nor both of them can continue the respiratory movements?" No physiologist, unless it be Sir C. Bell, has ever attributed to them any such inherent power; the necessity for a *stimulus* to their action, conveyed by the pneumogastrics from the lungs, having been long recognized. Our author's inference, that the section of the pneumogastric and spinal accessory operates, by cutting off the communication between the sympathetic system and the medulla oblongata, falls, therefore, completely to the ground.

We almost feel that we owe an apology to our readers for detaining them so long upon such a production as this; but we have not commented upon a tenth part of the inaccuracies and unsupported assumptions it contains. To the author, for whom we have great personal respect, we have only to say that it has given us great regret to be obliged to adopt so severe a tone; but we assure him that with us, as with him, truth, not system, is our aim; and that whenever he may give to the world the data on which his views are founded, they shall receive from us an attentive and impartial consideration. In the mean time we would recommend him to devote what leisure he can spare from the "active pursuits of agricultural life," in which he informs us that he is engaged, to making himself acquainted with the actual state of knowledge on the physiology of the nervous system.

PART SECOND.

Bibliographical Notices.

Art. I.—*The Nervous System and its Functions.* By Herbert Mayo, F.R.S., Senior-surgeon of the Middlesex Hospital, &c.—*London*, 1842. 12mo, pp. 182.

This little treatise was originally intended as a companion to a new edition of the author's Engravings of the Brain; but after commencing it, he thought it preferable to publish it separately in an expanded form. It is characterized by the clearness and precision which mark all the author's statements; and which, when brought to bear upon abstract truths, renders his exposition of them so lucid and valuable. This is especially the case in the introductory remarks on the relation of vitality to mind; the whole of which we should be glad to quote, as expressing a great deal of profound thought in a form in which elegance and conciseness are remarkably united. The following we consider as a fair specimen of the whole:

"Life is a force so contrived and used as to qualify the materials of the inert world for a temporary union with consciousness; a means how mind may enter into such relations with matter, that it may have its being and part in physical nature, and its faculties developed, and its capabilities and tendencies drawn out and proved (for whatever ulterior purpose) in subjection to, and in harmony with her laws. As we imagine the Supreme Mind to be ubiquitous, infinite, controlling but uncontrolled by matter, so, in contrast with these attributes, we conceive the finite mind to be bound down to place, and to be dependent on a certain arrangement of matter for its manifestation—each power displayed as the property of a tissue, each agency as the function of an organ. These views do not lead to materialism. For one cannot disjoin the physiology of the nervous system from mental philosophy, nor investigate the play of its organs without attending to the mind itself. And if equal consideration is given to the two classes of phenomena, it is impossible (so, at least, it appears to myself) to avoid the conviction that they are essentially independent the one of the other, and belong to distinct essences; and that *ipseity*—the consciousness of personal being—is not a mode of material existence, nor physical impenetrability an attribute of that which feels and thinks." (pp. 6-7.)

We are sorry that we cannot view the rest of the work as favorably. We do not find fault with its form and style, for we consider this peculiarly well adapted to its purpose. The author has arranged his materials in the form of isolated propositions, to each of which is appended a demonstration or illustration; and, when taken in connexion with each other, these propositions alone give a concise view of the doctrines he professes. What we regret to have so continually forced on our notice is that peculiar form of ipseity, termed egotism, which seems to be a besetting sin too common amongst scientific men, leading them to set a disproportionate value on their own achievements, and to under-estimate those of every one else. Yet we should not impute it to *all;* for the

most distinguished men of every age have been those most willing to award to others the merit which was justly due to them.

Any one who is acquainted with the history of discovery in the nervous system during the last twenty-two years, must be well aware that no one is likely to arrive at an accurate estimate of what has been actually accomplished, who does not go into the investigation with an entire absence of all party spirit, and with a thorough desire to seek truth wherever it may be found. That *we* have endeavoured to pursue this course, we think our readers have had ample evidence. How far Mr. Mayo is free from prejudice, may be judged from the following sentence, which, as far as we can perceive, is the only reference to Dr. Marshall Hall throughout the whole of this treatise on the nervous system: "Dr. M. Hall has given the good name of reflex function to this circle of impression and action, and has added one or two interesting facts in additional illustration of the principle." (p. 50.) We too clearly perceive, also, the same desire to deprive Sir C. Bell of as much merit as possible, not only in his own favour, but in that of Magendie, on which we commented with regret on a former occasion. Mr. Mayo still maintains, in spite of what we believe has been generally felt to be the clear exposition of the question which we gave in our Ninth Volume (p. 131), that Sir C. Bell, in his paper of 1821, "had as yet made no advance towards the conception of different nerves being appropriated to sense and motion;" and that the establishment of the true doctrine of the functions of the nerves of the face is due to himself, and that of the nerves of the spine to Magendie. Thus we find him stating that Magendie, in his paper of 1822 in the Journal de Physiologie, "incontrovertibly established that the anterior roots of the spinal nerves minister to voluntary motion *exclusively*, the posterior to sensation;" a doctrine of which almost the very reverse was maintained by Magendie for many years afterwards, (See Vol. IX. p. 134.) It is due to Mr. Mayo, however, that, as we formerly inserted Mr. A. Shaw's statement of the relation which once subsisted between Sir C. Bell and himself, we should now give place to his own. Which is the more correct, we shall leave those to decide who are specially interested in this piece of personality. After speaking of Sir C. Bell's essay privately distributed in 1811, he continues:

"It was a step of eminent service to science then, and again later in his career of investigation, to set on foot new inquiries into the functions of the nerves, and to show that they might be elucidated by new experiments. With the researches of Sir Charles Bell, made in this his first essay, and up to this point, I had the advantage of being familiar, having been his pupil in the years 1812, 1813, 1814, and 1815. After this I was not in the way of knowing, nor did I know anything whatever of the nature of Sir Charles Bell's researches, except through his published writings." (p. 37.)

How far this statement is consistent with the acknowledgment of assistance derived from Mr. Mayo, in three of Mr. J. Shaw's papers published in 1821 and 1822, we cannot clearly perceive.

We shall not dwell upon the scientific portion of Mr. Mayo's treatise, for the simple reason that to do so would require a great deal of unprofitable discussion, the minds of physiologists being too much made up in regard to certain leading doctrines, to be disturbed by the dogmatic and often contradictory assertions of Mr. Mayo. We may simply say

that the same inconsistencies which we formerly pointed out in his doctrines (Vol. V. p. 534,) still remain, and are even rendered more palpable. Thus we find him allowing in one place that the movements in the limbs of a decapitated frog do not depend upon sensation, (pp. 20-1 ;) whilst he afterwards adduces actions of a similar kind to prove that sensibility and even voluntary power exist in the spinal cord, provided its connexion with the segment of the medulla oblongata, from which the fifth pair arises, be entire. The actions of acephalous infants he considers as being thus directed : and again, in a subsequent page (54) he enunciates the proposition that "in the entire and living frame, each segment of the cranio-spinal cord, with its nerves, constitutes the whole apparatus requisite and sufficient for sensation being felt, and the impulses to muscular motion, both reflex and conscious, originated." To reconcile these and many other contradictions so completely surpasses *our* ability, that we must leave the question in the hands of our readers.

ART. II.—*The Education of Mothers of Families; or, the Civilization of the Human Race, by Women.* By M. AIMÉ-MARTIN. Translated by EDWIN LEE, Esq.—*London*, 1842. 8vo, pp. 384.

WE not long since perused Dr. Cerise's work, entitled "Des Fonctions et des Maladies Nerveuses dans leurs rapports avec l'Education Sociale et Privée, Morale et Physique," a work to which Mr. Lee, in his appendix to the above translation, makes frequent reference; and although,—from the general nature of its disquisitions, we did not think proper, at the time, to notice it in our pages; yet as a highly useful and scientific ethical treatise, and as embodying many philosophical and important views of education, we were prepared to recommend it to all persons interesting themselves in the subject of which it professed to treat. The same reasons which prevented us from noticing Dr. Cerise's work, applies to the one before us, which, however, is an extremely interesting and valuable production ; being that to which the prize of the French Academy was assigned.

The object of the work, as its title implies, is to show that it is through MOTHERS that society is to be regenerated. Seeing the influence which the maternal, more than the paternal, example and precept exercise over children, as proved in the lives of many celebrated persons, it is proposed to improve the education of mothers or women, in order to ensure a corresponding improvement in that of the rising race. Such views are not new, but were insisted on by various moralists of ancient Greece and Rome. And, although we fear that no education, whether applied to mothers or children, will radically remove all the ills and errors of this world, yet we cannot but strongly recommend a scheme so innocent and plausible as this, to the consideration and countenance of every philanthropist.

The translation seems well executed ; the work forms pleasing reading; and Mr. Lee has appended to it a useful and sensible treatise on "the prevailing methods of education, and their influence on health and happiness." We recommend the volume as at once instructive and entertaining.

Art. III.—*An Introductory Lecture on Pictorial Anatomy, delivered to the Students of the School of Design of the Honorable the Commissioners of the Board of Trustees for the Encouragement of Scottish Manufacturers.* By James Miller, F.R.S.E. F.R.C.S.E.—*Edinburgh*, 1842. 8vo, pp. 32.

Mr. Miller's Lecture, which has been published at the request of the Board of Trustees for the encouragement of Scottish manufactures, is an eloquent vindication of the utility of anatomy, as applied to the plastic arts, conceived in good taste, and on the whole most judiciously executed. Were we to suggest a defect, it would be that Mr. Miller in demonstrating the *use* has neglected to warn the artist against the *abuse* of anatomical knowledge; a caution by no means superfluous, when we recollect that such artists as Donatello, John of Bologna, and Fuseli made shipwreck on this shoal, and that Buonarotti himself might have left even a greater name, had he made less prominent display of his anatomical knowledge. Anatomy is but one of the many auxiliaries requisite for the painter and sculptor, and none of them perhaps require more cautious and skilful treatment, greater judgment, or more chastened taste to enable the artist to fully avail himself of their help, without falling into exaggerations, almost as painful, in an artistic sense, as deformity itself. Art is most triumphant when its efforts are least conspicuous, and of all the resources of art, a knowledge of anatomy, we mean the anatomy of external form, is at once the most indispensable, and that which should be the least ostentatiously displayed, and the most carefully subdued, so that its influence should be felt rather than seen.

Mr. Miller's observations on the utility of anatomy in depicting the more minute details of the human form, are very happy and judicious. He instances the "Elgin marbles, where the details of the subordinate parts, the loose hanging folds of the skin, the veins under the belly or on the sides of the horses, more or less swelled as the animal is more or less in action, are given with scrupulous exactness, almost resembling casts taken from the life. And yet who ventures to say that these are not replete with beauty and grandeur?" John Bell, (Observations on Italy, p. 330,) himself a master in anatomical science and pictorial art, but no very warm advocate of anatomical sculpture and painting, has noticed a beautiful example of this anatomy of detail in the dying gladiator, perhaps, take it all in all, the most glorious and exquisite relic of ancient art. He says: "No affectation of anatomy here, not a muscle to be distinguished, a full fleshy skin covers all the body, yet the general forms perfect as if they were expressed. The only anatomical feature discernible is that of full turgid veins, yet not ostentatiously protruded, but seen slightly along the front of the arms and ancles, giving, like the clotted hair, proof of violent exertion."

But we must conclude, or the fascination of the subject, rendered still more seductive by the admirable manner in which Mr. Miller has handled it, will lead us to transgress all reasonable limits. Mr. Miller is evidently endowed with a vivid but pure sense of artistic beauty, and every lover of the fine arts must peruse his lecture with both pleasure and advantage.

Art. IV.—*Die Lehre von der Zurechnungsfähigkeit bei Zweifelhaften Gemüthszuständen für Aerzte und Juristen praktisch dargestellt* Von Dr. Adolph Schnitzer.—*Berlin*, 1840, pp. 372.

The Doctrine of Legal Responsibility in reference to doubtful or disordered States of the Mind, practically elucidated for the use of physicians and jurists. By Dr. Adolph Schnitzer.—*Berlin*, 1840.

The press of Germany has of late years yielded many works similar to that of which we are here about to give a brief notice. In our Thirteenth Number (Jan. 1839, p. 129) we had occasion to review a treatise by Professor Jörg, of Leipsic, on Legal Responsibility as it applies to females in the pregnant and parturient states. Dr. Schnitzer's work is of a more extended character: it includes not merely these conditions, but all those disordered states of the mental faculties which are usually comprised under the head of Insanity.

We find but little that is practical before reaching the third chapter. Here the author enters fully into the characters of puberty in the two sexes, with the morbid psychological changes which often accompany the development of the sexual organs. The fourth chapter is entirely devoted to the subject of pyromania, as it has been called by Marc and others. In Dr. Schnitzer's view, young persons between the ages of ten and twenty-four years, are especially liable to that disordered condition of mind which leads to acts of incendiarism. We do not, however, find that his remarks possess any originality: a great part of the chapter is made up of quotations from Henke and other well-known medico-legal writers. For the present opinions of medical jurists on this subject, we must refer to what has been said in reviewing the work of Jörg (No. XIII. p. 130,) and more recently in the notice of Professor Marc's treatise on Insanity. (No. XIX. p. 170.)

We next have a considerable space occupied by an account of responsibility for illegal acts committed by females while in a state of pregnancy, in which the author appears to follow closely the views of Dr. Jörg. The signs of pregnancy are detailed, in order to show that there is a sufficient want of certainty about them to justify a medical jurist in admitting that, in some cases, females pregnant for the first time may be wholly unconscious of their condition up to the time of parturition. (p. 120.)

In regard to the proofs of abortion, we find nothing more than is commonly stated in most works on midwifery. The subject of responsibility in the parturient state is in great part derived from Jörg's treatise. Dr. Schnitzer, however, differs from that author in regard to his views on the fœtus. Dr. Jörg adopts the eccentric view, that the human fœtus is only a higher species of intestinal worm, that it has not human attributes, and is not therefore a subject for protection by human laws. We have already strongly condemned this doctrine, as unjust and immoral, (B. and F. Med. Rev., No. XIII. p. 133,) and as one which, if acted upon, would lead to the common practice of abortion.

Dr. Schnitzer censures the views put forth by Jörg, as likely to be subversive of a just administration of the law. As to the objection of that author, that the fœtus does not possess human attributes, because its functions are dormant, and it does not exercise the senses, he observes, the same objection would apply with equal force to a man lying in a

state of lethargy, or to one struck by apoplexy or paralysis. If a theory of this kind is to be admitted, it is obvious, as the author remarks (p.370), that infanticide is no longer homicide; and that there can be no greater crime in killing a child than in destroying any domestic animal. Moralists and legislators do not commonly divide murder into different degrees, according to the mental and bodily development of the person killed: but on Dr. Jörg's principle, the killing of a cretin would be a less punishable offence than the killing of a healthy well-developed man. Indeed, to carry this doctrine to its full extent, the crime of murder should always vary in its degree, and therefore in its punishment, according to the intellectual and corporeal development of the party killed!

Dr. Schnitzer in pursuing the subject makes use of much the same arguments as those employed by us in the notice referred to. They are such as would at once occur to most persons who reflected upon the nature and consequences of Dr. Jörg's theory. The sophistical basis on which they rest is too evident to deceive any reader possessed of ordinary acumen. Nevertheless we are not sorry to have met with a German author well fitted to oppose so pernicious a doctrine, emanating from one of his own countrymen, and a physician of no mean reputation.

On the causes of the death of a child during and after delivery, as well as on its "viability" or capability of living, we feel it unnecessary to make any remarks, since these subjects have been recently noticed in this Journal.

The second division of Dr. Schnitzer's book is occupied by an outline of the different forms of insanity. The greater part of this section appears to be compiled from other writers. This circumstance renders it unnecessary for us to occupy our space with quotations. We can find nothing to add to what has already been said on this subject in lately reviewing the works of several modern writers. (B. and F. Med. Rev., No. XIX. July, 1841, p. 129.) We shall only observe, with regard to the treatise before us, that the selections appear to have been made with judgment, and the language is clear and free from mysticism. Those who require merely an abstract of the opinions of modern German authorities on the subject of insanity, will find here what they desire. The author has contrived to weave together a medico-legal description of subjects which are commonly kept quite distinct among English writers; namely, pregnancy, delivery, abortion, infanticide, and insanity; and we must admit that he has shown great skill and ingenuity in blending together such heterogeneous materials.

ART. V.—*De Methodi Endermaticæ Ratione nec non applicatione.* Auctore FERDINANDO SCHUBERT, Philosophiæ et Medicinæ Doctore. —*Schaffnaburghi*, 1841, pp. 80.

The Endermic Method, theoretically and practically considered. By FERDINAND SCHUBERT, Ph. & M.D.—*Schaffhausen*, 1841.

THIS treatise purports to give the result of two years' observation of this plan of treatment, and to fill up to a certain degree the imperfections existing in this branch of medicine. The preface is devoted to a brief outline of those who have employed the endermic method in various

countries. The conditions under which this treatment can be employed with advantage are, according to the author, numerous, but from the account of the practical employment of the various remedies, this mode of treatment appears in many cases to be attended with much more pain and suffering than their internal employment.

"Symptoms accompanying the endermic application of medicines. The immediate effect of the removal of the cuticle is an irritation of the nerves of the part from the exposure to the atmosphere. The action of solid substances when uniform in their composition, depends on their roughness of surface; when dissimilar in composition on their chemical action. The immediate effect produced, which is frequently attended with great pain, is termed the local or primary effect, as distinguished from the general effect, which soon succeeds. This primary effect consists most commonly in a sensation of the most intense heat in the part, like that of burning coals, a feeling of creeping or crawling in the part, with shooting and tearing pains, the cutis subsequently inflames, suppurates, and sometimes ulcerates. . . . . In a shorter or longer period, according to the nature of the application, effects are produced in the remote organs, which are called the secondary effects, and are more or less directly connected with the primary effect. These effects resemble in their kind those resulting from the internal administration of the same medicine, and are materially increased by its application near the diseased part." (p. 33.)

The second part contains illustrations of the effects of the various medical agents, which have been employed endermically. The part devoted to the minerals is very brief, and contains very little new, and the small quantity of information derived from various sources is not of a very complete or practical nature. The part devoted to the medicines of the vegetable kingdom is more full and contains a good summary from various practitioners of the endermic method. Some of the medicines are said to have been found useful in the most serious diseases, as carcinoma and diabetes, but paralytic and nervous affections appear, from the statements of the author and the observations of others, to have been the forms of disease in which most benefit resulted from their employment.

ART. VI.—*Analekten für Frauenkrankheiten, etc.—Leipsig*, 1837-41. 8vo, 10 Hefte.

A Selection from the most esteemed Treatises, Monographs, Prize Essays, Dissertations, and Cases illustrating the Diseases of Women, and the Peculiarities of Pregnancy and the Puerperal State.—Leipsic, 1837-41. 10 Numbers.

The title of the work explains its nature, which is analogous to that of the Selections on Children's Diseases, of which a notice recently appeared in this Journal. The papers it contains are generally well chosen, and among them are several very valuable essays on puerperal fever by continental and British writers. After our recent review of Heim and Kiwisch, however, we will not again enter on the subject of puerperal fever, and a great number of the other papers being translated from the English would present little that is novel to our readers. The work, however, which is not yet completed, will be found a valuable addition to an obstetric library.

Art. VII.—*Memoir of the late James Hope*, m.d., *Physician to St. George's Hospital, &c. &c.* By Mrs. Hope. Edited by Klein Grant, m.d. &c. &c.—*London*, 1842. 8vo, pp. 358.

We have already given, in our obituary, so full a biographical notice of Dr. Hope, that it is unnecessary for us to dwell on the events of his life as recorded in the volume before us, nor is it indeed within our province to enter minutely into the details of personal history, however worthy its object.

This memoir is one of great interest to the religious, the general, and the medical reader. As a religious production it would be wholly out of place for us to comment upon it further than to express our respect for the sentiments it contains, and our wish that it may be instrumental in advancing the cause of practical Christianity. In a professional and general light we would chiefly draw attention to it, as an illustration of the power of perseverance and concentration of the mind on a particular object; for to these it appears to us that the great and early success of Dr. Hope in his profession was mainly attributable. At home or abroad, in the cheerfulness of success or the gloom of disappointment, he kept in constant view the object of a laudable ambition—the attaining a high rank in his profession; and he continued, from an early period of his studies, to accumulate observations on a very difficult and important subject, till he produced the best work upon it that has yet appeared in our language. His discharge of all public duties evinced the same undeviating perseverance, and a conscientious desire of fulfilling to the utmost the trust reposed in him.

While his example in these respects may be held up as worthy of all imitation, his premature death may serve as a warning against excessive exertion and overstraining of the mental and bodily powers—a warning not to be neglected in an age when the difficulty of rising in the learned professions is so great that many men of high promise fall victims in early life to excess of labour. Dr. Hope's exertions at St. George's Hospital were truly immense, and the able and conscientious manner in which he fulfilled his functions demands for him, in full measure, the respect of his profession and the gratitude of the public, while it is impossible to repress a feeling of deep regret that his labours in such a field of usefulness should have been the principal exciting cause of the disease which proved fatal to him.

The memoir is extremely creditable to the talents, good sense, and good taste of its author, Mrs. Hope; and the editor, Dr. Grant, seems to have discharged his share of the task with much judgment and with a degree of prudence and reserve which editors of others' writings are not always disposed to show. In conclusion, we warmly recommend this work not only to our own readers but to the reading public generally. It will be found highly interesting in the perusal, and will leave no feeling of disappointment in the mind, having reference to the author, the editor, or the lamented subject of it.

Art. VIII.—*A Treatise on Irritation of the Spinal Nerves as the source of Nervousness, Indigestion, functional and organic Derangements of the principal Organs of the Body, and on the modifying influence of Temperament and Habits of Man over Diseases, and their importance as regards conducting successfully the Treatment of the latter; and on the Therapeutic Use of Water.* By J. Evans Riadore, m.d. f.l.s. &c. &c.—*London*, 1842. 8vo, pp. 306.

Anything more contemptible, more utterly devoid of interest and value, than the work, whose prolix title we have just copied, it has seldom been our misfortune to peruse. We trust our readers will give us due credit for a large share of patience, when we inform them that we have waded through the entire mass of rubbish, lest perchance any concealed jewel, worthy to be exposed to day, might lurk unseen: but all in vain; with the exception of sundry quotations from well-known and standard authorities, there is nothing in the whole 306 pages which is deserving of attention, much less of praise.

But while we find nothing to commend there is abundance to condemn. The author's neglect of grammar and ignorance of the very simplest rules of composition are no less marvellous as a fact than they are disgraceful in a member of a learned profession. We shall give a few specimens: "Some anatomists have considered that the twenty-four bones and joints forming the spinal column *is* too strongly fixed together to be dislocated." (p. 3.) "Pains in the abdomen and considerable distention of it by flatus *is* a common consequence." (p. 10.) "Extreme violence of treatment, *of* which there is a disposition to run *into*, in the present age," &c. (p. 45.) "This temperament when *unduly*, or in a morbid condition, is often increased by sedentary habits." (p. 57.) "The *bowels* of a patient of the nervous temperament *is* subject," &c. (p. 63.) "No inconvenient *symptoms follows*." (p. 76.) Enough for false syntax. The orthography is equally brilliant: The prostate gland is, with one single exception, universally spelt *prostrate*. *Scybelœ* and *scybela*, *morphin*, *lumber* for lumbar, *dyspectic*, *bicarbon of potash*, *sura scapula* (supra-scapular) nerve, *gangliœ*, *manupulation*, *sphinctars*, &c. diversify and adorn these luminous pages; and granting that some of them are mere typographical errors, what shall be said for the carelessness of an author who could overlook such glaring faults?

But even the plea of carelessness, miserable as it is, will scarcely hold good in all instances. Can it be from a mistake of the printer that "*alkaline*" is made a substantive? That we are informed (p. 56) that "*atrophy*, engrafted upon original asthenic or feeble constitution, *will generally die* in consequence of obstruction of the mesenteric glands?" That we hear of an infant "*suckling* a foster-mother?" (p. 26.) And that poor Sir Astley Cooper is said to have recommended "*suckling* to be protracted, as being more *nutricious* and *easier of digestion* than the food usually provided afterwards." (p. 66.)

In the prosecution of our wearisome task, we had marked numerous passages for quotation, as illustrative of the peculiarly lucid style which characterizes this most philosophical production; but we shall spare our readers the infliction, condemning them to the perusal of two only, which are selected because with them are associated the names of two very eminent individuals, who, we trust, will feel themselves duly honoured by Dr. Riadore's notice:

"The *laws* of sympathy, which have always been observed by practitioners, *is* in effect the theory arrived at and named by Dr. Hall *excito-motory system*, which he divided into two sets of nerves, the incident and reflex." (p. 118.)

"That the stomach and the urinary organs are frequently secondarily affected, and not, as Dr. Prout, in his work on Stomach and Urinary Diseases, attributes to malassimulation as the effect of primary cause engendered in the stomach to derange it and the kidneys functionally or organically, is rendered still more probable *if confirmed as a truth*, by the fact that indigestion is cured by applications to the spine, as well as affections of the kidneys," &c. (p. 230.)

Literary delinquencies of a character so flagrant as the above it was manifestly impossible to overlook; yet might we have touched them with a somewhat lighter hand, had the work been redeemed by anything in the slightest degree valuable in pathology or practice. That such is not the case is, however, but too certain. The one object of the author seems to be to establish his favorite theory, that almost every ill that flesh is heir to may be traced to that vague indefinite something, denominated "spinal irritation."

Dr. Riadore is strong in dietetics. "Our mode of thinking," he tells us, "and even *the form of religion and degree of attention paid to its rites*, are under the control of the diet!" And yet he says in another place that "drunkards generally digest their food well." But we beg pardon for having so long detained our readers by the exposition of such absurdities. We feel it a sort of degradation that such a book as this *could* emanate from a titled member of our profession.

Art. IX.—*Traité des Maladies des Femmes qui déterminent des Fleurs Blanches, des Leucorrhées, ou tout autre Ecoulement Utéro-Vaginal.* Par Henry Blatin et V. Nivet.—*Paris*, 1842. 8vo, pp. 632.

A Treatise on those Diseases of Women which occasion Leucorrhœa or other Utero-vaginal Discharges. By H. Blatin and V. Nivet.—*Paris*, 1842. 8vo, pp. 632.

The authors of this work have proposed to themselves to continue and complete the *Traité du Catarrhe Utérin* of their relative and predecessor, J. B. Blatin. They have accordingly divided the diseases of which they treat into two classes, describing under the head of idiopathic discharges all those affections in which the mucous membrane only of the genital organs is affected, while all cases in which the morbid secretion results from change of structure in some more deeply-seated part, or from ulcerations or the existence of tumours or presence of foreign bodies, are referred to the head of symptomatic discharges. For the second part of the work, with the exception of a chapter on vesicular polypi, which has already appeared in the Archives Générales de Médecine, the writers do not claim any higher merit than that of conscientious compilers. In the former half of the book, which treats of idiopathic leucorrhœa, they do, however, lay claim to greater originality, though acknowledging their obligations to J. B. Blatin as well as to subsequent writers. Their acknowledgments are nevertheless by no means so full as they ought to have been; and after going through the tedious task of collating the two books with each other, we cannot help remarking that the present ought to be regarded rather as a reprint of Blatin's treatise with notes and additions than as an independent work, the result of original observation. The chapters on the nomenclature and classification of the disease, on

its causes and on the effects of its suppression, are little else than transcripts from J. B. Blatin's treatise, and in many places the authors have quietly appropriated to themselves the results of their predecessor's learning, quoting cases and opinions, and giving references to the original writers instead of to Blatin, from whose magazine of materials they have borrowed much of their erudition.

Taking the book, however, at the lowest estimate, and regarding it merely as a compilation, it is not without interest, presenting an epitome of the present state of knowledge in France, concerning a class of diseases which have been but comparatively little elucidated in this country. MM. Blatin and Nivet appear to be well acquainted with all the writings of their countrymen on uterine diseases; and notwithstanding a want of precision in their subdivisions and a wearisome prolixity of style, they have produced a work creditable to their diligence and sound practical common sense. When next we meet with them we trust that we may find them in the character of original observers rather than as the retailers of the opinions of others.

ART. X.—G. VROLIK *ueber eine Vollkommene Verwachsung der Gelenke an den Kreuz-Darm-und Schaambeinen ohne vorhergegangene krankhafte Beschaffenheit.—Amsterdam*, 1841. Mit zwei Kupfertafeln. Fol., pp. 7.

On a complete Anchylosis of the Sacro-Iliac and Pubic Articulations, without any previous morbid condition. By G. VROLIK.—*Amsterdam.*

Two very rare cases are here mentioned. In one the symphysis pubis was completely anchylosed, with the exception of a small portion at its lower and posterior part, which was still filled up by ligament. The adjacent parts of the ossa innominata presented irregular surfaces, covered with short stalactitic osseous deposits, which sufficiently indicated that, in this case, the anchylosis was the result of long-continued inflammation of the bones and of the articulation between them.

In the other and more remarkable case these signs of previous disease are entirely absent. The situation of the symphysis pubis is occupied by healthy cancellous tissue with a small cavity in the centre; and the surfaces over both this symphysis and those between the sacrum and ilia are smooth and evenly-continuous with those of the adjacent bones. In short, there is no trace of the ordinary effects of diseases of bones or joints, except the osseous anchylosis of all the latter. In this respect the case is, we believe, unique; nor does it seem explicable except by regarding it as the result of defective original formation. For the natural anchylosis of certain joints, such as the cranial sutures and those between the portions of the sternum and sacrum, takes place according to a fixed rule, and therefore no such exceptional anchylosis as this can be referred to the same category with them. Neither can it be explained by any process of extra-uterine disease; for we know of none which produces osseous anchylosis without leaving many other signs of its effects. Congenital anchylosis, on the contrary, by whatever circumstances it may have been produced, seldom, if ever, presents any sign of disease of the adjacent parts.

The history of these cases is unfortunately unknown. The pelves are both from adult and, to all appearance, well-formed women.

ART. XI.—*The Biographical Dictionary of the Society for Diffusing Useful Knowledge.* Vol. I. Part I. A . . —*London*, 1842. 8vo, pp. 440.

WE were induced to look into this new Dictionary by the hope of finding the *medical lives* more carefully written than we had found to be the case in other works of a somewhat similar kind; and we were not disappointed. All the lives of physicians and surgeons which we have examined are composed in a simple and unaffected style, and contain, in short compass, all the more material facts connected with the individual, and a correct list of his writings. The lives of the ancient physicians display great learning and research on the part of the author; and we doubt not but that most of our readers will be introduced, in his pages, to the acquaintance of not a few professional brethren heretofore unknown to them. The modern lives are compiled with equal care, and comprehend the names of almost every man, in every country, who has been in any way distinguished in medical science or practice. We strongly recommend this work to the members of our profession as one which, when complete, will contain a fuller and truer history of the lives and writings of medical men than any other that has yet appeared; and we believe we may conscientiously recommend it to the members of all other professions on the same grounds of superiority.

ART. XII.—*A Bedside Manual of Physical Diagnosis.* Second Edition. By CHARLES COWAN, M.D., Physician to the Reading Hospital.—*London*, 1842. 12mo, pp. 99.

WE noticed the first edition of this little work with much commendation, (Vol. III. p. 184.) It is considerably improved in its present form, yet not enlarged beyond its proper sphere of a pocket or bedside helper. It ought to be in the possession of all young auscultators.

ART. XIII.—*Pulmonary Consumption: its prevention and cure established on New Views of the Pathology of that Disease.* By HENRY GILBERT, M.R.C.S.—*London*, 1842. 8vo, pp. 296.

WITH the modesty commonly characteristic of the possessors of genius, Mr. Henry Gilbert commences his preface by "some apology" for adding another to the books already existing on the subject of pulmonary consumption. To us, who have read the volume, such apology is eminently needless; we hail with delight chapters as purely logical as they are profoundly meditative; especially as an under-current of sly humour (to which we are particularly partial) runs through them and leaves the critic "nothing to desire." The generous manner of noticing his inferiors in mental qualification and in fame is equally admirable and almost peculiar to Mr. Henry Gilbert; whether he kindly and patronisingly refers to "my highly talented friend, W. Farr, Esq.," gives occasional hint of the existence of such a person as "the ingenious Louis," or applies the rod of criticism to Sir James Clark, the *suaviter in modo*, and the placid condescension of a higher order of intelligence, alike conspicuously shine forth.

In the first chapter, that on the statistics of consumption, Mr. Gilbert refers to the difficulty which has hitherto existed in determining the influence of climate, quotes the conflicting opinions of writers on the point,

and having thus strongly excited curiosity, gratifies it by the following luminous and decisive solution of that which previously had been little more than an unanswered riddle: "*My* opinion is, that till such time as the seeds of consumption are absorbed by the lacteals, climate can have no influence whatever in producing the disease, and it matters not whether it be hot or cold, moist or dry, changeable or regular." (p. 25.) This is rendered perfectly clear by Mr. Gilbert's remark in explanation: "The farmer is well aware that a warm temperature, with a moderate quantity of rain, encourages and favours the growth of corn, when once the seed is deposited in the earth; but he would never expect a favorable climate to raise grain till such time as the seed was sown." Of course.

It is interesting to know that "although phthisis is very prevalent in some warm countries," Mr. Gilbert "cannot assign heat as the cause, being clearly convinced, by the pathology of the disease, that it cannot be induced by this agent." It is likewise advisable the reader should be made aware of the author's "full conviction that a moist or damp atmosphere tends very materially to the relief of the phthisical, and that the disease is comparatively rare under such circumstances." Adopting Dr. Wells's notion, that "ague and consumption rarely occupy the same district," Mr. Gilbert gives the proposition all the energy—and truth—of poetry, asserting that "ague is the *daughter of humidity*, but over consumption she *rides rough-shod.*"

The influence of trades of various kinds on the development of consumption meets with investigation at Mr. Gilbert's hands. The most important point we find among the novelties of the section is the statement that Mr. Gilbert has not, "to the best of his recollection, seen a case of consumption among the trade of butchers for two years." Although we might have hesitated to credit Woolcombe, Young, &c. on this point, of the exemption of this class of subjects from the disease no doubt can henceforth be entertained.

The second chapter, that on the pathology of consumption, bears the old motto, "Felix qui potuit rerum cognoscere causas," which derives new life in its applicability to our author. Early in his inquiry Mr. Gilbert finds motive for lament in the infrequency with which we hear "of a confirmed case of pulmonary phthisis radically cured;" and in the fact that every case is commonly seen to fail "till nothing but a shadow remains for the cold earth to claim." He goes on to affirm, "it is reasonable to believe that there must be some cause for so universal a want of success," (p. 72;) in other words, where an effect exists there must be a cause. As it is to be presumed, however, that a change will come over the face of things, when Mr. Gilbert's pathology of the disease has become generally known, we hasten to impart this to the reader. The lacteals, according to him, absorb unorganizable matter, which circulates with the blood and is deposited in the lungs as tubercle. There can be no possible doubt of this, for it appears that "Dr. Ayre" tells us "plainly and distinctly that diseased mesenteric glands occur in children from the acrid condition of the duodenal contents;" and the author "*therefore sees* that unorganizable matter may be absorbed by the lacteals." The keenness of Mr. Gilbert's vision is as enviable as his reasoning is logical. Indeed "the force of reasoning could no further go:" the pathology of consumption is henceforth in the unvexed category of Q. E. D.

Art. XIV.—*A Collection of Plates illustrating the Theory and Practice of Midwifery; accompanied by an Explanatory Text.* Translated from the French of Em. Debout, m.d.—*London*, 1841. Folio.

We notice this work merely on account of its demerits. The plates which illustrate it, though stated to be "designed by the author himself with great care," are with few exceptions unacknowledged copies from Hunter, Smellie, or other well-known originals. They are very indifferently executed, and the text which accompanies them is evidently the production of some ignorant compiler. Thus, for instance, the inclination of the pelvis is stated to be 45° instead of 60°, mollities ossium is spoken of as a peculiar disease described by Burns, and a direct communication is asserted to exist between the maternal and fœtal vessels in the placenta. The translation has been executed by some foreigner but ill acquainted with our language, and the book contains many typographical errors.

Art. XV.—*An Essay on Diabetes.* By H. Bell, d.m.p., one of the Librarians of the Faculty of Medicine of Paris. Translated by Alfred Markwick.—*London*, 1842. 8vo, pp. 96.

Although we have not had an opportunity of comparing Mr. Markwick's translation of Dr. Bell's little Essay on Diabetes with the original, we are so well pleased with the clear, concise, and compendious sketch which Mr. Markwick has supplied, as to be disposed to believe that he has falsified the assertion that "Every one suffers by a *translation* excepting a *bishop*." He has, in fact, given us a taste of, or a reference to all the honeyed water that has been known to flow for the last 2000 years and upwards, beginning with Hippocrates, b. c. 400, and hunting down to Mr. Smith, a. d. 1841; for in a small work of only ninety-six pages, teeming throughout with citations and references, five of them consist of a closely-printed enumeration of "those works which have appeared to him useful to consult, and capable of throwing light on some parts of the history of diabetes." Mr. Markwick has, in short, furnished us with a hive of diabetic honey, collected from a multiplicity of select literary flowers. *Sic vos non vobis mellificatis!*

Notwithstanding the researches of the most able physicians of this and of other countries, it is much to be regretted that no satisfactory information on the nature and mystery of the disease in question has hitherto been communicated. We have looked through all that Mr. Markwick has laid before us, in the anxious hope of discovering some mitigation of this opprobrium, but in vain. The theories which have appeared the most plausible have been briefly stated; the chemical analysis of the urine in the different species of diabetes has been given; the symptoms which characterize the disease have been defined; and the most successful modes of treatment enumerated; but, as our author observes:

> "All the hypotheses made on the nature of diabetes are still unsatisfactory; because the subtlety of mind is not sufficient by itself to discover the truth, and because we are still too ignorant of several points in the history of this disease to be able to find out its proximate cause." (p. 52.)

We closed the pithy little essay before us, therefore, with only the satisfaction of having refreshed our memory by a perusal of all that has

hitherto been advanced as to the etiology, pathology, and therapeutics of diabetes, but without acquiring any additional information as to the mode of curing it. As, however, our experience in the methodus medendi entirely coincides with that of Dr. Bell, we shall quote his translator's own words:

"We have seen that the animal regimen, opium, warm-baths, and blood-letting were the most efficacious remedies in relieving the severest symptoms of diabetes. It is always useful to combine them together, and not to trust to one only. Thus, opium, animal diet, and warm-baths have always succeeded, in Dr. Bardsley's hands, if not in curing, at least, in greatly alleviating the symptoms. Others add tonics and astringents, when the stomach can bear them. It is only in those cases where these means prove useless that we are authorized in having recourse to others of certainly much less efficacy." (p. 60.)

ART. XVI.—CONRADI JOANNIS MARTINI LANGENBECK, *Icones Anatomicæ.—Gottingæ*, 1842. Sumptibus Auctoris.

C. J. M. LANGENBECK'S *Anatomical Plates.—Göttingen*, 1842.

THESE plates have been drawn and engraved from dissections made by the celebrated Langenbeck of Göttingen, or under his direction. They are divided into eight *fasciculi*, three of which are occupied with the anatomy of the nervous system, and two with that of the vascular system. The viscera, muscles, and bones and ligaments occupy the remaining three; together with three plates representing the anatomy of the operations performed for ligature of the arteries. These last, and the plates exhibiting the anatomy of the muscles and bones and ligaments, are lithographs.

We have looked over these anatomical plates and can testify to their general accuracy. Having been engraved by different hands, they are, however, of unequal merit. The three neurological *fasciculi* contain engravings which, for novelty, beauty, and accuracy, are unrivalled. The drawing, engraving, printing, and paper are all unexceptionably good. There are sixty-four plates in these three fasciculi, containing nearly 130 figures. They exhibit the nervous system in all its relations, in all its ramifications, and in every possible point of view. Sixty-two of these figures are devoted solely to the anatomy of the cerebrum and cerebellum, and show several sections of these organs quite new to us. Those who desire to possess a thorough knowledge of the anatomy of the encephalon and of the complicated relations of its various parts, will find these plates invaluable. The successive course of the fibrils through the cerebral ganglia and their distribution in the cerebellum are admirably delineated. These neurological plates could scarcely be considered dear at half the price of the whole set. The anatomy of the blood-vessels is shown in a rather stiff style, and some of the engravers have not possessed the skill of the artist employed on the neurological drawings. However, they are generally accurate, and that is no slight merit. The engravings representing the anatomy of the eye and ear deserve special mention; and those of the heart are tolerably good.

We cannot speak favorably of the printing of the lithographs. Several of the prints in our possession have been struck off in a very slovenly and unworkmanlike manner, and the paper is of inferior quality. Nor can we compliment Messrs. "Siedentopf und Sohn" of Göttingen on their skill as copper-plate printers, or on their taste. It is surprising

that Professor Langenbeck should have allowed those persons to disfigure his plates by scrawling their own euphonious names on them in the way they have.

The plates correspond in size to those of Tiedemann's. They would certainly have been more convenient if got up in a more portable form. As a whole, they will add to Professor Langenbeck's high reputation and cannot fail to be valued by both student and practitioner.

ART. XVII.—*The Anatomy of the Urinary Bladder and Perinæum of the Male.* Illustrated by Engravings. *With Physiological, Pathological, and Surgical Observations.* By ALEXANDER MONRO, M.D. F.R.S.E. &c., Professor of Anatomy in the University of Edinburgh, &c. —*Edinburgh*, 1842. 8vo, pp. 90.

WE have read with becoming care a great part of the above work, and though we willingly admit that the Professor has redeemed the pledge put forth in his title-page, and that we have found but little to criticise, yet we have risen from our task with the old query of—*cui bono?* Was the volume (if such we may term it) which we have been perusing wanted? Has it filled a vacuum or superseded other similar works? However, these are questions which only a small number of authors can answer satisfactorily in the affirmative—it is a test too searching for a vast proportion of the books which issue from our modern press, and therefore one to which we would not desire to bring the production before us; it must stand or fall, as others do, on its own individual merits.

The plan and object of Dr. Monro's treatise are similar to those of Mr. Morton's work, which we had occasion to notice in a preceding Number of our Journal, (Vol. VIII. p. 244.) The anatomical descriptions are sufficiently long without being tedious, and the practical remarks on the various operations and their results are for the most part good, without however, we should say, possessing the peculiar character which familiarity with practice is alone enabled to impart. Indeed in some few examples this fault might mislead students: for instance, in the section devoted to retention of urine and the operation of puncturing the bladder, no notice is taken of the frequent consequence of obstinate stricture, viz. extravasation of urine, or of the ordinary method now adopted for its relief by division of the stricture in the perineum, an operation which possesses the double advantage of being palliative and curative, and yields in importance to no one operation on the urinary organs; but these are faults of omission.

The descriptions are in many parts graphic, and in some instances somewhat antiquated, as is also the matter. The references are numerous, and many of them to authors well-nigh forgotten in modern surgery. The woodcuts do not rise above mediocrity. Yet on the whole, we do not deny that the student and young practitioner may meet with many valuable hints in Dr. Monro's treatise; and certainly with many quoted opinions which would occupy much of his time to collect for himself.

PART THIRD.

Selections from the British and Foreign Journals.

I. THE FOREIGN JOURNALS.

ANATOMY AND PHYSIOLOGY.

On the minute Structure of the Spleen. By M. Bourgery.

In an injected spleen the discernible anatomical elements of its structure are ten in number: 1, vesicular membranes; 2, blood-vessels; 3, floating vascular corpuscles; 4, a granulo-capillary basis; 5, a splenic liquid; 6, splenic glands, which are all lymphatic glands; 7, lymphatic vessels; 8, nerves; 9, cellular tissue; 10, a membrane enveloping the whole spleen. The first five of these compose the *vesicular apparatus* of the spleen, the blood-vessels being placed here because it is in these parts that they offer their most remarkable peculiarities. The sixth and seventh form the *glandular apparatus*. The last three are common to all parts of the spleen.

1. *Splenic vesicles.* They are distributed in every part, being separated by partitions, to whose surface their's is as 3·2. In the dried spleen they are irregularly polyhedral; in the injected spleen spheroid or ovoid, and this, it is probable, is their form during life, when they are filled by fluid. In the calf they measure, on an average, about $\frac{1}{8}$ of an inch in diameter; in the dog and sheep they are of similar size; in man they are smaller and more regular, their average diameter being about $\frac{1}{25}$ of an inch. None of them have a simple form; but the walls of all are traversed by vessels which, covered by the lining membrane, form folds or falciform or crescentic lamellæ, like those formed by the umbilical vessels behind the peritoneum. The result of these folds, of which there is a series of various extent, is to decompose the principal cavity into recesses, and these again into loculi, on the bases of which are seen in relief the glands and granules and capillary ramifications.

In these vessels there are two kinds of orifices: 1. Those by which all the vesicles communicate with one another, which are nearly circular, with thin edges covered by the lining membrane. There are two or three of these in each of the larger vesicles, and the diameter of each is about $\frac{1}{2}$ or $\frac{1}{4}$ of that of the whole vesicle. 2. The venous orifices, which are less numerous, and only scattered here and there, on any part of the walls of the vesicles indifferently. They are the absorbent mouths of veins of the same size as themselves, which are passing to the veins of the partitions; and they are guarded by a crescentic fold, which forms an incomplete valve.

The *intervesicular partitions*, or rather *spaces*, are formed by the separation of the enveloping membranes of adjacent vesicles, and contain the vessels and the splenic glands. Their width or thickness is inversely proportioned to the size of the vesicles. Also, where two vesicles only lie together the partitions are narrow; where several meet they are enlarged into irregular spaces filled with glands.

The enveloping membranes of the vesicles are continuous with one another throughout the spleen; so that one may consider them as forming a single membrane, homogeneous in every part, and divided into thousands of little ampullæ, isolated by constrictions, which constitute their orifices of communication, and supported by ramifications of vessels, which form a kind of soft frame-work of the whole organ. The membrane of each vesicle is formed of a single layer, from $\frac{1}{150}$ to $\frac{1}{300}$ of an inch thick; but its organization is very complex, for it contains the granulo-vascular basis with its close network of capillary blood-vessels and lymphatics. It cannot, therefore, be considered as produced by a mere dilatation of the common internal coat of the veins; though, as will presently appear, the veins of the spleen present characters exactly analogous to those of the vesicles.

II. *Blood-vessels.* These, according to the form, size, and distribution, may be described in three sets: 1, the large trunks or splenic vessels, commonly so called; 2, the intervesicular vessels; 3, the vesicular vessels. The *splenic vessels* pass, artery and vein together dividing three or four times, towards the periphery of the spleen. The veins are perforated by small, circular holes, which lead to the veins of the partitions or intervesicular veins. The terminal veins are decomposed, in the course of their canal, into a succession of cellules, separated by vascular constrictions, of which the organic composition is absolutely identical with that of the vesicles which succeed to them. The *intervesicular vessels,* arising from the preceding, run in the partitions, where they are distributed to the splenic glands and the vesicular membranes. In man they have a diameter of about $\frac{1}{500}$ of an inch. The vessels of the glands pass abruptly into them, like those of the corpus cavernosum. The *vesicular veins* form those little folds on the surface of the small cavities which have been already mentioned. But, by a singular arrangement, their branches project into the vesicular cavity, to distribute themselves to the floating corpuscles which are appended to their ultimate branches, like a bunch of grapes, according to the comparison of Malpighi. On the basis of the membrane the last capillaries form with the lymphatics the granulo-vascular network. As to the venules, there are two kinds of them: those of the common capillary network, and the *absorbent venules,* which are much larger, and which are seen to open by free orifices in the cavities of the vesicles. Lastly, all the small vessels of the spleen, the intervesicular as well as the vesicular, are distinguished, when turgid, by continuous series of dilatations and constrictions, which give them a remarkably knotted aspect.

III. *Floating vascular corpuscles.* These float within the vesicles, appended, as in pedicles, to the last branches of the capillary blood-vessels and lymphatics. They are formed of a lenticular nucleus, from which, when they are turgid, there project the stems of little aigrettes, radiating towards the circumference, so as to resemble the flower of an umbelliferous plant. These aigrettes themselves are composed of a filament, terminated by brilliant little spherules, collected in the form of a chaplet. The cerpuscular nuclei have, in the calf, a diameter of from $\frac{1}{350}$ to $\frac{1}{50}$ of an inch, and their capillaries are from $\frac{1}{1200}$ to $\frac{1}{2500}$ of an inch in diameter. In man the corpuscles are from $\frac{1}{300}$ to $\frac{1}{250}$ of an inch in diameter, and the capillaries which pass to or from them have a caliber varying from $\frac{1}{800}$ to $\frac{1}{3000}$ of an inch.

IV. *Granulo-capillary basis* is the name given by M. Bourgery, on account of its aspect, to the membrane of the vesicles, which is itself composed of two elements: 1st, spherical granules, which are very pale, juxtaposed, and from $\frac{1}{800}$ to $\frac{1}{600}$ of an inch in diameter; and 2dly, arterial, venous, and lymphatic capillaries, varying from $\frac{1}{2500}$ to $\frac{1}{5000}$ or $\frac{1}{7000}$ of an inch in diameter, distinct from the larger capillaries of the floating corpuscles. The vascular net forms a lacework several layers thick.

V. *Splenic liquid.* This liquid or *splenic blood* appears to be the product of an elaboration by the floating corpuscles and the granulo-capillary basis, which is deposited in the cavities of the vesicles, and is taken up again by the absorbent

vessels of their walls. It is thick, viscid, and brownish red; and, under the microscope, seems to be composed of several kinds of globules suspended in a yellowish, unctuous liquid; especially of, 1st, *lenticular* globules, some of which are surrounded by a red limbus, and do not appear to differ from ordinary blood-globules, while others are colourless; and, 2dly, whitish globules, irregular in form and size, which remind one of those found in the chyle and lymph.

VI. *Splenic glands.* The preceding parts compose the vesicular portion of the spleen, the two next form its glandular apparatus. These glands constitute, in respect of volume and consistence, the most considerable organic element of the spleen. United by cords of the same substance, they fill up, with the ramifications of the vessels, the intervesicular partitions or spaces. Their greatest diameter, when in a state of repletion, is, in the calf, about $\frac{1}{25}$ of an inch, in man about $\frac{1}{100}$. In the ox they are as much as $\frac{1}{12}$ of an inch, or more, in diameter, and one can see them with the naked eye, in the form of brown or whitish corpuscles. It is these which most authors have regarded as the vesicular glandules of Malpighi. They are isolated or agglomerated in the intervesicular spaces, and are united by cords into chaplets which extend through every part of the spleen. They receive a great number of capillaries, and a thin layer of their substance well injected seems to resolve itself into infinitely small granules and capillaries.

The relation which the lymphatic vessels bear to these glands shows their physiological import. One sees plexuses of lymphatic vessels interlaced on their surface, afferent trunks penetrating them and their connecting cords, efferent trunks leaving them and proceeding to the intervesicular blood-vessels, and infinite subdivisions of the minute lymphatics in the interior of both them and their cords. So that there can be no doubt that these splenic glands are merely microscopic lymphatic glands.

VII. *Lymphatic vessels.* These exist in immense number in the spleen. Arising from the granulo-capillary network, in which they form the very closest lacework, they join to themselves the branches from the floating corpuscles, and with these they pass to the intervesicular glands. There are from fifteen to twenty of their larger branches on each vesicle, and the form of these is peculiar. Externally each large branch is surrounded by numerous interlacing small ones, just as the blood-vessels of the digestive organs are surrounded by lymphatics of larger size. And internally, besides possessing very distinct valves, they are divided into loculi, the agglomeration of which at their junctions gives the notion of a kind of rudimental gland: as if the lymphatic vessels were not merely canals of transport, but organs of elaboration.

VIII. On the remaining three elements M. Bourgery offers no peculiar remarks. Neither does he give any definite explanation of his view of the office of the spleen; he only calls it "a vast lymphatico-sanguine gland," regarding the veins and the vesicles as apparatus for the elaboration of blood, and the glands and their connecting cord as one great lymphatic gland, broken up into microscopic glands, united by cords of the same substance that it may extend itself through every part of the spleen, and everywhere surround the vesicles.

Gazette Medicale. Juin 11, 1842.

(*From a memoir read before the Academy of Sciences, 6th June.*)

Fatty and Cellular Degeneration of Muscle. By Dr. JOSEPH ENGEL.

ACCORDING to Gluge, the fatty degeneration of muscle consists in the deposition of fat, partly in a free, partly in an encysted state, between the primitive muscular fibres. The author is of opinion that the above is not the only metamorphosis which takes place, but is not able to persuade himself that an actual transformation of muscular into cellular tissue is to be admitted. So long as muscle, examined by the microscope, undergoes no other alteration than a mere change of colour, the form of the primary and secondary muscular bundles remains unaltered, except that their sheaths appear covered with a greater

number of granulations, the greater part of which, however, are readily removed by a slight pressure. The sheath itself of the fibre appears more brittle and easily ruptured than before; the least pressure destroys its continuity. In the same proportion in which the fatty condition of the muscle and its loss of colour become apparent to the naked eye, do the longitudinal lines of the muscular bundles become more evident, the granulations more numerous, and the bundles themselves appear covered with a multitude of large bladders of fat, which, from becoming flattened, present various forms, and are at length united, partly with each other, partly with the bundles of muscular fibres by a tolerably adhesive substance. The author failed to detect fat in a free state in fresh muscle; which, however, if treated with spirit of wine, gives out fat in notable quantity. Yet in this case the fat seems to be simply extracted from the sebaceous bladders or cysts by the action of the spirit of wine; these cysts, thus deprived of their contents, being visible, in a shrivelled state, between the bundles of muscular fibre. Water causes the fat to solidify, and to appear surrounding the muscular bundles like a sheath, from among which, by a slight pressure, it may be forced in the shape of half cylinders. The muscular bundles amid this deposition of fat, partly free, and partly encysted, lose entirely their sheaths; so that their delicate fibrillæ (primitive fibres,) after the fat being washed away, lie bare. This condition, and no other, it is which constitutes the so-called degeneration of muscle into fatty and cellular tissue, and which consists of nothing but a deposition of fat between the bundles of muscular fibre deprived of their sheaths. The sheath of the muscular bundles appears to be indispensable to elasticity, firmness, equal action, and vital function of muscle. Whether the change of colour which supervenes after some time in separated muscle be owing to the loss of the sheath, the author is not prepared to say. A question arises, whether the destruction of the sheath of the muscular bundles be owing to the deposition of fat, or whether the converse relation be the real one, or finally whether both changes be synchronous results of a common cause.

Oesterreich. Medicinische Wochenschrift, No. ix. den xxvi. *Februar*, 1842.

Pathology of the Tissues. By Dr. Joseph Engel.

1. An osteophysis, or formation of bone on the inner surface of the skull, in the case of pregnant women, is a frequent occurrence in Austria. It has hence been inferred that there is a certain relation between pregnancy and this species of osteous growth; although against the supposition that there is any such necessary connexion militates the fact that in England this growth has never been observed in pregnant women, and that it has been noticed in women who were never pregnant, and also in men.

The first appreciable phenomenon in this peculiar morbid process is the appearance of a gelatinous, yellowish-red exudation on the outer surface of the dura mater, without any apparent actual implication of that membrane. This exudation, examined by the microscope, exhibits cells containing nuclei, which become united by an amorphous, gluey substance, and extend themselves on one or on both sides by fibre-like prolongations. The nucleus of the cell is tolerably large, and is sometimes surrounded by a capsule. The dura mater becomes adherent, in consequence of this exudation, to the corresponding part of the cranial bone; and from this point dates the deposition of earthy salts, which is effected by the same process by which, in cases of fracture, the formation of bone takes place from the deposition of callus. The phenomena are analogous to those which take place in periostitis, in which exudation from the bone amalgamates with phosphate of lime, resulting in osteophysis.

The frequency of this morbid affection and the circumstance of its occurring in advanced life, are additional grounds for regarding it as having no necessary connexion with the pregnant state. Rarely does this disease manifest itself, during life, by any symptoms.

2. *Proneness of articular cartilage to suppuration.* The author illustrates this proposition by the case of a phthisical subject in whom the knee-joint exhibited all the appearances of chronic inflammation. The synovial membrane was connected with the subjacent fat by a glairy, yellow exudation, and was thickened though not injected; the cavity of the capsule was filled with healthy pus, a thin layer of which attached itself to the surface of the capsule; the semilunar cartilages were entire; the crucial ligaments very friable; the cartilaginous extremities of the femur and tibia were very thin, sharp, and their edges easily separable from the subjacent bone, and divisible into three layers. The brittleness of these "pathological cartilages" was remarkable. They were easily pressed, under the microscope, into an homogeneous, granular mass.

3. *The mode of cicatrization of typhus ulcers.* After the ulcerative process is at an end, and the so-called typhus ulcer appears clean, its bottom, situated in the cellular or muscular tissue, appears, when viewed under an oblique light, covered with an exceedingly fine and remarkable cuticle. This begins from the abrupt margin of the usually slate-gray-coloured ulcer, and runs towards the centre of the ulcer, but does not entirely reach or cover this point. The membranous expansion which extends over the rest of the ulcer, is partly of a cellular, partly of a fibrous character, separate or mixed. The cells are round or elliptical, and seem to form fibres by mutual approximation of their homologous sides and the subsequent absorption of the walls of the cells where these are in contact with each other. In other cases fibrous bundles are observable, which appear to be prolongations from a single cell. The author's observations do not permit him to say positively whether this last species of fibrous formation is the origin of submucous cellular tissue, the former of the mucous membrane itself.

4. *Morbid deposits on the serous membranes in cases of protracted diarrhœa.* Every anatomist knows that viscid, thin, colourless exudation which appears as a layer on the free surface of the great serous membranes, in cases of exhausting diarrhœa. In children, more especially, this exudation is not a rare occurrence, and in the most of this class of cases is limited to the pulmonary pleura. The false membrane is easily obtained by stripping it off, though seldom in such purity and quantity as to allow an analysis to be made of it. Examined with the microscope, it appears an amorphous, gluey mass, amid which are discernible numerous round cells, one or both of the sides of which are prolonged into threads; the cells seem two or three times larger than blood-corpuscles, and appear filled with many pigmentous nucleoli. In many there was discernible a pale, round granule or nucleus, about the size of a blood-corpuscle; and this nucleus seemed sometimes made up of many granules. The ultimate constitution of this false membrane is consequently not easily determined.

Oesterreich. Medicinische Wochenschrift, No. iii. *Jänuar*, 1842.

Pathological Observations confirming the Experiments of Panizza on the Nerves of the Tongue.

In this paper, which seems to be published by the Medico-Chirurgical Society of Bologna, by whom the patient was examined, there is but one original observation. It relates to a man fifty-two years old, who, after suffering for some time from wandering pains in his limbs, and being actively treated for them, was attacked by pain in the head and dulness of sensation in the left side of the face. The latter slowly extended over the right side also, and to the interior of the face, and ended in complete insensibility of every part supplied by the fifth nerves. Sight, hearing, and the sense of smell were unaffected. Many experiments were made to determine his power of taste; and it was found perfect for all sapid substances: he could distinguish the qualities of each article of food at his meals as well as he could when in health, and could discern the change and loss of savour which ensued when he kept them in his

mouth. From the loss of common sensation, however, swallowing was difficult: he could not drink a large quantity at a time, and he was often choked in the attempt to swallow fluids. He was treated with electricity with slight temporary advantage. He died of intercurrent disease, but his body was not examined.

Bulletino della Scienze Mediche. Aprile, 1841.

On the minute Structure of the Lungs in Man and the Mammalia.
By M. Bourgery.

The peculiarities of M. Bourgery's views relate chiefly to the mode in which the minutest bronchial tubes terminate. "Each lobule," he says, "receives a single central bronchial branch, which forms the common tree of its air-tubes; or, if the lobule be large, two or even three of these branches may enter into it. The smallest of these lose themselves laterally in the manner presently to be described; a single tube, which is the continuation of the trunk, reaches the peripheral base of the lobule, and there ramifying, turns towards one of its angles, which forms the terminal summit. Proceeding from this decreasing central tree there arise from every side, in alternate succession and in star-like radiation, secondary branches, which I have named *bronchial ramified canals*, the ultimate expansion of the tracheal tree, beyond which the *labyrinthic* arrangement commences."

This labyrinthic arrangement is made up of numberless minute canals, (*aerial labyrinthic canals*,) arising from the bronchial ramified canals, running tortuously in every direction, and branching and anastomosing in the most multiform manner. They thus inclose numberless irregular spaces, in which, as well as on their walls, the blood-vessels ramify and form their capillary network. They give the idea of a space all divided with thousands of tortuous branchings, everywhere continuous with itself, and where there is no termination except the one common orifice of entry and exit in the trunk of the bronchial tree of each lobule. In no observed instance was there anything like a cul-de-sac or cæcal termination of any of the canals; "whatever point or surface he observed, there are flexuous canals anastomosing in every plane, but nowhere are there any straight canals without anastomoses or any vesicles." The sinuous canals, moreover, are from space to space constricted, not, as Willis supposed by ligamentous fibres, but by annular vessels, circumscribing in their intervals *loculi*, at the bases of which are the orifices of other labyrinthic canals, and giving that appearance of chains of cellules which forms the basis of the theories of Malpighi and Helvetius. In such canals as these, the ramified bronchial canals, after giving off great numbers of them from their sides, ultimately, by dividing twice or three times, terminate; and their terminal branches enter into the common labyrinth, comporting themselves like the tubes given off from their sides.

Gazette Médicale. Juillet 16, 1842.
(*From a memoir read at the Institute, July* 11, 1842.)

On the Muscular Coat of the Stomach. By M. Noel Gueneau de Mussy.

The author makes four layers of muscular fibres in the stomach, by dividing the superficial or longitudinal layer into two, which he calls, respectively, cardiac and pyloric. The former he regards as the continuation of the longitudinal fibres of the œsophagus, the latter of those of the duodenum. [The distinction is unimportant, and not accordant with the analogy of the muscular coat of the stomach to that of the œsophagus and intestines, which is exact, if the stomach be regarded as a part of the one tube dilated on its left side. The arrangement of its muscular fibres is the same as would be produced in any other part of the canal, if such a dilatation could be effected.]

Gazette Médicale. Juin 4, 1842.

On the Development of the Entozoa. By M. Mandl.

In support of the doctrine of the spontaneous generation of the entozoa, the frequent occurrence of the ascaris nigro-venosus in the lungs of frogs has often been quoted. M. Mandl has succeeded, by a microscope magnifying 250 diameters, in detecting the coloured eggs of these entozoa in the lungs of frogs in which no trace of the entozoa themselves could be discerned. This, he observes, makes it very probable that these little ova, the diameter of which is scarcely more than four-times that of the blood-corpuscle of the same animal, have been carried into the lungs either in respiration or by some other passage. [The conclusion is in no degree warranted by the premises.]

Gazette Médicale. Juillet 2, 1842.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Case of Laceration of the Pulmonary Artery. By Dr. Helmbrecht.

A pontooner (pontonier), twenty-one years and half old, who had enjoyed good health, with the exception of slight dyspnœa during the three months that he had been in the army, and was quite equal to his laborious duties, was ordered to accompany his regiment on their march from Glogau to Mayence. On the route slight dyspnœa was his only ailment; and on December 5th having been engaged in carrying wood, he went to bed in good health. In the night he was roused by violent pain at the right side of the sternum with great dyspnœa, which subsided in the course of a few minutes, leaving him pale and cold, with a face expressing great anxiety and bathed in cold perspiration. After a short time nothing remained of his former condition but a sense of uneasiness in the precordial region, with very slight palpitation and remarkable feebleness of voice. He was bled, a mustard poultice was applied to the chest, and a mixture was given, containing sulphuric æther. During the afternoon of the following day he seemed well, but at eleven p. m. complained of sense of exhaustion and asked for drink. The attendant noticed his face become pale, and finding his feet cold, fetched the doctor. His pulse was then imperceptible, and the surface of the body cold, and in a few minutes the patient was dead.

On examining the body a large quantity of coagulated blood was found in the pericardium. The heart was healthy, the wall of the left ventricle rather thick. Minute calcareous concretions existed on the outer side of the pulmonary artery close to its origin, and extending towards the right ventricle. The inner coat of the artery was separated from the elastic coat for the space of three inches from the point where it joins the right ventricle, and torn into shreds which projected into and narrowed its cavity. The whole lining membrane of the artery was coated with a thin layer of fibrine. At its origin from the ventricle, exactly in the situation where the concretions terminated, was a hole of the size of a fourpenny-piece, through which the blood had escaped into the pericardium. The neighbourhood of this opening was of a deep red colour. The lungs were healthy and bloodless.

Casper's Wochenschrift. March 5, 1842.

On Softening of the Brain. By Dr. Budge.

The writer questions the inflammatory origin of the colourless softening of the brain, though he admits it in the more frequent case in which there is a manifest rose tint of the cerebral substance. In confirmation of this opinion he appeals to the investigations of Gluge, who, while he discovered in both kinds of softening of the brain a partial destruction of the nervous fibrillæ, could not detect any exudation corpuscles in the capillaries of the brain in the white softening, though they were very evident in the red softening.

In the red softening the circulation becomes suspended in some of the capil-

laries of the brain. The blood-corpuscles accumulate within the vessels while serum transudes through their walls and renders the nervous substance soft and pultaceous. A change, too, usually takes place in the blood-globules themselves, by which their colouring matter becomes dissolved in the serum, and imparts to it a red hue. This must result from some alteration in the serum; probably from a diminution in the quantity of chloride of soda, which has a principal share in producing the insolubility of the colouring matter. The hæmatin of the blood-corpuscles would thus become dissolved in the liquor sanguinis, and occasion the red serum, while the altered blood-globules, probably from those larger compound bodies to which Gluge gave the name of compound exudation corpuscles, and which Weber termed round, cohering granules. It is, indeed, probable that the inflammatory process gives rise to a decomposition of the chloride of soda, the hydrochloric acid being eliminated from the blood by the stomach and giving rise to the impairment of the digestive functions which attends the inflammation of important organs, while the soda might produce a change in the albumen by which it is rendered more analogous to fibrin; a change for the occurrence of which many facts might be adduced.

The symptoms attending softening of the brain may be referred to two classes. 1st, Those which result from the nature of the disease—that is, from the partial destruction of the fibres of the brain. These may again be subdivided into two classes, according to whether they are symptoms of *paralysis* or are produced by *antagonism.* 2d, Symptoms which depend on the seat of the disease; and, unfortunately, in the present state of our knowledge these cannot be stated with certainty. In conclusion, a case is related in which the symptoms during life were referred to disease of the cerebellum, and in which that organ was found after death in a state of general softening.

Organ für d. ges. Med. Bonn. Bd. i. Hft. 3.

Monomania Ophthalmica. By Dr. Winternitz.

A gentleman forty-nine years old, was seized with gout in 1837, which he himself was inclined to attribute to cold applications made to an accidental swelling of the head. But the simultaneous appearance of an obstinate paronychia on his right middle finger, together with the decline of his strength, earthy complexion, and emaciation, left no doubt as to the constitutional nature of the affection. Late in the autumn of 1840 he made a journey to north Germany in an open carriage, by which exposure he suffered from rheumatism. The abdominal organs became disordered, and about the same time he suffered morally from anxiety. All these concurrent circumstances had the effect of developing the hypochondriac diathesis in the patient. The disease which forms the subject of this article began in July, 1841. Dr. Winternitz found him in a state of the greatest uneasiness, complaining of a confusion of head. During the day and while in the act of walking he had frequent attacks of faintness, followed by general perspiration. He was sensible of a sudden spasmodic constriction of the right eye-ball, as if it were confined and pressed in the socket, accompanied by an actual twitching upward and downward of the upper eyelid. Objects appeared as if surrounded by a gauzy halo: the patient was unable to read, in consequence of the letters appearing to swim before him. Writing was less difficult, although he was continually apt to carry the pen beyond the paper. The eye itself, beyond a slightly diminished activity of the lachrymal gland, betrayed nothing morbid.

The author's opinion that the affection was of an hypochondriac or psychical origin was founded on the following circumstances. There were no objective symptoms; the patient's whole attention was engrossed by his disease, which he was disposed to consider by turns of a congestive, nervous, arthritico-metastatic, &c., origin; he sought eagerly the society and conversation of oculists; disconsolately anticipated the total loss of vision; the patient had, moreover, at a former period suffered from hypochondria.

The therapeutic indication was to withdraw the patient, as it were, from himself, and to interrupt his habit of constantly thinking of his disease. This was effected chiefly by means of a jaunt to Paris. Here the patient was advised to employ tepid camomile-flower baths, with cold water douches along the spine. Vertigo became of rarer occurrence; he gradually regained facility in reading and writing; and after three weeks' residence in Paris felt a strong desire to revisit his family, whom he reached in perfect health.

Oesterreich. Medicinische Wochenschrift. *Februar* 12, 1842.

Rupture of the Trachea. By Dr. Bredschneider, of Fischhausen.

The patient was a child one year and three quarters old, who was suffering from bronchitis with some signs of hydrocephalus, among which was that of incessantly and violently tossing its head about. On the 5th day of the disease a considerable tumour formed suddenly over the neck and chest. It began below the cricoid cartilage, reached upwards to the right ear, and extended downwards over both sides of the chest. A small incision was made into it, air escaped with a hissing noise, and, by the aid of pressure, the tumour collapsed. Two days after this the child died, and a rent was found in the trachea, half an inch long, on the right side, below the first cartilaginous ring. Was this produced by the violent movement of the head, or by coughing?

Casper's Wochenschrift. *Juli* 9, 1842.

Tic-Douloureux from a Tumour beneath the Cerebellum.
By Professor G. Barilli.

This paper was read at the meeting of the Academia delle Scienze, of Bologna, on the 12th of March. It relates to a lady, forty-two years old, of acute sensibility, in whom, after some inflammatory affection of the right side, symptoms of neuralgia in the right side of the face commenced. They gradually increased in severity, and, with two remissions—one of three the other of five months—they lasted till the time of her death, which occurred in the midst of the most severe sufferings, two years from the beginning of the neuralgia. The most striking characters of the affection were paroxysms of pain, preceded by a kind of aura and formication from the right temple to the gum of the same side, and violent tremulous spasm of the right temporal and buccal muscles. The sense of taste was also, during the paroxysms, lost. After death the blood-vessels of the brain and its membranes were found turgid. There was a tumour of a spheroidal form, two thirds of an inch in diameter, firmly adhering to the posterior-superior lamina of the tentorium, close by the sella turcica, and with its upper part lying in an indentation on the anterior margin of the right crus cerebelli. It was composed of a pulpy substance, deposited between numerous membranous partitions, and had a small plate of osseous matter at its base.

The author remarks, that the morbid appearances illustrated clearly the nature of the symptoms. One of the nerves affected was the motor division of the fifth, which Palletta has shown to arise by two filaments from the anterior margin of the crus cerebelli: on this the tumour pressed so as to push it from its usual direction. The sensitive branches of the fifth also were affected in the same proportion as they were subjected to the pressure of the tumour.

Bulletino delle Scienze Mediche. *Marzo*, 1842.

Case of Peritonitis, in which the effused Fluid escaped through the Umbilicus
By Dr. Beonhardi, of Eilenburg.

A healthy girl, aged 5 years, was attacked on January 4, 1841, with symptoms of catarrhal fever. On January 6, the disease put on a more serious

aspect, and signs of peritonitis came on. Active treatment, by local depletion, the administration of calomel, and mercurial inunction, was employed, and followed by marked improvement, but not by complete recovery. Some degree of fever still remained, the abdomen continued painful and grew distended, while percussion proved that this distension was not owing to the presence of air in the intestines but was produced by some liquid body.

No remedial measures could be adopted, the child obstinately refusing all medicine, and the condition of the patient became daily worse and worse. On January 22, the umbilicus became prominent, semitransparent, and red; and on the following day the tumour had increased to the size of a hen's egg, and fluid was distinctly perceptible through the attenuated skin. On the 25th the swelling burst and a quart and a half of fluid, partly purulent, escaped in a stream of the size of a goose quill, and an equal quantity drained away by degrees on the following day. The abdomen still continued tender, and the febrile symptoms were unrelieved, but the child now submitted to take small doses of calomel, which were continued till the 12th of February. A great improvement in the condition of the patient now took place, but the opening in the abdomen continued unclosed, and still gave exit to fluid though not in large quantity. Dr. B. waited till September, when he applied a graduated compress and elastic bandage. In fourteen days the opening closed, but the child still wears an umbilical hernia bandage, the destruction of the cellular tissue, by which the umbilicus was closed having permitted a protrusion of the intestines to take place.

Medicinische Zeitung. March 9, 1842.

Apparent Death by Lightning. By Dr. Hartmann, of Neu Ruppin.

Three persons were at the same instant struck by lightning. In one the symptoms were severe and remarkable. He was a healthy man of twenty-six. When the author saw him, an hour and half after the stroke, he lay completely unconscious, as if in apoplexy. His pulse was less than 60, full and hard; his respiration snoring; his pupils dilated and insensible. There were frequent twitchings of the arms and hands; the thumbs were flexed and immoveable; and the jaws firmly clenched. Soon after the author arrived, severe clonic spasms came on, so that four men could scarcely hold the patient in his bed; and his body was drawn to the left side. As soon as these had relaxed he was bled to 16 oz., cold was applied to the head, a blister to the nape, and mustard poultices to the legs. Stimulating enemeta and opium were also administered; and in the course of twenty-four hours the patient's consciousness slowly returned, and he was soon completely recovered. The only external injury discernible was a red streak, as broad as a finger, which extended from the left temple over the neck and the sternum, to the precordial region, and which disappeared completely in a few days.

Medicinische Zeitung. Juni 15, 1842.

On a very simple means of arresting Epistaxis. By Dr. Negrier, of Angers.

This consists in nothing more than closing with the opposite hand the nostril from which the blood flows, while the arm of the same side is raised perpendicularly above the head. In every instance in which he has had recourse to this means during the past three years M. Négrier has always found that it suspended the hemorrhage: a fact of which he offers the following explanation.

When a person stands in the ordinary posture, with his arms hanging down, the force needed to propel the blood through his upper extremities is about half that which would be required if his arms were raised perpendicularly above his head. But since the force which sends the blood through the carotid arteries is the same as that which causes it to circulate through the brachial arteries, and there is nothing in the mere position of the arms above the head to stimulate the heart to increased action, it is evident that a less vigorous circulation through the carotids must result from the increased force required to carry on the circulation through the upper extremities.

Archives Générales de Médecine. June, 1842.

On the difference between true Typhus and Typhus Abdominalis, and on the latter affection in Children. By Professor Seidlitz, of St. Petersburg.

Great confusion has arisen from the application of the word typhus to diseases essentially distinct and differing from each other, as typhus abdominalis and true typhus—typhus sanguineus.

The difference between the two diseases may be summed up somewhat in the following manner:

Typhus sanguineus is characterized by a short stage of premonitory symptoms; the rapid development and increase of the disease, marked by some striking though temporary symptom, as shivering; pain in the head; vomiting; extraordinary loss of strength. There is violent disturbance of the circulatory system; the blood is black, altered in character; and a great tendency exists to congestion of the brain, liver, and intestines, constituting hypostatic inflammations or apoplexies of those organs. The skin becomes burning hot, and an eruption of an erysipelatous character appears in large patches over the whole body. The urine is in moderate quantity, turbid, and loaded with crystalline deposits. The disease is contagious; it observes in its course a distinct septemeral type; and critical evacuations precede its termination.

Typhus abdominalis is characterized by long previous indisposition, gradual development and slow progress of the disease, with especial disturbance of the digestive powers. Excitement of the circulatory system does not come on till afterwards; and the blood, which is almost normal in character, does not become concentrated about the parenchymatous organs, but a tendency exists to serous effusion. Cerebral affections occur at a later period, and are evidently secondary. There is no disagreeable heat of skin, but rather a diminution of temperature; petechiæ are scattered about the lower part of the abdomen; and miliary eruptions appear towards the end of the disease. The urine is straw-coloured, and, at the end of the attack, deposits a sediment which resembles spiders' web. The disease is not contagious. Its course is very uncertain: it exceeds four weeks, and sometimes continues for six months. Sleep and a constipated state of the bowels announce its termination.

At different ages the symptoms of abdominal typhus vary considerably. One form which has usually not been referred to this head is that nervous fever which occurs in children between their sixth and their eighth year. Its precursory symptoms, which consist in irregular appetite, a variable state of the bowels, occasional attacks of fever, marked emaciation, dryness and harshness of the skin, unquiet sleep, and pettishness in the child's manner, often continue for several weeks and then disappear completely during the heats of summer, or, if they made their appearance in autumn, on the cold of winter setting in. Sometimes with the return of spring or autumn these symptoms recur, though less severely; and each year they reappear with diminished severity, till at last they cease entirely. At other times, however, under the influence of errors in diet or of moral causes—as the wearisomeness of a child's tasks at a boarding-school—the appetite is entirely lost, the evacuations become liquid, the skin grows drier, the pulse more hurried, and eventually the disease appears in its severer form. The febrile character of the affection becomes more marked, the skin of the body and the head are burning hot, the thirst is urgent, and slight delirium comes on. The patients now remain in bed without expressing any desire to get up, and soon they begin to pass their evacuations involuntarily. The dejections are liquid, of a yellowish brown colour, and offensive smell. The abdomen is tympanitic, slightly painful; the skin of the abdomen and of the whole body, with the exception of the feet, is hot; the lips and tongue are dry, but nevertheless, the patients do not ask for drink, though they swallow anything that is given them. They lie for hours together in the same position,

altogether unconscious and, to all appearance, in a deep sleep. After three, four, or five days consciousness seems to return, but the little patients seem unable to utter a word or even to protrude their tongue; they lie with their eyes closed, but sleepless; their senses being either weakened or else preternaturally acute. At the end of ten or twelve days there is some slight return of appetite; the tongue, which at no time is thickly coated, becomes moister, and the skin of the lips peels off; but before the end of the third week no critical evacuation takes place, no deposit is thrown down from the urine, no moisture breaks out on the skin. About the beginning of the fourth week, however, perspiration appears about the neck, chest, and abdomen; but after two or three days it ceases and leaves the skin harsher than ever. But now frequent crops of miliary vesicles show themselves, after some hours of restlessness and febrile disturbance; but in the intervals of these attacks the sleep is sound and tranquil. The tongue becomes perfectly clean, very red, broad, and flabby; the thirst ceases, but the patients are troubled by hunger, so importunate that they ask for food every half hour. In about the fifth or sixth week the skin once more becomes perspirable, desquamation of the epidermis takes place, and the hair falls off. The children, though worn to skeletons, may now be regarded as convalescent, but they are still unable to walk; and three, four, or even six months are requisite for their perfect cure, while they are very liable to relapse, from errors in diet or from neglect of the condition of the bowels.

The treatment adopted by Professor Seidlitz is the following: At the commencement of the disease, when premonitory symptoms only exist, he gives very small doses of hydrochloric acid, with ether or Hoffman's anodyne, or else a mixture of *eau de chlore,* in some aromatic infusion. He orders warm baths twice a week, directs the body to be well rubbed with castor oil every day, insists on a very abstemious diet, and gives the patient pure water for drink. The slight diarrhœa which may exist is not to be suppressed, but small doses of castor oil may be given, under the use of which the evacuations gradually become more consistent. If in the course of the disease the evacuations become flocculent and more frequent, an emulsion of olive or poppy oil may be given, and sinapisms may be applied to the hypogastric region or to the feet. If the abdomen become hot and tympanitic, half a drachm of strong mercurial ointment may be rubbed into the hypogastric region twice a day, until the symptoms decline, when recourse may again be had to the acid or chlorine; or, afterwards, to an infusion of the flowers of the *salmiac ferrugineux.* The occurrence of delirium must be met by the application of one or two leeches to the mastoid process, so as to keep up a slight draining of blood for twelve or eighteen hours, and ice must be almost constantly applied to the head till consciousness returns. At the same time, half-grain or grain doses of calomel must be given till they produce green evacuations, when that remedy must be suspended, and the acetate of lead, in quarter-grain doses, may be given three or four times a day, either alone or in combination with musk, in rather large doses, so long as the tympany and liquid stools continue; and, during the same time, mercurial inunctions may be employed every two hours. A small blister may now be applied to the epigastrium, and should, after an hour and a half or two hours, be removed to another part, so as to produce a slight counter-irritation of the skin of the abdomen. After ten or twelve days more the acetate of lead may be discontinued, a camphorated julep may be given instead, and a little Dover's powder may be administered, and a warm bath employed by way of preparing the skin for the future crisis. As soon as the miliary eruption appears all medicine may be discontinued, in the great majority of cases; but the convalescence, usually tardy, is always expedited by removal to a purer air, or by mere change of dwelling.

Abstracted from Professor Seidlitz's Clinique Médicale, published in the Journal de Médecine, &c., de St. Petersbourg. Part iii. 1841.

Insect Origin of Smallpox. By M. SERRES.

AT the Institut, on the 4th of July, M. Serres mentioned the following fact, seeming to favour the hypothesis of animalcules in smallpox. By covering each pustule with a glass capsule, which is kept for some days in its place, he has seen the process of eruption either go on or languish, or be completely abortive, according as the glass was transparent or more or less opaque. This influence was evidently due to the contact of the air. [Qy.—Should not this be the admission of light?] The experiment, he adds, was not merely curious, for it led to a modification of some of the hygiènic measures adopted in smallpox. Previously patients were generally placed in situations as well aired and lighted as possible; but now one knows that dark situations are far better for this kind of disease, and that this change alone is enough to ensure the most favorable progress of its evolution. The success at La Pitié was never more complete than during one year when all the patients with smallpox had, of necessity, to be put into a low, ill-aired, dark ward, a sort of cellar. The confluent cases there went on as favorably as was possible. At present, in the same hospital, they are moved from the first floor into the *rez-de-chaussée*, and they do well there.

M. Serres took this occasion to mention that he had seen between 1700 and 1800 cases of smallpox in private and in hospital practice, and that he was certain that the number of those affected with smallpox after vaccination was not greater than that of those who had smallpox twice. [For the first of these statements M. Serres may have made it probable, as our old physicians believed, that it is beneficial to exclude the light in smallpox; but there is no evidence that ill ventilation is to be desired.]

Gazette Médicale. Juillet 9, 1842.

Case of suddenly-occurring and as suddenly-ceasing Dumbness.
By Dr. BICKING, of Mühlhausen.

THE patient, a strong lad of fourteen, has, for three successive years, become suddenly dumb in the middle of October and has remained so till the beginning of the following March, when his speech has again suddenly returned. No external cause seems to have acted upon him previous to the occurrence of the dumbness, nor, during its continuance, has he ever shown any sign of general bodily or mental disorder. Medical treatment has been of no avail. [Qy.—May there be imposture here?]

Oesterreichische Med. Wochenschrift. Mai 14, 1842.

On a peculiar Granular Appearance of the Buffy Coat of the Blood.
By M. PIORRY.

IN a recent lecture at the Hospital of la Pitié M. Piorry called the attention of his class to a granular condition of the buffy coat of the blood, which he has observed only in cases in which pus was contained in some organ in considerable quantity, especially in the lungs. In these cases the buffy coat covering the blood contains rounded, grayish granulations, of a darker colour at the centre than at the circumference, varying from the size of a poppy to that of a hempseed. Sometimes five or six of these granulations are contained within the space of a square inch, at other times they are much more numerous. They are semitransparent at their borders, opaque at their centre. They are situated at different depths in the substance of the buffy coat, from which they can be detached with the point of a scalpel, though not easily. The buffy coat does not present any other peculiar appearance, and these granular bodies have never been observed in the clot.

This appearance is far from common: M. Piorry has not met with it above twenty times. In seventeen or eighteen of these twenty cases death took place;

and in every instance pus was found in the substance of some organ, especially of the lung, which, in fifteen cases was in a state of purulent infiltration.

Pneumonia with purulent infiltration of the lung existed in the patient whose case gave rise to the above remarks. The granulations are regarded by M. Piorry as collections of purulent matter; and he replies to M. Donné, who denies their resemblance, under the microscope, to the true pus-globule, that the circulation of pus in the blood must alter the characters of the former, and that the results of microscopic investigations are not of value sufficient to overturn facts gathered by clinical observation.

Gazette des Hôpitaux. Avril 16, 1842.

SURGERY.

On the Mechanism of Spontaneous or Symptomatic Luxations of the Femur. By M. J. Parise.

The author has in this paper very clearly and fully illustrated the opinion that collections of fluid, serous, synovial, or purulent, in the hip-joint are the ordinary cause of the symptomatic dislocation of the femur. We believe he takes too exclusive a view of the matter, and too much disregards other causes of luxation, as well as those cases in which, though the femur is still in the acetabulum, there are yet the signs of luxation; but of this class of luxations by accumulations of fluid, we have not before read so good an account. We shall therefore extract the newest and most important parts of it.

He overthrows satisfactorily all the objections advanced against E. H. Weber's account of the influence of the atmospheric pressure in maintaining the head of the femur in its place, and then shows the influence of artificial injections of the cavity of the hip-joint in forcing out the head of the femur. His plan was to bore through the thinnest part of the acetabulum obliquely, and then to inject fluid into it; the quantity which the capsule would hold being from forty to forty-five grammes of water. The constant results were: 1st, the femur was carried in the direction of flexion, abduction, and rotation outwards; the flexion being equal to an angle of from 30° to 35° with the plane of the horizon, and the rotation outwards being very slight; 2d, the great trochanter was forced outwards, and separated from the anterior superior spine of the ilium, and from the symphisis pubis, for more than half an inch; and was, at the same time, forced downwards, so that the limb was elongated about half an inch; 3d, the capsule was not distended equally, but was especially enlarged near the trochanters; and lastly the head of the femur was driven so far out of the acetabulum, that the distance of its summit from the bottom of the cavity, (as measured on an injection which solidified on cooling,) was upwards of two-thirds of an inch. The round ligament seemed to have but little influence on this displacement.

He relates an interesting case of a child twelve years old, in which it is quite clear that the luxation of the femur, where most of the signs of ordinary disease of the hip-joint had been present, was due to the accumulation of a serous fluid mixed with albuminous flocculi within the hip-joint. The cotyloid cavity and the head of the femur were uninjured, except that the latter was grooved by the pressure which it had experienced against the edge of the acetabulum. He believes that at a later period there would have been in this case the same disease of the bones and cartilages as is commonly seen in dislocations after disease of the hip-joint: [but this is, we think, very improbable; the femur and acetabulum would most probably have undergone only the same changes as after dislocation by external force. There is no proof here that the ordinary changes in diseased hips are, as the author suggests, consecutive to the dislocation.]

He enters upon a very elaborate yet clear account of the mode in which a fluid slowly secreted into the capsule of the hip-joint must act, and how it must produce, (as is evident from his experiments already mentioned,) first, elongation of the limb, real though but slight, and then the condition in which by the action of the muscles the femur may be drawn upwards and backwards. The peculiar positions of the limb, and the succession of their changes, he refers to three chief conditions: 1st. The dilatation of the capsule, which being inversely proportionate to the powers of resistance of its several parts, must be least in the situation of the accessory ligament on its anterior and inner aspect. This will restrain the movement of the head of the femur, which will therefore, as the capsule is distended, be carried outwards and upwards, while the shaft moves inwards and is flexed. 2d. The action of the pelvi-femoral muscles, of which those that lie next the capsule are like it distended by the accumulating fluid, and when they contract, since they all contract with nearly equal force, can have but little effect; while those which lie at a greater distance from the capsule, the adductors, glutei, &c. must by their contraction exercise an important influence in producing both the flexion and adduction, and, at last, the dislocation upwards and backwards of the limb. 3d. The position which the patient naturally assumes, and which is that best adapted for the avoidance of pressure on the diseased joint. For this purpose, he inclines over to the healthy side, and flexes his thigh and turns it inwards, in order that it may rest more easily on the other. At first, also, the round ligament assists in producing the flexion and adduction; and it resists, at the same time, the passage of the head of the femur upwards and backwards, but at length it gives way, and if the femur is pushed sufficiently outwards from the acetabulum, there is scarcely anything to prevent the muscles from pulling it upwards.

Lastly, the author explains the changes which he supposes to take place in the head of the femur after its luxation; [but these we think (as we have already said) he overrates, by presuming that a mere collection of fluid in the hip-joint without disease of the bones or cartilages is a common occurrence.]

Archives Générales de Médecine. Mai et Juin, 1842.

On a new Cranial Saw. By M. Bertherand.

The author has invented a saw for cutting the cranium, in examinations after death, and calls it a *cranial cyclotome.* It consists of a saw, concave at one edge and convex at the other, which, by means of a screw, can be turned in its long handle, so as to present either edge at will in the direction for cutting. It is also fitted with a copper conductor, which can be worked in the same way, and fixed at any required distance from the edge, so as to prevent the saw from passing too deeply through the skull.

Gazette des Hôpitaux. Juin 23, 1824.

Passage of Air into the Veins. By Dr. Asmus.

The author was removing a "steatoma" as large as the two fists from the region between the lower jaw and clavicle of a man forty years old, and was very carefully separating its base from the carotid artery with which it was in contact, when he accidentally opened the internal jugular vein, which had been pushed far from its usual place by a lobe of the tumour. No blood flowed; but on the instant he heard air enter the vein with a bubbling sound. He asked the man how he felt, who said "Well;" but in the next moment cried out, "Its all up!" and began to be convulsed, first in the face, and then in the whole body. He sunk down, and at the same instant another bubble was heard; but still no blood flowed. Alternate convulsive movements and opisthotonos ensued; the face was deadly pale, the breath short, and death seemed close at hand. Rapid bleeding now took place from the wound, and a stream of black blood was seen to issue from the vein, but as often as the patient was convulsed,

air again passed in, and the bubbling was distinctly both seen and heard. A ligature was as quickly as possible put upon the vein above the injured part, and with this the bubbling ceased; the tumour was cut off level and the patient was put to bed.

Syncope, alternating with severe convulsions, still continued; the pulse was not discernible, the heart seemed only to vibrate, and the respiration was short. Stimulants and a variety of restorative means were employed, and about twelve hours after the operation (in which the loss of blood was altogether moderate), the patient began to revive. His condition continued to improve, and he at length completely recovered. [This appears a well-marked case. At the time of opening the vein the patient had lost less than five ounces of blood, and was cheerful; at the instant the bubbling was heard the symptoms began, and the passage of air into the vein was distinctly observed by the eye and ear.]

Medicinische Zeitung. Juni 8, 1842.

Intra-parietal Hernia after a Wound of the Abdomen. By M. BERARD.

THE case detailed in M. Berard's clinical lecture affords a good example of an accident which is apt to occur not only in penetrating wounds of the abdomen, but in operations for hernia. In the endeavour to force the intestine (which had protruded) back into the abdomen, it was pushed up between the layers of abdominal muscles, and here, in the cavity thus artificially formed, became strangulated. The case was the more perplexing, because, when the intestine was in this position, the finger could be easily passed into the abdomen, and the intestine seemed to be entirely reduced.

Gazette des Hôpitaux. Juin 28, 1842.

Case of Strangulated Hernia through the foramen thyroideum.
By Dr. FRANTZ, of Genthin.

THE patient was a strong woman, forty years old, whom the author found with many of the signs of strangulated hernia, and complaining of a severe pain at the upper and inner part of the left thigh, which had come on suddenly and was increased in paroxysms at intervals of about ten minutes. There was no redness, heat, or swelling at the part, but on pressing the point of the finger high up between the triceps and adductor muscles, severe pain was produced. There was pain, but no tenderness, of the abdomen. The patient had long had double femoral hernia, but neither of these was now down. Three years before, she had had signs exactly like the present, but had been suddenly relieved when, as she was pressing upon the part, something seemed to go back with a noise into the abdomen. Since that time the same symptoms had occasionally recurred in a less degree, but they had been always relieved by the same plan of pressing, as if to reduce a hernia. On the present occasion, however, they were much more severe; bleeding, purgatives, repeated applications of pressure, and various other remedies were tried in vain. On the fourteenth day the signs of strangulation having regularly increased, and stercoraceous vomiting having existed since the ninth, the patient seemed to be quickly dying, when, to the surprise of all, a spontaneous evacuation of fæces took place, and she began slowly to recover. Her recovery was ultimately complete.

Allgemeine Medicinische Central-Zeitung. April 27, 1842.

Remarkable Case of Thoracic Fistula. By Professor RETZIUS, of Stockholm.

THE patient is a clarionet-player, who notwithstanding this diseased state, can follow his avocation and take long walks. He closes the opening in his side with a cork; and as often as he *uncorks* himself, air mixed with pus passes freely in and out through the aperture.

Tidskrift för Läkare, 1839, and *Schmidt's Jahrbucher*, No. 3, 1842.

Surgical Use of the Magnet.

IN the workshops of Fairbairne (in Belgium) there has been recently put up an artificial magnet of great power, at the level of the eye. Every instant one may see a turner, or an adjuster, or some other kind of workman, who has had a particle of iron driven into his eye, running to the magnet, which draws it out as soon as the eyelids are separated and the eye is held near its pole. One may conceive from this how a magnet might be made of sufficient power to draw a piece of iron even from the flesh or from the bones.

Gazette des Hopitaux. Juin 14, 1842.

Preservation of Nitrate of Silver. By M. DUMERIL.

FOR this purpose M. Dumeril has invented a plan. He melts in a vessel over a fire the best sealing-wax, containing a large quantity of gum-lac, and in this he dips the piece of caustic, over which he thus obtains a complete and firmly adherent varnish impermeable by the air or light, and completely preventing the staining of the fingers. In using the caustic all that is necessary is to scrape off with a penknife as much of the wax as is required to expose a surface of given size. Caustic thus coated is of course very convenient for introducing into cavities of which only a small portion is to be touched.

Bulletin Gén. de Thérapeutique. Mai, 1842.

A case of Exophthalmia, with Œdema of the Conjunctiva, and Opacity of the Crystalline Lens in a Puerperal Woman. By M. BLANDIN.

A woman, forty-one years old, was delivered, after a tedious labour, on December 3d, 1841. For fifteen days no unusual symptom occurred, but on the sixteenth and seventeenth day the patient was attacked by a violent shivering fit. On the eighteenth day, however, she returned from the hospital to her own home. and for some days afterwards suffered from febrile attacks, though they were no longer preceded by severe shivering. From the 25th of December the right eye began to project, the patient suffering little beyond a sense of weight in the head, principally in the supra orbitar region. Vision was at first unimpaired, but failed as the exophthalmia increased, and at last the patient became quite blind of that side. In this condition the patient applied to M. Blandin, at the Hôtel Dieu. There was then considerable prominence of the right eye, the conjunctiva of the globe was prominent, red, and swollen, and evidently infiltrated. The cornea was natural, the aqueous humour retained its transparency, and there was no evident change in the structure of the iris, but it had lost its contractility, and the eye was uninfluenced by exposure to a strong light. The crystalline lens appeared opaque, and of a shining, milk-white colour; the anterior membranes of the lens being in all probability the seat of the opacity. The volume of the globe was normal, the pain in the affected parts was inconsiderable, and no tumefaction existed of the parotid or cervical glands. The intellectual faculties were perfect and the general health was good.

In his remarks on the case M. Blandin offers some observations on the diagnosis of the affection. Some ramifications of the conjunctiva gave exit to a small quantity of pus from its inferior external portion. From that time the eye gradually retreated into the orbit, and from these circumstances M. Blandin concludes that there existed a small abscess behind the eye. The cause of the formation of this abscess is open to debate. It might be one of those purulent deposits occasionally met with in puerperal women. M. Blandin, however, regards it rather as the result of phlebitis, probably of the ophthalmic vein. He is likewise disposed to regard the affection as altogether analagous to *phlegmasia dolens,* in which disease the femoral vein becomes obliterated, just as here, in all probability, the ophthalmic vein was. On any other supposition the opacity of the capsule of the crystalline lens does not admit of explanation; while, in two other instances in which this lesion of the ophthalmic vein was discovered after death, precisely this condition of the crystalline lens had been noticed during the lifetime of the patient. *Gazette des Hôpitaux. Jan.* 27, 1842.

Observations on Fibrous Polypi of the Uterus. By M. A. Berard, of the Hôpital Necker.

On the occasion of a patient labouring under polypus of the uterus being admitted into the hospital, M. Bérard made some observations on the management of these growths.—The patient was a single woman, 47 years old, who had good health and menstruated regularly from the age of 11 to 24. From that time, however, till her 44th year menstruation became very scanty, but afterwards returned, being exceedingly abundant, and at length flowed continually in greater or less quantity. From this constant loss of blood the patient became at length much exhausted, and was admitted into the hospital in a state of complete anæmia. An examination per vaginam discovered the neck of the uterus to be distended, its orifice directed to the left side and slightly open, and the lips of the os uteri were thinned, and its cavity was occupied by a rounded oblong tumour, which was ascertained to be attached to the interior of the uterus.

With reference to the symptoms that were observed, M. Bérard remarks that Levret is mistaken in supposing that hemorrhage occurs only when the tumour having cleared the cervex uteri and descended into the vagina, the circulation in it becomes impeded by reason of the constriction of the polypus by the neck of the uterus. He is on the other hand disposed to regard these hemorrhages as active, and analogous to those which take place during the course of abortion, an analogy the more strongly marked, since in the one case as in the other uterine contractions take place. From a consideration of the symptoms he proceeds to an inquiry into the proper treatment, and decides in this case in favour of excision rather than of the ligature. The dangers of excision he regards as trivial while the great superiority which it possesses over the ligature in the speedy removal of the growth is universally admitted.

The operation which proved completely successful, was performed by placing the patient in the position for lithotomy. Two assistants kept the thighs properly bent, while a third made pressure on the hypogastric region. The operator having introduced a bivalve speculum, incised the two commissures of the cervix uteri with a probe-pointed bistoury, which permitted the polypus to enter the vagina. The speculum was then withdrawn, and two hooks being placed in the tumour it was gradually drawn down to the vulva. The operation was then completed by excising the pedicle of the polypus with a pair of curved scissors. The patient perfectly recovered.

Gazette des Hopitaux. Jan. 8, 1842.

On the Treatment of Leucoma by Incisions into the Cornea. By Dr. Holscher, of Hanover.

Two cases are related in which this treatment was adopted. The case which suggested it was that of a girl, twenty-two years old, who had lost the left eye from purulent ophthalmia in infancy, and in whom the right was almost blind from leucoma of nearly the whole cornea. Various means had been used in vain. The author, therefore, made an artificial pupil by drawing the iris through the cornea and excising a portion of it. Severe inflammation ensued which was with difficulty managed; but three months after, the patient not only had a good artificial pupil, but the cornea had become much less leucomatous, and this especially at the part where the incision through it had been made. The next bad case of leucoma, therefore, which the author met with, he treated as follows: The patient was a lad fourteen years old, who had lost his right eye from purulent ophthalmia in infancy, and had leucoma of nearly all the left cornea. At four different times, with intervals of eight days, a common cataract knife was passed into the cornea as deep as possible without penetrating it, and was drawn out with a sliding motion. After the first three times no inflammation ensued; therefore, after the fourth, some tinct. opii was dropped into the wound three times a day. Severe inflammation set in, but it was moderated by local bleeding, and the treatment by opium was continued for two

months. The leucoma became gradually less, and the patient who could at first only discern light from darkness, became able to guide himself in walking, and to perceive the window-frames in his room. The second case was that of a man forty years old, who had leucoma of one eye from gonorrhœal ophthalmia. It had been variously but vainly treated for a year. The author made incisions into the cornea twice, with an interval of fourteen days. After the second, a tolerably severe inflammation ensued which required active treatment. As soon as it had ceased, sulphate of zinc and tincture of opium were again dropped into the eye, and after a year and a half, not a trace of leucoma could be seen.

Holscher's Hannoversche Annalen. September, 1841.

On the local Employment of Calomel in Ophthalmia Neonatorum. By M. LAUER.

THE introduction of finely-powdered calomel into the eye in ophthalmia was originally practised by Dupuytren. Professor Fricke of Hamburg has noticed in his Zeitschrift for 1837, the violent reaction which its employment excites in the case of persons who have been taking iodine, probably owing to the formation of an iodide of mercury. About a year since Dr. Kluge began to use it as a local application in the cases of ophthalmia of new-born infants which came under his care in the lying-in department of the Charité at Berlin. The results were extremely fortunate, and Pr. v. Siebold of Göttingen, who was induced to try the remedy, has obtained from its employment very great success.

The manner of introducing the calomel into the eye is by means of a camel's hair pencil loaded with the powder, which is shaken from it into the eye, while an assistant separates the lids. In the treatment of the ophthalmia neonatorum this remedy may be had recourse to as soon as the first traces of the disease appear, and its employment once daily is then in general sufficient. After the lapse of from half an hour to two hours, according to the quantity of the secretion, the eye may be washed from the powder, and the ordinary rules as to cleanliness be attended to. In severe cases the application may be repeated twice every day; but when the disease is mild a single application daily suffices to effect a cure in from four to ten days, if the remedy had been had recourse to from the outset. The more severe and intractable forms of the disease do not appear to have been benefited by the local employment of calomel.

Medicinische Zeitung. Juni 8, 1842.

MIDWIFERY AND DISEASES OF WOMEN AND CHILDREN.

On the Management of Cases of Prolapsus of the Funis.
By Professor OSIANDER, of GÖTTINGEN.

AFTER a brief historical sketch of the various plans which have been adopted for the management of this accident, Professor Osiander relates several cases in illustration of the method of treatment to which he gives the preference. He sums up with the following rules, in estimating which, however, the partiality of the Professor for the long forceps must not be forgotten. He observes that the possession of a pair of forceps at least sixteen inches long is indispensable to the successful practice of midwifery. Professor Osiander's conclusions are:

1. Manual intervention is not required in every case of prolapse of the funis beyond the os uteri. It very frequently happens that the head passes beyond the funis, and that labour is terminated without any accident, though the case is left entirely to nature.

2. When the conditions are favorable, that is to say when the child is of moderate size, when the structure of the parts is natural, and the pains are effective, it is best to leave the case entirely to nature. Moderate pressure on the

cord is seldom dangerous in these cases any more than when the funis is twisted round the child. At the most, if the process of labour is slow it may be proper to apply the forceps.

3. Turning should not be resorted to unless some other circumstance than the prolapse of the cord renders it necessary. The old axiom that in all cases of funis presentation the child is to be turned is as ill-founded as it is mischievous.

4. Cessation of pulsation in the cord is not a certain sign of the death of the fœtus, and is rather an indication for hastening delivery than a reason for neglecting the condition of the child.

5. Attempts to replace the prolapsed funis within the uterus are seldom indicated; but on the contrary are almost always fruitless, while they are likely to interrupt and arrest the process of labour. If, however, the funis is low down in the vagina, or has descended out of it, it must be replaced, and retained within it by a sponge, a compress, or other means, since the action of the cold air speedily interrupts the circulation in the cord and occasions the death of the fœtus.

Neue Zeitschrift für Geburtskunde. Band xii. Heft i.

On the employment of Preparations of Quinine in the Diseases of Children. By M. Trousseau.

In a lecture delivered by him at the Hôpital Necker, M. Trousseau made some observations on the difficulty of administering the sulphate of quinine to children, owing to its nauseously-bitter taste. In reference to this he recommends the impure quinine (quinine brute), which is more active than the sulphate of quinine, is insoluble in the saliva, and has not much taste. One hundred parts of it contain sixty-five of pure quinine, thirty of cinchonine, and only five of water; while in one hundred parts of the sulphate of quinine there are thirty of water. This preparation is soluble in lactic acid, and is quickly changed into soluble salts in the stomach by the acids which exist in that viscus. Its resinous consistence is another advantage, since it admits of its being rolled into pills; in which form it may be mixed with sago, or given in any vehicle which contains no acids, since they would at once convert it into bitter and insoluble salts.

La Clinique des Hôpitaux des Enfans. Mai 15, 1842.

Account of a case of extra-uterine Pregnancy, the subject of which had attained her seventieth year. By M. J. Audry.

On September 25, 1841, a female was admitted into the hospital Cochin, under the care of M. Blache. She had given birth to one child at the age of 27 years, and had ceased to menstruate at fifty. She had always had good health till she was 40 years old, but then she became subject to dull pains which returned at irregular intervals and were referred to the left side and iliac fossa. After these pains had lasted for some years a prominence appeared at the left side and lower part of the abdomen, and for the last ten years the pain had been more severe, obstinate constipation had supervened, and the patient had grown very thin. At the date of her admission into the hospital she was in a state of exhaustion, so great that for several months she had kept her bed. Her abdomen was tender, tense, and tympanitic, and in the left iliac fossa was a large, hard, and irregular tumour, which was taken for an ovarian cyst. The patient died on the day of her admission.

On examining the body after death, signs of recent peritonitis with serous effusion mixed with shreds of lymph were found in the abdominal cavity. The great omentum was adherent to the borders of the left iliac fossa and covered the tumour, which occupied almost the whole of the cavity of the pelvis, and compressed both the rectum and bladder. In front of the tumour was a portion of the sigmoid flexure of the colon, in which no perforation could be perceived, but its parietes presented considerable friability. The uterus was situated at the

lower part and right side of the tumour, which resembled an enlarged ovary. None of the uterine appendages could be distinguished. The interior of the tumour contained the remains of a fœtus, all whose bones were connected together by imperfect ligaments which possessed very little firmness. The skeleton was rolled on itself in such a manner that the lower limbs occupied the posterior part of the tumour, while the occiput corresponded to its anterior part. More than two thirds of the mass were formed by the head, the bones of which overlapped each other, but formed a cavity containing a moderately firm white mass. The bones of the head were hard, and did not present any traces of cartilage, but they differed much from the form of fully developed bones. The articular extremities of the long bones were not formed, but they resembled cylinders somewhat enlarged at either end. The thigh bones were five centimetres and three millimetres, or about two inches long, the tibiæ four and a half centimetres. Very little of the soft parts remained, and, with the exception of a small portion of aorta, and a few pieces which presented a fibrous or membranous appearance, they formed a soft shapeless mass, in which no organ could be recognized.

The seat of the ovum in this case could not be ascertained, since the appendages of the uterus were not distinguishable. The age of the fœtus is another point which cannot be ascertained with precision, since, though the bones were fully ossified, their form differed from that which they present at the full term of utero-gestation. Their anomalous form was probably due to the cartilage which had originally covered them having been absorbed. No reason could be assigned for the occurrence of peritonitis after the abdomen had tolerated the presence of the cyst and its contents for so many years.

Archives Générales de Médecine. March, 1842.

Singular case. By Dr. RUSSEGGER.

A WOMAN, who had already borne four healthy children, was, in the seventh month of her fifth pregnancy, bitten in the right calf by a dog. The author saw the wounds made by the animal's teeth; which wounds consisted of three small triangular depressions; by two of which the skin was merely slightly ruffled; a slight appearance of blood was perceptible in the third. The woman was, at the moment of the accident, somewhat alarmed; but neither then nor afterwards had any fears that her fœtus would be affected by the occurrence.

Ten weeks after she had been bitten, the woman bore a healthy child, which, however, to the surprise of every person, had three marks corresponding in size and appearance to those caused by the dog's teeth in the mother's leg, and consisting, like these, of one larger and two smaller impressions. The two latter which were pale, disappeared in five weeks; the larger one also is not now so large or deep-coloured as it was at birth. The child is, at present, four months old. *Oesterreich. Medicinische Wochenschrift.* No. 19, *Mai* 7, 1842.

MEDICAL STATISTICS.

Statistics of Delirium Tremens in Berlin.

FROM November 1, 1840, to November 1, 1841, 106 patients suffering from delirium tremens, were admitted into the Charité at Berlin, of whom 24, or 1 in 5 died. Of these 106, 10 had been admitted once before for the same affection, 3 twice, 2 thrice, 2 four times, 1 five times and 1 twelve times before. Most stated that they were in the custom of drinking half a pint of spirits daily, but some confessed to a quart, and one woman, only 22 years old, said that she was in the practice of drinking three pints of spirits every day. Forty-five of the patients were day labourers, 41 were mechanics, 6 were women, and 6 were persons who were above the poorer classes. It is evident, however, that most were of that class whose opportunities for indulging their vice would be greatly curtailed by the imposition of a heavy tax upon spirits.

Allgemeine Medicin. Central Zeitung, 9 *April*, 1842.

II. THE AMERICAN AND COLONIAL JOURNALS.

ANATOMY AND PHYSIOLOGY.

Evolution of Electricity. By Dr. William Muller.

The author details some very curious results of experiments made by him, of which the following particulars are the principal:

Place the electrometer on the mantel-piece over a good fire. Take a common sized chair, with a back to it, of such a height, that the feet resting on the floor, the thighs shall be horizontal. Sit towards the front edge of the chair, and lean back, so as to have the trunk of the body quite relaxed; then rise quickly and touch the cover of the electrometer. The leaf, or leaves, will scarcely fail to indicate the presence of eletricity. If the first trial should fail, it will be owing to the non-observance of some of the above mentioned conditions; a second or third *must* succeed. The electrometer may also be placed on a table before the fire; the experimenter seated, as described, on a chair near it, may place his hand on the cover, and then, after leaning back, he should lean a little forwards and rise quickly, or but partially assume the erect position. At the instant of rising, and very often at that of sitting down after having risen thus, the electrometer will indicate a large amount of electricity. I have charged a jar with as much as could be detected by the instrument, by thus alternately rising and sitting. I have not, however, been able to cause the leaf to move more than a quarter of an inch by applying a jar thus charged by half a dozen risings and sittings; though, by keeping the finger on the electrometer while I thus rose and sat, I could, as I said before, cause a continual flight of the leaf to and fro through an inch and more. I have hitherto found my own electricity positive, and I have a suspicion that the electricity is different according as I rise up or sit down. This shall be decided by future experiments.

Prevost and Dumas pretend to have shown that electricity is produced during muscular contraction, and Edwards has shown that the same bodies which do or do not conduct electricity, do or do not conduct the nervous power, but no one till now, has observed the relation which exists between bodily motion in a particular direction, and a copious evolution of electricity.

Medical Examiner. No. viii. *Feb.* 19, 1842.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Chronic Pleuritis, with Ossification of the Diaphragm. By James Pagan, Esq.

Rohun, a convict, was sickly, emaciated, and unable to labour, although his tongue, skin, &c., were quite natural, and his appetite good. He could only speak in a gentle whisper. It appeared on inquiry at the native doctor, that the man had been often in the hospital during the last two years, with fever; but no mention was made of thoracic disease; although, on examination, the lower part of the right side of the chest was found to be dull. On inspection, there was found close to the sternum, a perfect cavity formed by a strong false membrane, which contained from four to six ounces of serum. The inner coat of this cavity was soft and jelly-like; but the outer was strong and thick. There were strong adhesions between the costal and pulmonary pluræ, and the right lung was considerably hepatized.

The portion of diaphragm, situated between the liver and right lung, was ossified to a considerable extent, so much so that it was difficult to remove or cut it with a knife.

[We have again to call attention to the deplorable typography of this Journal, and to object to the constant practice of transferring entire articles from our pages, without the civility of acknowledgment. The article on Bonnet and Thomson on Hepatic Disease, in No. 73, is a *verbatim et literatim* transcription from our columns, without the slightest indication as to the source from which it is obtained. We must also express our disappointment at the extreme pau-

city of original contributions in this periodical, which will account for the small number of "Selections" which we are enabled to make from it.]

India Journal of Medical and Physical Science. No. lxxi.. *Nov.* 1, 1841.

SURGERY.

Cancer of the Penis. By Dr. RIDLEY.

A gentleman, 85 years of age, who had always been healthy, but "whose habits had been for many years occasionally dissipated," complained, in 1840, of itching on the prepuce and glans penis, which were supposed to indicate lithiasis. These symptoms, however, disappeared; but about the same time a wart-like pimple, the size of a pea, and hard and red, was perceived about the insertion of the frenum into the glans. Little attention was paid to it, and it remained without any alteration for six months, when it began to extend itself. By and by, it implicated the whole glans. When the author was consulted the glans had been nearly eroded; the whole penis was involved in inflammation; the cellular tissue tumified; while about twelve lines from the scrotum was a sort of pap, with jagged edges, from which urine dribbled when the patient endeavoured to pass his water. The pain was unintermitting and excruciating. Amputation was proposed as the only remedy; but while the patient hesitated about giving his consent to this measure, suspicious tumours, filled with atheromatous or cheese-like matter, appeared in either groin. Subsequently one appeared on the pubis. The patient sank. There is no account of any post-mortem examination, and the whole case is vaguely reported.

Medical Examiner, [*Philadelphia, U. S.*] No. xliii. *Oct.* 23, 1841.

Iodine in Erysipelas. By Dr. BURNS.

THE author, after having tried the usual remedies fruitlessly, had recourse to the tincture of iodine (as originally prepared by Mr. Davies of Hertford), which he employed at first diluted with rectified spirit, in the proportions of ʒj of the tincture to ʒij of the spirit. Finding good effects from the application he then applied the pure tincture, pencilling the affected part with a camel-hair brush. The effects described are, subsidence of the tumefaction, corrugation of the epidermis, disappearance of the effusion in the loose cellular substance, and, in two days, desquamation, leaving a sound unbroken surface. The author describes it as surpassing any other remedy he had ever tried.

Medical Examiner. No. xlv. *Nov.* 6, 1841.

MIDWIFERY, AND DISEASES OF WOMEN AND CHILDREN.

Atresia Vaginæ. By Dr. MARSH, of Delaware.

A young woman, aged 18, complained of symptoms indicating dysmenorrhea. On inquiry, it was found that she had menstruated at a very early period, and had continued to do so regularly up till a few months previously. Examining per vaginam, the author discovered a strong septum, no doubt the result of inflammation, occluding the passage to the womb. This was divided, and the operation was followed by a jet of tar-like blood, which spouted to the height of one or two feet, and flowed to the extent of five or six pounds. The relief was complete.

[A nearly similar case is given in No. 5 (Jan. 29, 1842) of the same Journal. In this case, the existence of the septum, which was not complete, a small central part being patent, was only discovered during parturition. The practitioner resolved to leave the membrane untouched, "in order that he might observe how far the unaided efforts of nature would overcome the obstruction," and knowing that he "could at any time divide it." The membrane dilated, and that without rupture, sufficiently to allow the birth to take place: but the child came into the world asphyxiated, and was only recovered after great exertions. The practitioner's conduct was plainly reprehensible.]

Medical Examiner. No. i. *Jan.* 1, 1842.

III. THE BRITISH JOURNALS.

ANATOMY AND PHYSIOLOGY.

On the Physiology of Secretion and Absorption. By Messrs. Goodsir and Bowman. [We have great pleasure in directing the attention of our readers to the recent observations of Mr. Goodsir and Mr. Bowman (of which we subjoin the general results), as constituting a most important accession to our knowledge of the two functions by which the living organism is brought into relation with the world around. These observations are also of peculiar value, as demonstrating the utility of the microscope in anatomico-physiological research, a fact which the differences of opinion amongst observers, in regard to many questions of great delicacy, have led many to doubt.

We mentioned in our last Number (p. 291-3) the very probable theory (which, though extended and made more definite by Henle, was first propounded by Purkinje,) that the nucleated epithelium-cells of the gland-ducts are the real agents in the secreting process, elaborating from the blood certain of its constituents, just as do other cells, but subsequently discharging these by rupture or other modes; and we pointed out that, on this view, it is not possible to draw a strict line between secretion and nutrition. The first of Mr. Goodsir's papers may be considered as adducing almost demonstrative evidence of this theory (to which Mr. Bowman's observations on the fatty degeneration of the liver afforded no small support); whilst the second extends it to the process of selective absorption also. Mr. Bowman's researches into the minute anatomy of the kidney have most successfully determined a long-disputed question as to the nature and connexions of the Malpighian bodies; whilst they have also thrown great light on the mechanism (so to speak) of the renal secretion.

It is necessary that we should draw the attention of our readers *in limine* to an important distinction between *selective absorption* and *imbibition*, and to a difference of a similar kind between true *secretion* and *fluid exhalation*. Imbibition and exhalation of fluid are changes of a merely physical nature, depending upon the amount of fluid in the vessels and tissues of the body, and upon the permeability of these; and they take place, as is well known, in the dead as in the living body. Not only does water thus transude, but various substances which are held in complete solution in it, especially those of a saline character; and the quantity and nature of the fluids of the body may thus undergo a considerable change, without any purely vital function having any direct connexion with it. This change we believe to be chiefly accomplished through the medium of the blood-vessels, which absorb, through the capillary plexus of the villi of the intestines, perhaps a larger amount of fluid than is taken in by the lacteals; and which, as we shall presently see, get rid of what is superfluous, by an exhalation through the thin-walled capillary plexus of the Malpighian bodies of the kidney. On the other hand, *selective absorption* and *selective secretion* both seem to be performed by the agency of cells, which, during the progress of their development and growth, appropriate certain substances with which they may be in relation; and, when their term of existence is over (which is longer or shorter in different cases), they discharge their contents, in the one case into the neighbourhood of the absorbent vessels, and in the other into the channels of excretion. Of the reason why certain cells should perform these functions, we know no more than we do why certain others should be transformed into the several tissues of the body; but the one process is just as comprehensible as the other.]

1. *On the Ultimate Secreting Structure, and on the Laws of its Function.* By John Goodsir, Conservator of the Museum of the Royal College of Surgeons, Edinburgh.

After a brief statement of the history of opinion on this subject, the author re-

marks: "No one, so far as I am aware, has proved that secretion takes place within the nucleated cell, or has pointed out the intimate nature of the changes which go on in a secreting organ, during the performance of its function. It is the object of the present communication to supply this deficiency in physiological science." He then enumerates several instances in which he has found the secreting surface covered by cells that evidently contained the characteristic elements of the peculiar secretion. Amongst others we may mention the lining membrane of the ink-bag of the cuttle-fish—the terminal sacculi of the hepatic organs of numerous radiata, mollusca, articulata, and vertebrata—the generative caeca, whose cells contain spermatozoa—the colour-secreting glandulæ in the margin of the mantle of aplysia and janthina—and the mammary gland of the bitch. In all these instances, the character of the secreted product was easily recognizable by its appearance. In others, the secreted products are too transparent and colourless to be distinguished by the microscope; and we do not possess chemical tests of sufficient delicacy for the detection of such minute quantities. But the identity of the changes that can be proved to occur in the cellular elements of the organs that form the colourless secretions, together with the general argument drawn from the analogy, leave no doubt that the plan is the same.

The author then goes on to state the laws which he has deduced from numerous observations, as regulating the original formation, the development, and the disappearance of the primary organ. He contents himself in the present paper with giving two illustrations of the mode in which these laws apply to particular instances. One of these illustrations is drawn from the testicle of *squalus cornubicus*, which is taken by the author as the type of a class of glands in which the secretion is formed in the cells of their parenchyma; amongst which we presume that the human liver is to be included. The following is his summary of the process:

"1. The glandular parenchyma is in a constant state of change, passing through stages of development, maturity, and atrophy.

"2. This state of change is contemporaneous with and proportional to the formation of the secretion; being rapid when the latter is profuse, and *vice versá*.

"3. There are not, as has hitherto been supposed, two vital processes going on at the same time in the gland, growth and secretion; but only one, viz. growth. The only difference between this kind of growth and that which occurs in other organs being, that a portion of the product is, from the anatomical condition of the part, thrown out of the system.

"4. The vital formative process which goes on in a gland is regulated by the anatomical laws of other primitive cellular parts.

"5. An acinus is at first a single nucleated cell. From the nucleus of this cell others are produced. From these, again, others arise in the same manner. The parent cell, however, does not dissolve away, but remains as a covering to the whole mass, and is appended to the extremity of the duct. Its cavity, therefore, as a consequence of its mode of development, has no communication with the duct.—The original parent-cell now begins to dissolve away, or to burst into the duct at a period when its contents have attained their full maturity. This period varies in different glands, according to a law or laws impressed upon each of them.

"6. In the gland there are a number of points from which acini are developed, as from so many centres. These I denominate the germinal spots of the gland.

"7. The secretion of a gland is not the product of the parent-cell of the acinus, but of its included mass of cells. The parent-cell or vesicle may be denominated the primary cell; its included nucleated cells, after they have become primary secreting cells, may be denominated secondary cells of the acinus.

"8. The matter which passes off by a duct of a gland may be, 1st, a true secretion, that is, matter formed in the primary secreting cell cavities; or 2d, a mixture of a fluid formed in these cell cavities, with the developed or undeveloped nuclei of the cells themselves; and 3d, it may be a number of secondary cells passing out entire."

The author then gives a similar history of the process of secretion as it takes

place in the cells lining the hepatic cæca of *carcinus mœnas*, which may be taken as the type of the follicular glands; such cells may be regarded in the light of an epithelium, as in the tubes of the human kidney.

"1. Each follicle is virtually permanent, but actually in a constant state of development and growth.

"2. This growth is contemporaneous with the function of the gland, that function being merely a part of the growth, and a consequence of the circumstances under which it occurs.

"3. Each follicle possesses a germinal spot situated at its blind extremity.

"4. The vital action of some follicles is continuous, the germinal spot in each never ceasing to develope nucleated cells, which take on the action of and become primary secreting cells as they advance along the follicle. The action of other follicles is periodical.

"5. I have not been able to satisfy myself, but I am inclined to believe, that the wall of the follicle also is in a state of progressive growth, acquiring additions to its length at its blind extremity, and becoming absorbed at its attached extremity. A progressive growth of this kind would account for the steady advance of its attached contents, and would also place the wall of the follicle in the same category with the primary vesicle or wall of the acinus in the vesicular glands.

"6. The primary secreting cells of the follicle are not always isolated. They are sometimes arranged in groups; and when they are so, each group is inclosed within its parent cell, the group of cells advancing in development according to its position in the follicle, but never exceeding a particular size in each follicle."

The author, lastly, points out the analogy between this history of the growth of a gland during its state of functional activity, and that of its embryonic development; and he closes with two important suggestions: 1st, that the follicle, like the acinus, is originally a single nucleated cell, and is the source of the germinal spot which plays so important a part in the subsequent operations; 2d, that the ducts of glands are intercellular passages, like those which contain the products of secretion in plants.—*Transactions of the Royal Society of Edinburgh*, 1842.

II. *On the Structure of the Intestinal Villi in Man and certain of the Mammalia, with some observations on Digestion, and the Absorption of Chyle.* By John Goodsir.

Having fed a dog with oatmeal, milk, and butter, the author examined the intestinal villi three hours afterwards, when the lacteals were turgid with chyle, and the gut full of milky chyme mingled with a bilious-looking fluid. In the white portion of the fluid, which was situated principally towards the mucous membrane, numerous epithelium cells were found; some of which had evidently (from their form) been detached from the surface of the villi, whilst others had been thrown off from the interior of the follicles of Lieberkuhn. The villi were turgid, and destitute of epithelium except around their bases. Each villus was covered by a very fine smooth membrane, continuous with what Mr. Bowman terms the basement membrane of the mucous surface, which is reflected into the follicles. The villi were semi-transparent, except at their free or bulbous extremities, where they were white and nearly opaque. The summit of each villus was crowded, beneath the enveloping membrane, with a number of perfectly-spherical vesicles, varying in size from 1-1000th to 1-2000th of an inch; the matter in the interior of which had an opalescent milky appearance. At the part where these vesicles approached the granular texture of the substance of the villus, minute granular or oily particles were situated in great numbers. The trunks of two lacteals could be easily traced up the centre of the villus; and as they approached the vesicular mass, they subdivided and looped; but in no instance could they be seen to communicate directly with any of the vesicles.

These vesicles can scarcely be considered in any other light than as cells, whose lives have but a very brief duration, selecting from and appropriating the materials in contact with the surface of the villi into their own substance, and then liberating these, by solution or disruption of the cell-wall, in a situation where they can be absorbed by the lacteals. When the gut contains no more chyme,

the development of new vesicles ceases, the lacteals empty themselves, and the villi become flaccid. During this interval of repose, the epithelium is renewed, for the protection of the surface of the villi, and for the secreting function of the follicles of Lieberkuhn. It is considered by Mr. Goodsir that the epithelium-cells have their origin in certain nuclei which he detects scattered through the basement membrane.

There appears to be a strong resemblance between the process of absorption in animals, as thus explained, and that which takes place in plants through the medium of the spongiole.—*Edin. New Philosophical Journal*, July, 1842.

III *On the Structure and Use of the Malpighian Bodies of the Kidney, with observations on the Circulation through that Gland.* By WILLIAM BOWMAN, F.R.S. Demonstrator of Anatomy in King's College, London.

[The Malpighian bodies have excited much interest amongst anatomists, from the time of their first discovery; but no satisfactory account has yet been given of their structure and vascular connexions Still less has their true relation with the uriniferous tubes been ascertained; in fact, Müller and Huschke, two of the latest inquirers on the subject, have positively denied the existence of any such relation. The following is the account of Mr. Bowman's researches given in the Proceedings of the Royal Society.]

"The author describes the results of his examination of the structure and connexions of the Malpighian bodies of the kidney in different tribes of vertebrata, and shows that they consist essentially of a small mass of vessels contained within dilated extremities of the convoluted uriniferous tubes. The tubes themselves consist of an outer transparent membrane (termed by the author the *basement membrane*) lined by epithelium. This basement membrane, where it is expanded over the tuft of vessels, constitutes the capsule described by Muller. The epithelium lining the uriniferous tube is altered in its character where the tube is continuous with the capsule, being there more transparent, and furnished with cilia, which, in the frog, may be seen, for many hours after death, in very active motion, directing a current down the tube. Farther within the capsule the epithelium is excessively delicate, and even in many cases absent. The renal artery, with the exception of a few branches given off to the capsule, surrounding fat and coats of the larger blood-vessels, divides itself into minute twigs, which are, the afferent vessels of the Malpighian tufts. After it has pierced the capsule, the twig dilates, and suddenly divides and subdivides itself into several minute branches, terminating in convoluted capillaries, which are collected in the form of a ball; and from the interior of the ball the solitary efferent vessel emerges, passing out of the capsule by the side of the single afferent vessel. This ball lies loose and bare in the capsule, being attached to it only by its afferent and efferent vessel, and is divided into as many lobes as there are primary subdivisions of the afferent vessel; and every vessel composing it is bare and uncovered, an arrangement of which the economy presents no other example. The efferent vessels, on leaving the Malpighian bodies, enter separately the plexus of capillaries surrounding the uriniferous tubes, and supply that plexus with blood. The blood of the vasa vasorum also probably enters this plexus. The plexus itself lies on the outside of the tubes, on the deep surface of the membrane which furnishes the secretion; and from it the renal vein arises by numerous radicles.

Thus the blood, in its course through the kidney, passes through two distinct systems of capillary vessels; first, through that within the extremities of the uriniferous tubes, and secondly, through that on the exterior of these tubes. The author points out striking differences between these two systems. He also describes collectively, under the name of *Portal System of the Kidney*, all the solitary efferent vessels of the Malpighian bodies, and compares them with the portal system of the liver, both serving to convey blood between two capillary systems. In the latter a trunk is formed merely for the convenience of transport, the two systems it connects being far apart But a portion even of this has no venous trunk, viz. that furnished by the capillaries of the hepatic artery throughout the liver, which pour themselves either into the terminal branches of the portal vein,

or else directly into the portal-hepatic capillary plexus. On the other hand, in the kidney, the efferent vessels of the Malpighian bodies, situated near the medullary cones, having to supply the plexus of the cones, which is at some little distance, are often large, and divide themselves after the manner of an artery. They are portal veins in miniature. In further confirmation of his view of the existence of a true portal system in the kidney of the higher order of animals, where it has never hitherto been suspected, the author describes his observations on the circulation through the kidney of the boa constrictor, an animal which affords a good example of those in which portal blood derived from the hinder part of the body traverses the kidney. He shows that here the Malpighian bodies are supplied, as elsewhere, by the artery, and that their efferent vessels are radicles of the vena portæ within the organ, and join its branches as they are dividing to form the plexus surrounding the tubes; thus corresponding with the hepatic origin of the great vena portæ. In other words, the vena portæ is an appendage to the efferent vessels of the Malpighian bodies, and aids them in supplying blood to the plexus of the tubes. Thus in this variety of the kidney, as in the liver, there is an internal as well as an external origin of the portal system; while in the kidney of the higher animals, this system has only an internal or renal origin, viz., that from the Malpighian bodies.

A detail of the results of injection by the arteries, veins, and ducts, is then given, and they are shown to accord with the preceding description. Many varieties in the Malpighian bodies in different animals are also pointed out, especially as regards their size.

The author then proceeds to found on his previous observations, and on other grounds, a theory of a double function of the kidney. He conceives that the aqueous portion of the secretion is furnished by the Malpighian bodies, and its characteristic proximate principles by the walls of the tubes. After giving in detail his reasons for entertaining this view, he concludes by referring to the striking analogy between the liver and kidney both in structure and function, and by expressing his belief, first, that diuretic medicines act specially on the Malpighian bodies, and that many substances, especially salts, which when taken into the system have a tendency to pass off by the kidneys with rapidity, in reality escape through the Malpighian bodies; secondly, that certain morbid products occasionally found in the urine, such as sugar, albumen, and the red particles of the blood, also, in all probability, pass off through this bare system of capillaries."—*Proceedings of the Royal Society*, No. lii. *Feb.* 3, 1842.

Guided by the discovery of the normal anatomy and physiology of this organ, Mr. Bowman is now engaged in the elucidation of its morbid structure in various diseases, especially the morbus Brightii; and we can scarcely doubt that he will be equally successful. Without committing himself to any premature opinions, he informs us that the scattered red spots which are frequently seen in the cortical substance during the earlier stages of the disease (when blood is often passed with the urine, and many circumstances contribute to prove that the kidneys are in a state of sanguineous turgescence,) are certainly *not enlarged Malpighian bodies*, as they have been recently stated to be, but convoluted uriniferous tubes filled with blood that had burst into them from the gorged Malpighian tufts at their extremities. They are seen on the surface where no Malpighian bodies exist; and they are much larger than these could be supposed to become under any amount of distention.

[In conclusion, we again desire to call the attention of our readers to the papers themselves, and especially to the last. It is one of the most complete and satisfactory elucidations of an obscure question, with which we are acquainted, and is a model for all similar inquiries.]

Best Mode of conducting Physiological Researches. The author is opposed to that school of physiologists called *experimental*, whose rule is to withhold belief from every deduction drawn from anatomical structure, and to consider such deduction, hypothetical and unproved, until they have been over and over again

tested on living animals. He, on the contrary, holds that, by drawing inferences from the anatomical structure, and adopting as our guides certain acknowledged principles acquired from the same source, we might obtain conclusions the truth of which the most sceptical could scarcely deny. The author, with considerable ingenuity, illustrates his meaning as follows:

1. Reason alone suggests that a nerve of motion should be anatomically distinct from a nerve of sensation. As these influences travel in contrary directions—that which induces muscular action being propagated *outwardly* from the brain, and that which gives rise to sensation being conveyed *inwardly* to the brain—it surely appears highly probable, at least, that each property must belong to a distinct structure.

2. The ninth pair has long been considered a motor nerve. Let us enquire what has led former physiologists to form that opinion. This nerve supplies the tongue, an organ endowed with motion and with sensibility: but it is remarkable that it is limited in its distribution to the muscular substance alone, and that it avoids sending any of its branches to the surface, which is therefore supplied by a different nerve. Now this circumstance, viz. that the ninth pair is limited to the supplying of *muscles*, carries to my mind as distinct a proof that it is subservient to motion, and that it has been correctly called the motor of the tongue, as the origin and insertion of the biceps muscle of the arm do, that the action of its fibres is to bend the forearm of the arm.

3. If we observe the peculiar structure, and also the exact point of origin, of the ninth pair, we shall be convinced that it is in truth an anterior root of a spinal nerve. In no one character can we recognise a difference between them. In short, the ninth nerve is in every point of view like a spinal nerve, which is deficient in a posterior root. May we not therefore infer that the anterior roots of the spinal nerves have a similar function to that of the ninth pair: in other words that they are nerves of motion?

4. Guided by these views, which naturally lead us to the belief that the whole line of nerves from the ninth downwards, arising from the anterior column of the spinal marrow, are subservient to motion, we look with natural curiosity to those nerves which originate from the same column prolonged into the brain.

The first nerve which meets us is the sixth pair, a nerve that goes to a muscle alone; the next is the third pair, which likewise goes to muscles exclusively. Now in the same manner as it was inferred from the distribution of the ninth pair, that its function was to confer motion, so it may be concluded that these two nerves are destined for the same purpose. Indeed the fact cannot be forgotten, that to one of them the name *motor oculi* has been applied for centuries past. The course of observation hitherto pursued consequently leads us to infer, that the whole series of nerves arising from the anterior column of the spinal marrow, from the third of the brain to the lowest nerve of the cauda equina, is destined for the giving of motor pewer.

So much for the uses of the anterior roots; let us next briefly attend to those of the posterior roots.

5. The fifth cerebral or trigeminus nerve is chiefly remarkable, in its anatomical character, for having an exact resemblance to the spinal nerves. This resemblance, it may be remarked, attracted the attention of anatomists for some time before the recent discoveries in the nervous system were thought of. The peculiar character which distinguishes the fifth from all the other nerves of the brain, is that it originates by two roots,—one of which corresponds with the anterior roots of the spinal nerves in having no ganglion upon it, while the other roots has a ganglion exactly similar in structure to that formed on the posterior roots of the spinal nerves. Our line of argument therefore, leads us to inquire whether, in the anatomical arrangement of the roots of the fifth pair, anything presents itself which can throw light on their distinct endowments. Now there is a circumstance in the anatomy of this nerve which is greatly in favour of our obtaining the desired knowledge. Instead of both its roots being of equal dimensions, that which has a ganglion, is about four times as large as that which is without one; and the former

can therefore be traced to many parts of the head where it is not accompanied by the latter. If we consider the lesser roots, the analogue of the anterior roots of the spinal nerves, in the first place, and trace it to its destination, we find that it is distributed exclusively to muscles—it is the nerve proper to the muscles of the jaws. Indeed Paletta, resting on the circumstance of this root not going exclusively to the muscles, so far anticipated the present views in regard to the physiology of the fifth, as to call this root a motor nerve, and to represent it as the nerve on which *trismus* depended. Accordingly, the observation of the simple distribution of this smaller root of the fifth sanctions the conclusion that, inasmuch as it resembles the anterior roots of the spinal nerves, and I may add, the ninth pair, as well as the sixth, and the third, in its structure, it is in all probability identical with them in function—namely, in being a nerve of motion.

I proceed next to the larger root, which from having a ganglion resembles the posterior roots of the spinal nerves; and will endeavour to show how far the distribution of its branches tends to elucidate the nature of its functions.

What principally strikes an anatomist, when surveying the course of the several great branches derived from this larger root, is that they supply numerous parts of the head where no muscles exist, and when there are only sensible surfaces. I will put aside as doubtful the branches which emerge upon the face, since the muscles of the features appear to be supplied by these branches in common with the portio dura.

But taking other branches, we find that several pass to localities where no muscles are situated, and where of course we cannot suppose that nerves of motion should be sent. For example, we have the cavities of the nose liberally supplied with branches proceeding from the ganglionic root; we have branches of considerable size from the same origin going to the hard palate: branches of the same kind go to the teeth: and lastly, there is a large branch, the proper destination of which is manifestly the sensible surface of the tongue.

These facts, coupled with the circumstances that the whole extent of the integuments of the head, (except a part behind, which is supplied by spinal nerves), has branches of this root sent to them, afford by themselves strong presumptive evidence that the office of the ganglionic root of the fifth pair is to confer sensation as distinct from motion.

Now, if the inferences stated above be thought justly deducible from the premises, they may be applied legitimately to the explanation of the functions possessed by the posterior roots of the spinal nerves. We are thus led to conclude that, as the function of the larger or ganglionic root of the fifth pair is to bestow sensation, so it is the function of the posterior or ganglionic roots of the spinal nerves also to bestow sensation.

In this simple manner have we been brought, without the performance of a single experiment on a brute creature, but by relying exclusively on anatomy as our guide, to educe the beautiful truth, that the anterior roots of the spinal nerves, including their analogies in the encephalon, are subservient to motion, and the posterior roots of the spinal nerves, together with the ganglionic root of the trigeminus, to sensation.—Mr. Shaw, *Medico-Chirurgical Review*, No. 72, *April*, 1842.

The smallest Blood-corpuscles of Mammals. In a paper read at the Zoological Society of London, Aug. 9, 1842, Mr. Gulliver remarked that, before his discovery of the singularly minute size of the blood-discs of the musk-deer, (See Med. Chir. Trans. v. 23—Dublin Medical Press, Nov. 27, 1839—Philos. Magaz. Dec. 1, 1839,) those of the goat were the smallest known; and he announced that the blood-corpuscles of the ibex are slightly smaller than those of the goat, and therefore intermediate in size to the corpuscles of the goat, and those of the musk-deer. The following average measurements of the discs were given in fractions of an English inch.

Goat (*Capra Hircus, Linn.*)	$\frac{1}{6366}$
Ibex (*Capra Caucasica ?*)	$\frac{1}{6855}$
Musk-deer (*Moschus Javanicus, Pallas,*	$\frac{1}{12325}$

Geo. Gulliver, F.R.S.

ON THE FIGURE OF THE OVAL BLOOD-CORPUSCLES OF VERTEBRATA. Though the usual figure of the blood-discs of the camelidæ, and of the oviparous vertebrata is an ellipse, of which the long diameter is from one and a half to twice the short diameter, Mr. Gulliver shows that there are many exceptions to this rule. In other words, the corpuscles may either be comparatively long and narrow, or short and broad. Of the former shape, M. Mandl sometime ago stated, that the corpuscles of the crocodilidæ were a singular example, since he found that the length of the corpuscles of *crocodilus lucius* was nearly three times as much as their breadth. Subsequently, (Proc. Zool. Soc. Nov. 10, 1840,) however, Mr. Gulliver found that the blood-discs of *crocodilus acutus*, and of *champa fissipes* had the more common elliptical figure first mentioned.

As examples of the elongated ellipse he has since pointed out the corpuscles of the snowy owl, passenger-pigeon, great butcher-bird, nightingale, and snow-bunting; and of the short and broad ellipse, the corpuscles of the Java sparrow and of some other granivorous birds.

Lastly, the corpuscles of the British ophidian reptiles are examples both of the ordinary and of the more elongated ellipse; for while the blood-discs of the slow-worm are long and narrow in figure, those of the snake and viper do not differ from the common shape, as the following average measurements in fractions of an English inch will show.

	Long diameter	Short diameter
Slow-worm (*Anguis fragilis*, Linn.)	$\frac{1}{1178}$	$\frac{1}{2666}$
Snake (*Natrix torquata*, Ray)	$\frac{1}{1371}$	$\frac{1}{2157}$
Viper (*Coluber Berus*, Linn.)	$\frac{1}{1274}$	$\frac{1}{1800}$

From a paper read by Mr. GULLIVER at the Meeting of the *Zoological Society of London*, August 9, 1842.

ON THE LYMPH-GLOBULES OF BIRDS. In birds, as in mammals, the lymph-globules are rather smaller than the pale globules of the blood; yet, since the time of Hewson, who describes the lymph particles of birds as oval, like the nuclei of the blood-discs, the description of the lymph-globules of birds has been drawn from the pale globules of the blood.

Mr. Gulliver figures the particles obtained from the lymphatic glands of birds as globular, which corresponds with his observations on the lymph-globules of the camelidæ and of the musk-deer. (Lancet, 1840-41, and Med. Chir. Trans. v. 23.) For numerous measurements of the lymph-globules of birds the original paper may be referred to. The pus-globules of the camelidæ are also circular.—GEORGE GULLIVER, F.R.S.; *Lond. and Edinb. Phil. Mag., June,* 1842.

MUSCULAR FIBRE OF THE GULLET. In a paper read at the Zoological Society, June 14, 1842, Mr. Gulliver mentioned the following as some of the results of his inquiries.

1. In man, the quadrumana, viverridæ, felidæ, the porpoise, the horse, &c., the muscular fibre of animal life terminates on the gullet at a greater or less distance before it reaches the stomach.

2. In the insectivorous feræ, in the ursidæ, mustelidæ, ruminantia, rodentia, &c., the muscular fibre of animal life runs along or forms a complete investment to the entire length of the gullet, and sometimes extends a short distance on the cardiac end of the stomach.

3. In birds and reptiles the gullet has no covering of the muscular fibre of animal life.

4. The statement therefore of Professor Müller, that "the third act of deglutition is perfectly involuntary, being performed by the muscular fibres of the œsophagus, which are not in the slightest degree capable of voluntary motion," is regarded by Mr. Gulliver as applicable to some animals, but not to others, especially those referred to in part 2.

On the Nuclei of the Blood-corpuscles of Vertebrata. The author gives two figures to illustrate the effects of several reagents, and especially of repeated washing with water, till all the colouring matter is removed, on the corpuscles of mammalia and of the lower vertebrata. The corpuscles of man for example, as Mr. Gulliver had formerly stated, are reduced about one third or one fourth in size after completely removing all their colouring matter by repeated additions of large quantities of water. These washed corpuscles appear very faint and pellucid, presenting no appearance of a nucleus, even when treated with acids and other reagents; nor do the washed blood-discs agree in any respect with the particles which have commonly been described as the nuclei of the blood-corpuscles. Now when all the colouring matter is washed away in like manner from the corpuscles of any of the lower vertebrata, both the envelopes and nuclei remain, and are plainly distinct parts, both appearing circular; although the nucleus, when exposed by acetic acid, has an oval figure. The effect of acids is equally different on the blood-discs of mammals, and on the discs of the lower vertebrate animals; and the oval corpuscles of the camelidæ agree in structure with the corpuscles of other mammals.—G. Gulliver, f.r.s.; *Lond. and Edin. Phil. Mag. for Aug.* 1842.

Structure of Fibrin. The red clots of fibrin found in the dead body inclose corpuscles similar, though of a ruddy colour, to the pale organic germs depicted by Mr. Gulliver in colourless clots of fibrin. (Appendix to Gerber's Anatomy.) Both kinds of corpuscles may be altered blood-discs; but if so, it is remarkable that both the ruddy and pale organic germs of fibrinous clots must have undergone great changes in form and chemical characters; these germs exhibit nuclei when treated with acetic acid, &c., while precisely the same treatment does not show any nuclei in the free or floating blood-discs. A figure is given of the germs contained in a network of fibrils; these latter are also shown in fibrin obtained by washing from the blood of a bird; and this fibrin is pervaded by particles like the nuclei of the blood-discs.—G. Gulliver, f.r.s.; *Phil. Mag. for Aug.* 1842.

Chemical Characters of the Spermatozoa, Molecules of the Semen. In a paper read at the Zoological Society, July 26, 1842, Mr. Gulliver stated that, in his experiments, the spermatozoa of mammalia were but little, or not at all, affected by nitric, muriatic, acetic, oxalic, tartaric, and citric acids; while the *spiral* spermatozoa of birds were very susceptible of the action of the vegetable acids, although the *cylindrical* spermatozoa of birds were nearly allied to those of mammalia in chemical characters; and that the chemical characters of the spermatozoa of man might probably be turned to account in medical jurisprudence.

The molecules of the semen he thinks may be connected with the perfecting of the semen, as they are to be found in small numbers at all times in that of healthy men, and in great abundance in that of birds and reptiles just before the testicles become ripe, and disappear or become scanty when the spermatozoa are completely formed. The molecules were described as resembling, in form and chemical characters, the minute particles which he has figured (Gerber's Anatomy,) in the juice of the supra-renal glands.

Millipedes discharged from the Human Stomach. A boy, fifteen years of age, had complained of stomachic derangement. At last he had an emetic of antimonial wine; after which he vomited a common-sized teacupful of woodlice, (millipedes,) an insect of the genus scolopendra. They were mostly alive and grown, but wanted the brown colour of those found in natural situations, being white. Their ova had been probably swallowed by the boy in his food. These insects often burrow in bacon.—Dr. A. W. Davis, Presteign; *Prov. Med. Journ.* No. xix. *Aug.* 13, 1842.

On the Chemical Analysis of the contents of the Thoracic Duct in the Human Subject.—The author, availing himself of a favorable opportunity which presented itself of examining the contents of the thoracic duct in a human subject, procured an hour and a quarter after death by hanging, to the amount of six fluid drachms, obtained by analysis the following result:—

Water, per cent.	90·48
Albumen, with traces of fibrinous matter	7·08
Aqueous extractive, or zomodine	2·56
Alcoholic extractive, or ozmazome	0·52
Alkaline chloride, carbonate and sulphate, with traces of phosphate, and oxide of iron	0·44
Fatty matters	0·92
	100·

The fatty matters possessed the same general characters as those of the blood, except that they did not contain phosphorus, as appeared from their yielding an alkaline, instead of an acid ash by incineration. The aqueous extractive differed from that of the blood by giving a ferruginous ash. The salts obtained by incineration from the alcoholic extractive yielded a larger proportion of alkaline carbonate than those of the blood. The author is confirmed, by the experiments he made on the present occasion, in his former views concerning the cause of the white colour of the chyle, which he ascribes to the presence of opaque white salivary matter as one of its constituents. The author then gives the results of his microscopical examination of the globules of the chyle, which he finds differ totally from those of the blood. He points out as being remarkable the large quantity of fatty matter existing in the chyle, and constituting an hydrocarbonaceous ingredient, which is constantly being added to the mass of blood, and is very rapidly consumed, as appears from the small quantity of this matter discoverable in the blood itself. The proportional quantity of ozmazome in the chyle he finds greatly to exceed that contained in the blood.—George Owen Rees, m.d.; *Proceedings of the Royal Society,* No. 52, *Feb.* 10, 1842.

Extrophy of the Bladder. The hypogastrium was occupied by a tumour of a deep red colour, the size of a half section of a moderately large orange. Its surface was uneven and undulated, with several nodules; on each side of the inferior one were the orifices of the ureters. A thin, reddish fluid oozed from the whole surface; and, on irritating the orifices of the ureters with a probe, a small stream was ejected to a considerable distance. At the inferior part of the nymphæ was an opening, which resembled the urethra, but was, in reality, the vagina. There were neither clitoris or urethra. The child died seventeen days after birth, of cellular inflammation, which, commencing in the right leg, extended to every part of the body.

The pubis on each side was deficient. A prolapsis ani existed to some extent. During life the child had almost continual forcible straining. Was this owing to the deficiency of the pubis?

The peculiarities in this case were, its occurrence in a female, the undulated appearance of the vesical tumour, and the absence of the clitoris and urethra.—T. Harrison, Esq.; *Lancet, June* 25, 1842.

Of the Ultimate Distribution of the Air-passages, and of the Modes of Formation of the Air-cells of the Lungs. After reciting the various opinions which have prevailed among anatomists regarding the manner in whicl the bronchial tubes terminate, whether, as some suppose, by cells having free communication with one another, or, as others maintain, by distinct and separate cells having no such intercommunication, the author states that having been engaged in investigating, with the aid of the microscope, the seat and nature of pul-

monary tubercles, he could never discover, in the course of his enquiry, any tubes ending in a *cul-de-sac;* but, on the contrary, always saw, in every section that he made, air-cells communicating with each other. He concludes from his experiments and observations, that the bronchial tubes, after dividing dichotomously into a multitude of minute branches, which pursue their course in the cellular interstices of the lobules, terminate, in their interior, in branched air-passages, and in air-cells which freely communicate with one another, and have a closed termination at the boundary of the lobule. The apertures by which these air-cells open into one another are termed by the author *lobular passages:* but he states that the air-cells have not an indiscriminate or general intercommunication throughout the interior of a lobule, and that no anastomoses occur between the interlobular ramifications of the bronchiæ themselves; each branch pursuing its own independent course to its termination in a closed extremity. Several drawings of the microscopical appearances of injected portions of the lungs accompany this paper.—WILLIAM ADDISON, F.L.S., Surgeon, Great Malvern; *Proceedings of the Royal Society,* No. 53, *March* 17, 1842.

RELATIVE SIZES OF THE TRUNKS AND BRANCHES OF ARTERIES. Ratio of the area of each arterial trunk to the joint area of its branches, or of its branches and continuation:

	Trunk.		Branches.
Arch of the aorta . . .	1	:	1·055
Innominata . . .	1	:	1·147
Common carotid . . .	1	:	1·013
External carotid . . .	1	:	1·19
Subclavian . . .	1	:	1·055
Abdominal aorta to the last lumbar arteries	1	:	1·183
Abdominal aorta just before dividing .	1	:	·893
Common iliac . . .	1	:	·982
External iliac . . .	1	:	1·15

From this it follows that the notion of earlier anatomists, that the arterial canal enlarges as trunks divide into branches, is true, though the enlargement is less than was supposed. There is, however, one constant exception to the law now stated: namely, where the aorta divides into the common iliac arteries; for there, or at the division next lower down, the stream is always contracted.

The effect of such an arrangement must be to increase the velocity of the current, not only in the iliac arteries themselves, but in the arteries given off from the trunk above them, such as the mesenteric of the renal; and it is, surely, not improbable that the acceleration of the circulation through the kidneys, and through the organs from which the roots of the portal vein are derived, is the special purpose which so singular an arrangement serves.—JAMES PAGET, Esq., of St. Bartholomew's Hospital; *Medical Gazette, July* 8, 1842.

THE PERIOD OF PUBERTY IN NEGRO WOMEN. The author maintains that the notion which generally prevails, of puberty being earlier in the negro than in the white races, and in hot than in temperate climates, is "no better than a vulgar error." He also regards as equally unfounded the opinion generally entertained, that certain mental and physical influences—as the modes of life of the richer and more luxurious classes, and working in heated factories—have an effect of hastening puberty; and affirms that period to be as early with peasants as with the inhabitants of cities.—JOHN ROBERTSON, Esq., Manchester; *Medical Gazette, July* 29, 1842.

ON CORPORA LUTEA. Properly defined, the corpus luteum is a Graafian vesicle; and a Graafian vesicle is simply the receptacle for the nucleus of a cell which is contained within a nucleus or parent cell. Anatomically, the parts constituting the ovary and Graafian vesicle may be defined as one and indivisible; physiologically, the ovarium is the primitive body.

The Graafian vesicles possess two separate tunics, the outer one being vascular, the inner one not so. Dr. Lee first made known the fact that these bodies become more vascular during the menstrual period. It is difficult to state with precision the period at which solid matter begins to form in the Graafian vesicle The cavity may be filled with blood, a serous, straw-coloured fluid, or simply the granular reliquiæ of the ovum. The solid matter of the corpus luteum, examined microscopically, appears to consist neither of secretion or effusion; but to be a true growth, made up of two separate parts. The first, small, nucleated cells, common to all parts of the body; the second consists of cells also, but much enlarged, floating freely, and filled with small granules.

The purpose served by the corpus luteum it is not easy to ascertain; but, perhaps, the best explanation which can at present be given is, that the escape of "a substance" having caused a breach in the ovary, the corpus luteum, by filling up the aperture, brings the sides of the cavity into apposition, and thus heals the lesion. This purpose being accomplished, the office of the corpus luteum is fulfilled, and the absorbents remove it.

The locality of the corpus luteum is very palpable. It is invariably interposed between the external or vascular membrane of the Graafian vesicle, and the internal or non-vascular ovisac, which is always more or less thickened, and has a greater or less quantity of granular or other matter internal to it, in pure specimens. The structure is decidedly lobular, having a radiated appearance; one of the radii always extending as far as that portion of the ovary, whence the ovum originally made its escape.—FRANK RENAUD, Esq., Edinburgh; *Lon. and Edin. Monthly Journ. of Med. Science,* No. vi. *June,* 1842.

INFLUENCE OF THE CORONARY CIRCULATION ON THE ACTION OF THE HEART. By a variety of experiments on the hearts of dogs and rabbits, in which the animals were pithed, the thorax opened as quickly as possible, and ligatures applied to the coronary arteries, Mr. Erichsen found that the action of the heart ceased considerably sooner than when no ligatures were applied. In an animal that is pithed, but the heart of which is uninterfered with, cardiac pulsation will go on for an hour and half, under the influence of artificial respiration, but only, at an average, for about twenty-three minutes and half after ligature of the coronary arteries, and about thirty-two minutes and forty seconds after the death of the animal. He therefore agrees with Dr. Hall in thinking that ossification of the coronary arteries, a fatty condition of the heart, a contracted aortal, or deficient nitral valve may, by interrupting the flow of blood through the coronary vessels, give rise to syncope or sudden dissolution.—JOHN E. ERICHSEN, Esq. *Medical Gazette, July* 8, 1842.

ON THE FORCES BY WHICH THE BLOOD IS CIRCULATED IN THE CAPILLARY VESSELS. The author, in a somewhat elaborate paper, endeavours to show that the capillaries have an important influence in carrying on the circulation of the blood.

He strongly objects to the experiments of Dr. Hall and other physiologists, in relation to the functions of the capillaries, that in most or all of these experiments the mutual relations uniting in harmonious action the several powers carrying on the circulation, are destroyed. When, for example, a ligature is passed round the limb of an animal, or when the aorta is tied, or the heart itself destroyed, in each of these cases the circuit of the circulation is broken; and it necessarily and obviously follows that the parts below the ligature (in the case of ligature) cannot receive blood, on the one hand, nor the parts above the ligature propel it, on the other.

Sensible that all such experiments are fraught with insuperable difficulties, the author set himself to devise some method by which the action of the capillaries might be determined with precision, and the placenta appeared to him to be the most proper organ for the suitable observations and experiments. The whole substance of it may be regarded as composed of arteries, veins, and capillaries; and although the disengagement of it from the uterus disturbs the normal relation

of those vessels, it appeared to the author, notwithstanding, possible to obtain sufficiently accurate results. Accordingly, a placenta was procured twenty minutes after separation from the uterus, and placed, with the exception of the cord, in a bladder, which was immersed in water, at the temperature of 100° Fahrenheit. The free extremity of the cord at the same moment was elevated to an angle of 30°, resting on the edge of a glass, and at the distance of a foot from the placenta. At the commencement of the experiment no blood escaped from the vein, but in two minutes from the immersion of the placenta the blood began to flow, and continued to do so for about twenty minutes; and at this time it was found that the glass had received about an ounce. The author regards this experiment as less exceptionable and more conclusive than those usually referred to, in regard to the circulatory function of the capillaries.—Dr. Calvert Holland, Sheffield; *Edin. Med. and Surg. Journ., July* 1, 1842.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Rhubarb as an external Application in sloughing venereal Ulcers. The author reports one case in which rhubarb, applied in powder, was useful in severe and extensive venereal ulceration of the abdomen and wrist, after some other applications had failed. It caused, however, considerable pain and irritation, and could only be applied on alternate days. This was continued for six weeks, by which time the ulcers had "nearly healed."—Alfred Markwick, Esq., North Brixton; *Medical Gazette, July* 29, 1842.

Pathological Chemistry. From the chemical pathology of a case of scirrhous pylorus Dr. Bird infers: 1. That the gastric secretions are in this disease persistently acid. 2. That they are always more or less coloured, and contain, in suspension, sebacious particles. 3. That free hydrochloric acid exists, in considerable quantity, in the vomited fluids during the more irritative stage of the disease, and gradually disappears as the powers of life sink. 4. That free organic acids are secreted and are met with abundantly; at first co-existing with hydrochloric acid, and subsequently nearly replacing it. These acids are most probably lactic, acetic, and butyric.—Dr. Bird; *Medical Gazette, June* 10, 1842.

Ulcer on the Thorax communicating with the right Lung. The patient was a woman of thirty-one years of age, of scrofulous diathesis. During winter, when confined with a disease of the hip-joint, she had had occasional paroxysms of cough, with slight hæmoptysis; but in March she had regained flesh and strength, when, unfortunately, she caught fresh cold and had a return of cough, with profuse expectoration and perspiration. In May a scrofulous ulcer, which she had previously been troubled with, broke out on the right side of the thorax, and, assuming a phagedenic character, laid bare a portion of the fourth and fifth ribs. On the 25th of May the author's attention was directed by the patient to a curious noise proceeding from the ulcer, and which, on examination, proved to proceed from the expulsion and entrance of air in respiration; showing that a communication existed with the lungs. On the morning of the 24th of the following month she was found lying in her bed with such a profuse hemorrhage from the ulcer, that the person who saw it described it as "pumping out" each time that the patient breathed or attempted to speak. She died in ten minutes after. No examination of the body was allowed.—J. S. Allen, Esq., of the St. Marylebone Infirmary; *Lancet, July* 30, 1842.

Death from Peas. On the 27th of June a labourer, aged sixty, was brought to the hospital, who had been suffering since the 22d from an obstruction of the bowels, caused by a surfeit of gray peas on the 21st. When he came to the hospital he was in a sunken state, and died suddenly as he was being carried up in a chair to the ward. The rectum contained upwards of a pint of peas, which had

been swallowed in a dry state, and almost without mastication, and had undergone no other change in their passage through the intestine than that of becoming swollen by the absorption of moisture They had formed a solid mass in the rectum, and filled almost the entire pelvis, pushing up the bladder (which was greatly distended) and prostate, so as to render the evacuation of urine, by any effort on the patient's part impossible.—GEORGE JOHNSON, Esq., King's College Hospital; *Medical Gazette, July* 15, 1842.

PERFORATION OF THE STOMACH. The patient was forty-nine years of age, had been in indifferent health for some years. His appetite was, however, good, and he never suffered any inconvenience after eating beyond that arising from excessive flatulence. About six months before Dr. Kennion saw him, his symptoms had suffered an aggravation, in consequence of the death of a near relative; since which he had been subject to paroxysms of severe pain, generally coming on at night, and referred to the umbilical and hypogastric regions. Under judicious treatment his health was partially improved, but no radical amendment took place. At four o'clock, on the morning of June 12th, Dr. Kennion was called to see him, and found him labouring under all the usual symptoms of perforated stomach, with effusion into the peritoneal sac. The patient died at half-past nine.

On the anterior border of the lesser curvature were two irregular openings of about three quarters of an inch in diameter; the margins of which were as thin as a piece of paper. These perforations were at the distance of about an inch and a half from the pylorus, and were situated in the midst of a mass of cartilaginous substance, which involved the whole circumference of the stomach, three or four inches from the pylorus. — Dr. KENNION, of Harrowgate; *Medical Gazette, July* 1, 1842.

ON THE TREATMENT OF HEMORRHAGIC DIATHESIS. In almost all cases the blood which escapes from the incised or lacerated surface is preternaturally fluid. A deficiency of fibrin certainly exists, and this deficiency continues to augment in proportion as the bleeding continues. The number of the blood-corpuscles seems also to diminish. The coagulable power of that fluid is seriously impaired, and if a clot at all forms at the orifices of the torn or incised capillaries, it is loose, spongy, and easily detached; while a fresh clot is formed with more and more difficulty, if at all. Increased density of coagulum (which is what we want) is well known to depend on increase of the proportion of fibrin in the coagulating blood, as occurs in the inflammatory diathesis But the proportion of fibrin is not to be here estimated in regard to the general mass of the blood merely, but rather in regard to the blood-globules; it being only when excessive in proportion to these, that the tendency to, and power of coagulation becomes most marked. Mere loss of blood produces a deficiency both of fibrin and globules, but not in an equal ratio At first the latter are chiefly removed, and, consequently, at an early period of the case, loss of blood favours natural hæmoptysis, by increasing the proportion of fibrin to globules, and thereby augmenting the tendency to, and power of coagulation.

But the blood is not alone to blame. The capillaries and arterial tubes are deficient in contractility. In treating the disease, therefore, we must (as in the scrofulous diathesis, to which the hemorrhage bears a considerable resemblance,) prescribe a diet nutritious without being stimulating. Although the rapidity of many cases of hemorrhage preclude the possibility of any material benefit being derived from dietetic means, yet, seeing these are rationally indicated, it behoves us to avail ourselves of them.

The other means recommended by the author are acetate of lead and opium, given in heroic doses, and, if these disagree with the patient, sulphate of alum and potass, in doses of 15 or 20 grs.—nauseating remedies—and the sulphate of soda, which last he considers useful, by procuring serous discharges from the bowels, and thereby disposing the blood to "solid coagulation."

In regard to the relaxed condition of the capillaries, the author is of opinion

that the acetate of lead will help to remove that state, while opium will calm the heart's action and the general circulation.

He is quite opposed to the actual cautery; and, as respects local treatment, would mainly rely on *pressure*, the bleeding part being first lightly touched with nitrate of silver. And, as "last, not least," in his list of remedies, he recommends *transfusion*, on the plain ground of the possibility of replacing, by this means blood "lamentably deficient in both globules and fibrin," with blood sufficiently abounding in both.—James Miller, Esq., Edinburgh; *Lon. and Ed. Journ. of Med. Science, July*, 1842.

Interesting Case of Syphilis. Mrs. B., a married woman and a mother, had an unhealthy-looking ulcer on the nipple, which, at one time was indolent, at another extended itself, and threatened to "excise" the nipple. An eruption subsequently appeared on the other breast and on the scapula, along with a large ulcer on each tonsil. The eruption consisted of many roundish spots, of the size of sixpences, of a dusky copper colour, constituting, as plainly as possible, that form of lepra described by Bateman as arising from venereal poison.

Some weeks before, a young woman had given birth to an illegitimate child, in the house of a mutual friend, Mrs. A. The woman either would not or could not suckle her infant, and Mrs. B. having at the time a child of her own at the breast, volunteered to give milk to the woman's infant, if brought to her house at particular hours. The child, a miserable creature, soon died; and it was after this that Mrs. B., as above described, presented signs of venereal infection. But Mrs. A., who, though married, had never borne children, and could, consequently, never have suckled the servant-woman's child, presented an ulcer of the nipple also, along with an eruption, differing, however, from that of Mrs. B., (which was decidedly leprous,) in being papular and covering the whole body.

On inquiry, it appeared that the infant of the woman had been covered with sores about the genitals and at every point. Yet the mother seems to have been healthy. It appeared also, that the child, when sleeping with Mrs. A., had been used to apply its lips to her nipple.

Subsequently to the affection of Mrs. B., her own infant had a leprous eruption on the posterior parts, exactly similar to that of the mother. The author concludes: 1. That a diseased infanf may be borne by a mother *apparently* healthy; which must be admitted to be extremely possible. 2. That such an infant may, by sucking, communicate the disease to sound persons. 3. That the disease so communicated, may be followed by secondary symptoms. 4. That the disease may be different in such persons. That the disease imbibed may be communicated by suckling.—George Lowdell, Esq., Lewes; *Medical Gazette, June* 17, 1842.

On Albuminuria. The author's opinion is, that the presence of albumen in the urine is produced by, and its proportional quantity is in a direct ratio to the degree of congestion of the capillaries of the kidney, from whatever cause that congestion may arise. Some experiments on rabbits are related, in which the renal vein was ligatured; and it would appear from these, that the secretion of urine of a highly albuminous character was the result of the operation referred to.—George Robinson, Esq., Newcastle-on-Tyne; *Medical Gazette, June* 3, 1842.

[In the third succeeding number (760) of the same journal Dr. Burridge, in a paper on "Granular Disease of the Kidney," remarks that the hyperemia produced by Mr. Robinson's ligature of the renal vein "does not constitute inflammatory action, being merely *one* of its elements;" and that "the absence of albumen from the urine cannot be admitted as a sound test of the absence of congestion."]

Hemorrhagic Diathesis. On the 18th of August, 1820, Dr. Allan was called to visit a boy, five years old, with a slight abrasion of the wrist, caused, a few hours before, by a bit of window-glass. A brother of the boy had died by he-

morrhage from a wound equally slight. Both children were remarkably beautiful, with an unusual blush on the skin, auburn hair, and bright hazel eyes. Dr. Allan, along with Mr. Harvie, who had treated the first case, tried every means usual in such cases—such as alum, actual cautery, acetic acid, ol. terebinthinæ, bandaging, &c.—without the slightest good effect. On the evening of the 21st the patient appeared to have lost ground. On the 22d the action of the heart was indistinct and irregular; the urine, after becoming less in quantity and perfectly limpid, had ceased to be discharged during the last ten hours. On the 23d the breathing was slow, with frequent sighing; the action of the heart intermitting, the pulse imperceptible; but neither now nor heretofore any complaint of suffering on the patient's part. Slight oozing from the wrist on the 24th, and death in the morning of the 25th. Dr. Allan examined the wrist with a glass, and could only observe "a few small points, on which particles of colourless fluid rested." The author observes, "death appeared to take place more from decomposition of the blood—the serous portion seeming to drain off—than from loss of quantity; for the drops were slow, and many seconds between each."—Dr. ALLAN, of Haslar Hospital; *Lon. and Edin. Journ. of Med. Science, June*, 1842.

ABSCESS IN THE LUNGS, POINTING BELOW THE UMBILICUS. About ten months before his death, the patient, an intemperate working jeweller of thirty-four years of age, noticed a distinct, hard swelling under the short ribs on the left side. When Mr. Barker saw him, in October 1839, and about four months after its commencement, the tumour had a circumference about as great as that of "two hands opened and joined together," and extended on both sides of the mesial line, but principally to the left. There was distinct fluctuation in every part of the swelling. The patient complained of pain in the back and loins, particularly during night, with a heavy dragging pain between the shoulders. But there was no chillness or shivering; no pain in the chest; and no evening exacerbations. Some weeks after this, the tumour pointed and discharged itself. Hectic symptoms set in, and the patient died in the beginning of March.

On dividing the parietes of the abdomen, the muscles "were observed to be split up, as it were by a quantity of matter," not proceeding from the abdomen, but from the left lung which presented a cavity capable of containing ℥iv of pus. The matter had made its way through the external and internal oblique muscles of the abdomen, after passing through a small opening in the ninth intercostal space of the left side —T. HERBERT BARKER, Esq., of Bedford; *Med. Gazette, July* 1, 1842.

FÆCAL VOMITING DURING THIRTY-FOUR DAYS. This is a rather uncommon case. The patient, a woman thirty-four years of age, was admitted into the hospital on the 3d of February, on which day she seems to have vomited fæcal matter, and continued to do so, with slight intermissions. Her bowels appear to have been constipated, until the 20th, and then to have been opened by enemeta. On the 12th her abdomen became covered with large, round vesicles, or rather pustules. She had always complained, from the time of admission, of pain in the epigastrium and abdomen, and had, in consequence, had leeches, bloodletting, and mercurials administered. On March 1st she appeared to be better, but on the 2d the vomiting returned, and on the 9th the pustular eruption of the abdomen reappeared, and a large patch of the integuments below, and to the right of the umbilicus, became discoloured. On this day she was seized with intense pain and shivering, the abdomen became tympanitic, and she died on the 10th.

A rupture was found in the lower part of the jejunum, through which a small quantity of thin fæcal matter had escaped into the peritoneal cavity. The intestine, for about sixteen inches, was gangrenous. Immediately below the rupture, and occupying the whole caliber of the bowel, was a solid, fleshy substance, about three inches in length, growing from the mucous membrane by a very narrow pedicle. This growth, by dragging down a portion of the intestine, had produced intus-susception.—J. S. ALLAN, Esq., of the Marylebone Infirmary; *Lancet, June* 4, 1842.

Post-mortem Appearances of a Case of Grinders' Asthma. The grinders are extremely averse to their bodies being opened, and take great care not to be overtaken with death in hospital. It is consequently rare that an opportunity occurs of examining the necroscopic appearances of consumption caused by inhaling dust and metallic particles.

In the present case—that of a man forty-nine years of age—the pleura adhered extensively on the left side, and was of a dark colour; both sides were studded with tubercles, of the usual light hue, most numerously along the anterior margins; there were two large cavities near the apex of each lung; the lining membrane of the bronchial tubes was natural, nor were there any tubercles around any part of these tubes. The principal peculiarity consisted in the great enlargement of the bronchial glands, one or two of which were of the size of walnuts, and contained a considerable quantity of a stone-like material, as hard as a piece of common lime-stone, but quite black like the glands themselves, which were as hard as fibro-cartilage. This induration was not confined to the conglobate glands, situated about the bifurcation of the trachea, but extended to the smaller ones under the pleura, some of which were of the size of a horse-bean. From microscopical and chemical examination, these concretions consisted simply of "animal tissue," and carbonate of line.—E. D. L. Gillat, Esq. Sheffield; *Lancet, June* 18, 1842.

SURGERY.

Malignant Diseases of the Head and Face. The question naturally arises what are the symptoms of osteo-sarcoma of a malignant nature, and how are we to ascertain the precise time when the change from benign to malignant takes place. The following inferences are deducible from what is known of osteo-sarcoma of the lower jaw-bone. 1st. The disease almost always commences in the cancellated structure of the bone, and has generally a cystic origin, which it maintains from first to last. 2d. It is always (as far as recorded cases permit us to judge) mild under the ages of twenty-eight or thirty years; and although it does not necessarily become malignant after these periods, yet it generally does, involving the soft parts, and assuming the characters of carcinoma. 3d. Osteo-sarcoma of the lower jaw is almost always curable by free excision, before the soft parts become involved in the disease, which they never do in the benign form, nor for several months after the disease has assumed the carcinomatous form. 4th. Cancer of the face, especially the bones, admits of cure more frequently than when seated in any other part of the human frame; cancer scroti, chimney sweeper's cancer, perhaps excepted. 5. Ligature of the carotid is unnecessary previous to or during the disarticulation of the jawbone.—Dr. Byron; *Dublin Journal of Med. Science, July*, 1842.

Displacement of the Stomach and Colon in consequence of a Wound. A serjeant of the 88th regiment, when skirmishing on the day previous to the battle of Fuentes d'Onor, was fired at as he was attempting to ascend a steep hill, the man who fired at him being at the top. The ball entered close to the nipple of the left breast, and passed out at the back, between the eighth and ninth ribs. The man recovered, rejoined the service, and died of gangrene of the lower limb in 1833.

On examining his body, the anterior surface of the stomach was found firmly attached to the lower lobe of the left lung. It would appear that the whole of the stomach and greater part of the transverse colon, with the omentum, had passed into the left cavity of the thorax. The heart was displaced so as to lie nearly parallel to the spine, its apex almost on a line with the coronary artery of the liver. The wound in the diaphragm, through which the hernia of the stomach and other parts had taken place into the thorax, appeared to have extended originally about three inches.—G. Williamson, Esq., *Medical Gazette, July* 15, 1842.

CASE OF OSSEOUS ANEURISM. The patient, aged nineteen, had injured the right knee, near the inner condyle, by slipping over a stone bank, five months before Mr. Hargreaves saw him. Five weeks after the accident, the patient observed a slight swelling on the inner side of the knee-joint, occasionally communicating to the touch, "a degree of beating." The tumour had now attained an enormous magnitude; measuring round the knee-joint, twenty-four inches. It was accompanied with diffused hardness, and extended as high as the inferior third of the thigh, filling up the whole popliteal space. A very indistinct pulsation could *sometimes* be heard at the internal condyle, but neither pulsation nor tremor was communicated to the touch.

The femoral artery was tied on the 11th of April last. The case appeared to go on well till the 28th, when there was epistaxis of an apparently obstinate character; and an ichorous discharge commenced from the ulcerated integuments, [around the wound we presume,] and a sensation of fluctuation was felt. The limb also gradually enlarged. A consultation was held as to the propriety of removing the limb; but this step was deemed inadmissible, as petechiæ appeared on the affected member, and spread rapidly over the body, while hemorrhage took place incessantly from the mouth and fauces. He died on the 1st of May.

On making an incision, and exposing the popliteal space, a sac was exposed, extending as high as the middle third of the thigh; this sac was full of sanguineous fluid, and contained a semi-cartilaginous substance; interspersed with a little ossific matter. It was difficult to say, whether the sac itself was composed of the periosteum, or had a distinct origin, but the tendons, muscles, nerves, and blood-vessels, did not present any abnormal appearance. The popliteal artery, vein and nerve, were situated very superficially, and upon the sac, which had not communicated with the artery; this vessel being perfectly sound.—Mr. HARGREAVES, Lancashire; *Medical Gazette*, No. 757, *June* 3, 1842.

MR. STAFFORD'S TREATMENT OF STRICTURE OF THE URETHRA. The single lancetted stilette, or urethral perforator, is passed down to the stricture, the exact distance of which from the extremity of the urethra is first ascertained. When the point of the instrument is arrived at and rests upon the contraction, (which is known by means of its graduation,) and is in an exact line with the natural course of the canal, the instrument is held and maintained in that position by the left hand, the forefinger of which being passed through the ring on the under part of its handle, the thumb of the right hand is passed through the ring on the handle of the stilette. The stilette is then pressed gently and gradually forward, *in the direction of the canal*, when, on reaching and resting on the stricture, the lancet is protruded at its point, and is thus made to incise the stricture, immediately after which the bougie or catheter may be passed with facility.—WILLIAM COULSON, Esq., Surgeon to the Magdalen Hospital; *Med. Gazette*, No. 763. *July* 15, 1842.

MORPHIA IN STRANGULATED HERNIA. Mr. Lyell records a successful case. Within three hours, the patient had three grains of opium, and four and a half of hydrochlorate of morphia, and the rule which Mr. Lyell would recommend is, *to employ the morphia in half grain hourly, or half-hourly doses, till the patient is fairly narcotised.*—JOHN LYELL, Esq., Fife; *Lond. and Edin. Journ. of Med. Science*, No. vii. *July*, 1842.

MOXA IN RHEUMATISM. The writer reports excellent effects from the use of the moxa among the labouring poor of Ireland, who appear to be very subject to local rheumatism, in consequence of exposure and insufficient clothing. Mr. Leney soaks a piece of lint in a strong solution of nitrate of potass, then dries it, and cuts off pieces the size of the thumb-nail, which he fastens with thin adhesive plaster over the seat of pain, sets fire to the opposite extremity, and then applies the blowpipe. The pain during the operation is very severe, but the Irish prefer it much to the application of blisters!—JOHN LENEY, Esq. Bray; *Med. Gazette*, No. 763, *July* 15, 1842.

Tincture of Catechu in Sore Nipple. Mr. Farr highly recommends the tincture of catechu in sore nipple. It has the effect of "thickening and toughening the nipple and surrounding integument." Messrs. Hopgood of Bampton, Devon, and Pye, H. Chavasse of Birmingham, confirm Mr. Farr's account of the uncommon efficacy of the remedy.—William Farr, Esq. of the General Register Office; *Lancet*, No. xv. *July* 9, 1842. [The nipple is first to be washed and dried, and then the tincture applied with a hair pencil. In No. xvii. of the same journal (July 23) Mr. Wansbrough of Chelsea recommends "the leaden shield," which is "moulded by the plumber from thin sheet lead, over a wooden nipple, and forms a receptacle for the natural one, which, from the oozing of the breast, is constantly immersed in a solution of the lactate of lead, and speedily effects a cure."]

MIDWIFERY, AND DISEASES OF WOMEN AND CHILDREN.

Extraordinary Case of Twins. On the 3d of April last, Dr. Jamieson was called to visit a lady thirty years of age, in consequence of severe pain in the abdomen, recurring at uncertain intervals, and lasting generally about five minutes at a time.

The author discovered a firm hard tumour, reaching as high as the umbilicus, which softened on the subsidence of pain, and appeared to be the gravid uterus. On applying the stethoscope, Dr. Jamieson thought he heard a placental murmur in the right iliac fossa; but the lady said it was impossible she could be with child, as she had been confined so recently (seven weeks before), and was at present nursing. As, however, Dr. Jamieson was convinced that the tumour was the uterus, and that it was acting tc get rid of something, he ordered a dose of oil and retired to another room, in order to explain to the husband that he believed there was some foreign body in the uterus of his wife.

The author was hurriedly summoned, while engaged in this explanation, to the apartment of the lady: and on examining per vaginam, found the head of a small child presenting, with the membranes entire, which, on the occurrence of another pain, was expelled together with the placenta. The child was dead, and seemed to be about the sixth month of gestation; and though shrivelled and dark, was not at all putrid or decomposed. It was between eight and nine inches long. The mother was of course greatly surprised. She had been confined of the other twin on the 13th of February. Consequently the dead fœtus must have remained in the womb for forty-nine weeks.—Dr. Jamieson, Dublin; *Dublin Journal of Med. Science,* No. lxiv. *Sept*. 1841.

Case of Short Funis. The funis in this case was only seven inches and a half. In Dr. Churchill's tables, the average length is eighteen inches; the next most frequent length, twenty-four inches. The shortest cord in 500 cases of the latter gentleman was twelve inches, the longest fifty-four. Mr. Stone, however, has met with a shorter funis than that of the present case, namely one of only six inches.

Dr. Thomson is of opinion that a quick delivery of the fœtus is always or generally followed by a prompt expulsion of the placenta.—Dr. J. B. Thomson, London; *Lancet,* No. x. *June* 4, 1842.

Case of Arm Presentation, in which Turning was impracticable. The membranes had given way at 9 o'clock of the preceding night, and Dr. Lynn found the right hand and arm protruding from the vagina, the shoulders and part of the thorax and neck of the child being firmly impacted in the upper part of the pelvis. With every justifiable effort which Dr. Lynn was capable of making, he could not succeed in seizing the feet, but got his forefinger into the ham of the right leg, as it lay bent upon the thigh towards the sacrum of the mother. The author was utterly unable to bring down the leg. He, therefore, now eviscerated the thorax, and with great difficulty succeeded in seizing the right foot, in bringing

it down, and in applying a fillet around it. But although he now applied all justifiable traction, he was unable to move the child out of its impacted position. Accordingly, he let out the contents of the abdominal cavity, and with the crotchet brought down the pelvis of the fœtus. The operative part of the proceeding occupied about an hour; not half a pint of blood was lost before or after delivery, This is the third successive unnatural presentation which has occurred to this woman. who, notwithstanding, has made an excellent recovery.—Dr. LYNN; *Dublin Medical Press*, No. 190, *August* 24, 1842.

CASE OF UTERINE POLYPUS, AND OF SHORT FUNIS. A woman was delivered on the 4th of December, and on the 6th she was again taken, with what she supposed to be labour-pains. Mr. Collyns on examining, found a large substance partly in the vagina and partly in the uterus, which was soon expelled by the efforts of the womb. It proved to be an organized tumour, of an oblong form, which had been attached to the womb by a neck about two inches long, and the thickness of one's finger. The tumour, which was perfectly solid, looking like flesh, with small arteries and veins, running through it, measured sixteen inches in length, eleven in circumference, and weighed two pounds and a quarter. The woman got perfectly well.

The same practitioner mentions another case in which the funis was only, or not quite, six inches long. This case was marked by alarming hemorrhages during pregnancy, but not during parturition.—WM. COLLYNS, Esq.; *Prov. Med. Journal*, No. xviii. *August* 6, 1842.

CASE OF TRIPLETS. On examination, the breech was found presenting, with a hand hanging quite out of the passage, showing that it was a case of twins. The feet were easily reached, and the child, a female, was delivered. During the whole of this time, the arm remained hanging out, but receded as soon as the first child was born; and the right shoulder presenting, the mother was, after sometime, delivered of a dead male child, connected with the cord of which was a double placenta. Fresh pains came on, and a third child, a male, came away. The third child was in no way connected with the first two.

The woman had attained her full time, and three children were all perfectly formed; weighed six pounds and a half each, and were sixteen inches long.—Dr. DAVID, Edinburgh; *London and Edin. Jour. of Med. Science*, No. vii. *July*, 1842.

LARGE CONGENITAL TUMOUR. A male child was born on the 22d of July last, with a tumour extending from the middle of the *os frontis* to within about an inch of the ear, and from the upper eyelash to the coronal suture. Its circumference round the base was nine inches; its substance was firm and doughy; its shape conical, on the apex it contained fluid. Round its base, the bone was raised into a circular ridge, but an opening into the brain could not be felt. The child is in good health and thriving. *The mother states that, in an early period of her pregnancy, she had been disgusted by the sight of the entrails of a pig.*—ALEX. BREDON, Esq.; *Dublin Medical Press*, No. 188. *August* 10, 1842. [In our foreign selections of this number, a remarkable case is given of the impressions which may be made on the fœtus through the mother.]

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

CASE OF SUICIDE. A woman twenty-seven years of age, was committed for theft to the jail of Edinburgh. She was much addicted to drinking, and was apparently ill of delirium tremens "after" admission. That night she complained that somebody was chasing her. She was calm in the morning, and she got a jug, composed of glazed earthenware, containing milk, along with a spoon of iron, for breakfast. The matron saw her at eleven, before going to church, gave her a bible, and desired her to learn the first psalm. She was found, at one o'clock,

by the matron, on her return from church, lying dead on the floor, her throat horribly cut, and the fragments of the broken jug lying beside her, and covered with blood. The jug was broken into pieces, some of which were of considerable size, and had very sharp edges and angles.

This case forms one of a class not unfrequent in prisons in which suicide is attempted very shortly after confinement, especially by persons convicted for the first time, accustomed to indulge freely in alcoholic liquors, and strongly predisposed to, if not actually under delirium tremens. It is probable that this woman was really under the influence of the disease now named on the very day of her admission. It is remarkable that she had selected, as the instrument of suicide, the broken fragments of her milk jug, in preference either to the glass of the window, or the iron spoon, which if sharpened on the stone floor of the cell, might have been easily made available to the purpose of self-destruction. Indeed, the latter instrument had been partially used for that end; since the wound after having been originally made with the fragments of the jug, seemed to have been deepened and enlarged by the handle of the spoon having been employed in "boring and pressing aside the parts."—Dr. SPITTAL, Physician to the Royal Infirmary of Edinb.; *London and Edinb. Month. Journ. of Med. Science*, No. vi. *June*, 1842.

POISONING BY SULPHURIC ACID. The author was called to a young woman supposed to have swallowed sulphuric acid. He found her lying on the floor, with a black froth issuing from her mouth, her extremities cold, pulse almost imperceptible, breathing labouring and irregular. A quantity of whitening mixed with milk, was copiously administered, to the relief of the patient, who now vomited a quantity of streaky mucus. This was on a Friday.

On the following day, enemata, bloodletting, anodyne and mucilaginous drinks were administered, and great pain occurring over the epigastrium, with symptoms of severe gastritis, eighteen leeches were applied to the abdomen, followed by epithems of hot turpenture. She died easily on the afternoon of this day.

The lips were found excoriated and much blackened; the œsophagus congested and also blackened; the cardiac and pyloric ends of the stomach intensely inflamed, blackened and excoriated; the duodenum slightly affected; the other portions of the intestinal tube distended with flatus; the kidneys much inflamed and *exerting an acid reaction on litmus paper;* the right ovary contained a clot of blood; the uterus contained some purulent or mucous (?) fluid adhering to its mucous membrane.

The author obtained distinct traces of the presence of sulphuric acid, both in the dress and in the kidneys of the patient, by the application of Devergie's test.—Dr. SCOFFERN; *Lancet*, No. 11. *June* 11, 1842.

COPORA LUTEA. To ascertain the precise structure of one of these bodies, is of importance in a medico-legal point of view. The ovary, like other parts, being liable to disease, its vessels may become varicose, and apoplexy, as it is termed, forms no uncommon appearance in it, consisting of an irregular effusion of blood into its tissues, "varying indefinitely as regards size." Tubercles may also occur in it. Both these morbid conditions may be mistaken for corpora lutea. The following appearances ought always to be present in true corpora lutea; 1. A distinct external envelope, in contact and in union with the stroma of the ovary, but capable of being dissected away from it entire. 2. A solid substance, fleshy-looking, red, pinkish or yellow, divided into a greater or less number of lobuli. 3. An inner membrane or ovisac thickened. 4. A central deposit of granular or other matter, or the remains of it. 5. The general microscopic appearance presented will form a good auxiliary means of diagnosis The fact of one of the radii reaching as far as the surface of the ovary, is useful as a diagnostic indication, but is not decisive, being occasionally present in the false as well as in the true bodies.—FRANK RENAUD, Esq., Edinburgh; *London and Edinb. Month. Jour. of Med. Science.* No. vi. *June*, 1842,

PART FOURTH.

Medical Intelligence.

OBSERVATIONS ON SOME POINTS IN THE

ANATOMY, PHYSIOLOGY, AND PATHOLOGY OF THE BLOOD.

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The blood, as it circulates in the vessels of the living body, consists of a transparent colourless liquid, called *liquor sanguinis* or *plasma*, containing in suspension microscopical corpuscles of two kinds: the one red and in very great number, the other colourless and very few in number.

Liquor Sanguinis. The liquor sanguinis is scarcely an object of microscopical examination, except in connexion with the corpuscles suspended in it. The liquor sanguinis of frog's blood may be separated from the red corpuscles by filtration, but that of human blood cannot be obtained free from red corpuscles, except when the blood is drawn during inflammation, and certain other states of the system. In this case the liquor sanguinis, mixed, however, with colourless corpuscles, rises to the top in considerable quantity, and may, before coagulation takes place, be removed for examination. I shall return to this point, more particularly afterwards, when considering the formation of the buffy coat.

Red Corpuscles of the Blood. *Form.* The red corpuscles of the blood, though they have been called globules, are not in their ordinary state globular, but have the form, as is now generally known and admitted, of biconcave lenses, with the peripheral edge obtusely rounded; the biconcave lenticular form is proclaimed by the appearance which the corpuscle presents under the microscope, viz. the circumference as a broad bright ring and the centre as a dark spot; or, on the contrary, the circumference dark and the centre bright, according as one or other is in the focus of the microscope.

Size. In healthy blood, newly drawn, and unaltered by any reagent, the corpuscles are between $\frac{1}{3000}$ and $\frac{1}{3,500}$ of an English inch broad, and about one fourth of that thick at the circumference, but thinner, of course, at the centre.

Structure. The red corpuscle is a vesicle, or cell, with thick walls, but in a collapsed flattened state. Its form is exactly such as a thick-walled vesicle, or cell, in a collapsed flattened state would present. The bright or dark ring represented by the circumference, and the dark or bright spot by the centre, according as one or other is in the focus of the microscope, are appearances which tally with those known to be presented by a collapsed thick-walled cell. Certain reagents by giving rise to exosmose, render the corpuscle flatter, and consequently bring more distinctly into view its lumen, and the double contour of its thick wall.

Misinterpreting the appearance presented by the double contour of the thick wall of the corpuscle, Dr. Martin Barry has described it as being produced by an annular fibre contained in the interior of the corpuscle. In his papers on the blood, he described the corpuscles as containing in their interior certain minute bodies, which he calls "*discs*," in shape flat, elliptical or circular, usually concave in the middle of the flat surface. He now finds that these "*discs*," by uniting, form "fibre." In mammalia, including man, says he, the "fibre," or filament, is simply annular, (hence, he says, the biconcave form of the corpuscle in this class,) but in the other vertebrata the "discs" contained within the corpuscle are too numerous for such a *ring*, therefore their arrangement forms a *coil*.

In regard to these statements it is to be observed: 1st, That the corpuscles

which Dr. M. Barry delineates and describes as blood-corpuscles containing "*discs*" are not real red corpuscles, but the colourless ones known by the name of lymph-corpuscles; 2dly, That in the real red corpuscles of human blood there are certainly no "*discs*" to be seen; and 3dly, That the appearance which the real red corpuscle may be made to present by reagents, and which Dr. M. Barry interprets as an annular fibre in its interior, is simply the bright annular appearance above spoken of as being produced by the folding of the thick wall of the corpuscles; the double contour being rendered well defined by the great flattening which takes place. The figures of the red corpuscles of man given by Dr. Barry to show the alleged contained fibre are not true to nature, but appear to be exaggerations of that state of the corpuscle produced by reagents in which, at the same time that it is much flattened, its edge is beaded or as if bent here and there in opposite directions like some kinds of biscuits. The form, often assumed by the blood discs of the newt, of flask-like vesicles, with the appearance of a minute body protruding from their neck, and which Dr. Barry describes as the extremity of the filament in question, I have observed presented even by the human blood-corpuscle, but it was easy to see that the alleged fibre was nothing more than the substance of the corpuscle, changed in consistence by the reagent, drawn out, as a viscid matter is, into a thread.

The fibres which the fibrin may be observed to form in solidifying, are described by Dr. Martin Barry as fibres escaped from the interior of the corpuscles. He even affirms that he has noticed "the ring formed in the blood-corpuscle of man, and the coil formed in that of birds and reptiles, unwinding themselves into the straight and often parallel filaments of the coagulum; "changes which," he says, "may be also seen taking place in blood placed under the microscope before its coagulation."

The thick wall of the red corpuscle consists of two layers. The outer is transparent, colourless, structureless, and resisting, and constitutes about one half of the whole thickness of the wall. The inner layer is softer, and less resisting; and is that which is the seat of the colouring matter. The outer layer may be compared to the vitellary membrane of an ovum, the inner layer to the representative of the yelk in the mammiferous ovum.

By the addition of water, &c., to the blood, endosmosis takes place through the walls of the red corpuscles, which thus acquire the spherical form of a distended vesicle or cell, becoming at the same time so transparent as to be with difficulty perceived. Red corpuscles thus changed may be brought back more or less nearly to their original form by the reagents which give rise to exosmosis. Solution of iodine, besides doing this, tinges the wall of the corpuscle yellow, and thus renders it very distinct.

Water, acetic acid, &c., readily extract the colouring matter with which the inner layer of the corpuscle is impregnated. The distention of the corpuscle, which at the same time takes place, if considerable, causes the bleached inner layer to be broken up, and separated into minute colourless granules, or into streaks, while the outer layer remains, though collapsed. In examining the corpuscles of frog's blood under the microscope, I have sometimes observed that when dilute acetic acid was added, the inner layer suddenly gave way with a jerk.

The red corpuscle of the blood of the frog presents out of the body a very distinct nucleus, but not, it is said, when the blood is observed circulating in the web of the living animal's foot; hence, it has been suggested that the nucleus may be formed only after abstraction of the blood from the body. The cause, however, of the nucleus not being always distinctly seen in the blood while circulating, appears to be that the corpuscles are then somewhat more distended than they are when out of the body.

Does the *red* corpuscle of human blood possess a nucleus? This is a question which has been variously answered. Some have spoken familiarly of a nucleus, some doubtingly, and some have altogether denied the existence of one. In the unaltered red corpuscles there is no appearance of nuclei;* but when to the blood some reagent has been added, for instance, acetic acid, minute shining par-

* Of course the central depression is not to be confounded with a nucleus.

ticles, about one fourth or one fifth of the diameter of the corpuscles, come into view, but not in connexion with the corpuscles. These minute particles float about quite free, and exhibit molecular movements. They are nothing but particles of fibrin, or albumen, precipitated by the reagent, as may be proved by adding to liquor sanguinis, or even serum, in which there is not a single red corpuscle or particle to be seen, some reagent, as acetic acid, when the minute particles under notice will be produced in great quantity. When liquor sanguinis is used, some larger particles are also produced.

This question of a nucleus has no reference whatever to the colourless corpuscles which exist in small number in the blood dispersed among the red corpuscles, for they are well known to present one or more nuclei in their interior after being acted on by acetic acid. In his second paper on the corpuscles of the blood, Dr. Martin Barry speaks of the nucleus of the red corpuscles being composed of several parts instead of being one object, as is, according to him, usually considered to be the case. What he delineates, however, as blood-corpuscles of man, after the addition of acetic acid, most certainly are not the red corpuscles which constitute the mass of the blood, but the perfectly distinct colourless ones just referred to, and to be noticed more particularly below.

The red corpuscles, it is well known, are extremely prone to become granulated on the surface, especially at the circumference, which thus appears beaded and notched. Not unfrequently one bead is observed about the centre, which might be put down as a nucleus, but it is not so. The granulated appearance seems to be owing to a contraction of the inner, and a wrinkling of the outer, of the two layers of the wall of the corpuscle. The circumstance that the corpuscles so changed are less in diameter than natural, besides other appearances, support this view. The granulated or mulberry appearance may be at once produced by pressure, e. g. by pressing down closely the superjacent thin plate of glass on the minute quantity of blood under examination. It may also be readily produced by certain reagents, as, for instance, a solution of common salt, oil, &c. In our examinations of the blood some granulated corpuscles are generally seen towards one or other side of the field ; this I believe, is owing to the evaporation of the fluid part of the blood at the edges of the superposed plate of glass, allowing the latter to be more closely pressed down on the blood.

It appears to be the change to that state giving rise to the mulberry appearance just noticed that Dr. Martin Barry describes as "progressive division of the blood-discs into globules." He considers it to be a vital process, and that the globules thus alleged to be produced are the foundations of new corpuscles of the blood. It has been already shown that Dr. Martin Barry mistakes the colourless for the red corpuscles of blood; we now find that he mistakes decomposed and distorted red corpuscles of blood for living blood-corpuscles, undergoing progressive division. Under this erroneous impression he describes the granulated or mulberry state of the red blood-corpuscles, as an advanced degree of that which the colourless or lymph-corpuscles of the blood present, after being acted on by acetic acid.

The red corpuscles are yielding and elastic, so that they readily change shape when slightly pressed upon, like partially filled bladders, which, indeed, they are, and as readily regain their original form when they have escaped from the compressing agent. In consequence of this property the corpuscles glide along in their vessels, with great ease accommodating themselves to all obstacles and to each other. In a mixture of blood and pus the red corpuscles are observed to yield in the most extraordinary manner, so as to accommodate themselves to obstacles. Thus, in order to pass through a narrow channel, they will be drawn into a mere filament, and yet, when free, immediately regain their original form. Their capability of being moulded into various shapes depends on the state of distention of the red corpuscles, and this again on the nature of the liquid in which they are suspended.

Dr. Barry describes and delineates corpuscles found in fluid having nearly the colour of blood taken from an abscess. The fluid examined by Dr. B. was evidently no other than a mixture of pus and blood, and he has mistaken the changes which the red corpuscles of blood undergo in consequence of the reagency of pus, for transformation of the blood-corpuscle into a pus-globule.

Mr. Gulliver describes very well these changes of the red corpuscles which Dr. Barry has so completely misinterpreted.*

The rapid and incessant changes in form of the altered blood-corpuscles which Dr. Barry speaks of, can have been owing merely to his having ill observed the turning over and over from side to edge, and from edge to side, of the irregularly-shaped corpuscles. In reference to the same point, Dr. Nasse says—"I do not exactly know what Barry means, but probably it is merely an appearance produced by imbibition."

Colourless Corpuscles of the Blood. The colourless corpuscles are slightly larger than the red corpuscles, appear finely granulated on the surface, and strongly refract light. They are specifically lighter than the red corpuscles; hence, when a minute drop of blood is mixed with a similar drop of water, acetic acid, &c., they float above, and are, therefore, seen in a different focus from the red corpuscles; hence, also, they are found suspended in the liquor sanguinis, which rises to the top, when a buffy coat is to be formed. Though comparatively few in number, the colourless corpuscles in the blood are sufficiently numerous for two or three to be observed at once in a minute drop of pure blood thinly spread out. In a drop of blood mixed with water they are perhaps more readily detected at once, and in greater number from their being in a different focus from the red corpuscles, and from their not being, like the latter, rendered indistinct by the action of the water.

Structure. By the action of acetic acid several nucleus-like bodies connected together become visible, and the delicate membrane composing the external wall of the corpuscle is very much distended, so that the diameter of the corpuscle is now about one third or more greater than that of the red corpuscle.

Coagulation of the Blood. In the course of a few minutes after its escape from the body the blood coagulates into a soft red mass, like jelly. By the contraction of this mass, which slowly ensues, a yellowish liquor is gradually squeezed out. The solid and liquid matters into which the blood is thus resolved are known by the names of *crassamentum* and *serum.*

Intimate part of the process of coagulation. The crassamentum and serum into which the blood is resolved by coagulation, it is to be noted, are not the same, respectively, as the red corpuscles and liquor sanguinis, above mentioned, as the components of the blood, in respect of form, while it is still circulating in the body, and for a short time after it has been drawn.

The annexed table† illustrates the differences between the components, in respect of form, of living, and of coagulated blood.

Living Blood.	*Chemical Components.*	Coagulated Blood.
Liquor Sanguinis.	Water	*Serum.*
	Various salts	
	Fatty matters	
	Extractive do.	
	Albumen	
	Fibrin.	*Crassamentum.*
Red Corpuscles.		

Coagulation of the blood is due to solidification of the fibrin, which was previously in a fluid state in the liquor sanguinis. The red corpuscles have no posi-

* "The corpuscles are sometimes rather humid on the surface, lenticular, and occasionally cup-shaped. They are often swollen at the edges, which, in consequence, project towards the centre, thus producing there triangular, oval, or irregular depressions. The cup-shaped variety is rather frequent in corpuscles which have been mixed a little while with saline solutions, and it is not uncommon in man, particularly among the particles of purulent or other morbid fluids."—(Appendix to translation of Gerber's Anatomy.)

† Similar Tables have already been given by Mandl and Bruns.

tive share in the process, and though contained in the crassamentum, they form no necessary part of it. The crassamentum is formed essentially of the solidified fibrin.

The simplest demonstration of the proposition just stated is the well-known process of stirring newly-drawn blood, when the fibrin, as it solidifies, is precipitated on the rod in the form of a soft elastic fibrous substance, whilst the red corpuscles remain perfect and entire, mixed with the serum. A more satisfactory, though not so simple a way consists in separating, by artificial means, the liquor sanguinis, still containing the fibrin in solution, from the red corpuscles. The liquor sanguinis, thus separated, may then be observed to coagulate, and afterwards to resolve itself into a colourless crassamentum and serum.

Hewson's method of separating the liquor sanguinis from the red corpuscles consisted in preventing blood from coagulating by dissolving in it, as it flows from the vein, sulphate of soda. "The red particles," Hewson* remarks, "readily subside, and the surface of the mixture becomes clear and colourless; and being poured off from the red part, it is found to contain the coagulable lymph, which can be coagulated, and thus separated, by the addition of water." Various other neutral salts, besides the sulphate of soda, possess the property of keeping the blood fluid, and yet allow it afterwards to jelly on being mixed with water. Some salts again, though they keep the blood fluid, do not allow it to jelly when mixed with water. The most elegant and convincing method is that pointed out by Professor Müller. It consists in receiving frog's blood on a filter, which, in consequence of the size of the red corpuscles, may be porous enough to allow the liquor sanguinis to pass quickly through. The pure liquor sanguinis thus obtained soon coagulates, and by and by resolves itself into colourless crassamentum and serum. It is to be remarked that for this process to succeed, the filtering paper must neither be so loose in texture as to give passage to the corpuscles; nor so close as to retard the oozing through of the liquor sanguinis, and thus give time for coagulation to take place.

The question has been much agitated whether coagulation of the blood be owing to purely physical causes, or whether it is a vital process excited by an external stimulus, or whether it is the effect of loss of vitality. Instead of attempting to determine the validity of one or other of these opinions it may be asked with Henle,† why does the blood circulating in the vessels *not* coagulate? A satisfactory answer, perhaps, to this question may be given by saying, that the liquor sanguinis being constantly pervaded by the red corpuscles is elaborated by them, and that the coagulable part of the blood is taken up as quickly as it is formed. If the view here taken be well founded, it may serve as a step towards the solution of the above questions.

MODE OF FORMATION OF THE BUFFY COAT. In the healthy condition of the blood, no separation of the liquor sanguinis from the red corpuscles takes place naturally; but in certain states of the system, in inflammations especially, the blood soon after being drawn undergoes the separation to a greater or less amount. The liquor sanguinis separated from the red corpuscles collects at the top, and its fibrin in a short time coagulating, the well-known buffy coat is formed In such a case, if, before coagulation, some of the clear liquor sanguinis, as soon as it rises to the top in sufficient quantity, be removed with a spoon, it will in a short time be found to coagulate, and to separate into a colourless crassamentum and serum.

In the liquor sanguinis which rises to the top in inflammatory blood, colourless corpuscles are found in great number. The cause of the colourless corpuscles rising to the top with the liquor sanguinis is, I believe, their small specific gravity, an attraction for the liquor sanguinis, and a want of attraction for the red corpuscles, to be noticed below. Their great number appears to be owing to this, that the whole of the corpuscles of this kind, which were diffused through the quantity of blood drawn, are now collected in the small quantity of liquor sanguinis

* Experimental Inquiries: Part the first, containing an Inquiry into the Properties of the Blood, &c. 2d ed. p. 12. London, 1771.

† Allgemeine Anatomie, &c., 1841.

which has risen to the top. I do not deny but that the colourless corpuscles may be more numerous in buffy than in healthy blood, but the explanation just given renders it probable that they are actually not so very much more so as might at first have been supposed. In corroboration of this I would add, that in all my microscopical examinations of blood, which afterwards became buffed, I cannot say that a very great increase in the number of colourless corpuscles was noticed.

When the fibrin of the liquor sanguinis coagulates to form the buffy coat, the colourless corpuscles are entangled among the fibres and minute granules into which the fibrin solidifies. A portion of the buffy coat, examined under the microscope, thus appears as a fibrous tissue, containing, interspersed through it, nucleated corpuscles.

The colourless corpuscles contained in the liquor sanguinis which has risen to the top in inflammatory blood, have been supposed to form by their coalescence the buffy coat. This inaccurate interpretation of the matter, first given by Mr. Addison,* has been assented to by Dr. Barry.

The separation of the liquor sanguinis, to a greater or less amount, from the red corpuscles in inflammatory blood, and its non-separation in healthy blood, has been variously accounted for. A very generally received view is, that the separation of the liquor sanguinis from the red corpuscles giving rise to the buffy coat, is owing to the blood, on such occasions, coagulating more slowly, and the corpuscles thus having time to subside. In regard to this, however, it is sufficient only to remark, that the separation of the liquor sanguinis from the red corpuscles may often be observed to have taken place long before the time at which healthy blood usually coagulates.

As will be seen in the course of this paper, I do not deny that the greater specific gravity of the red corpuscles has some share in their separation from the liquor sanguinis, but that it plays a subordinate part merely in the process, is proved by the circumstance that "the separation of the fibrin (liquor sanguinis) from the colouring matter (red corpuscles) in such cases, takes place in films of blood so thin as not to admit of a stratum of the one being laid above the other; they separate from each other laterally, and the films acquire a speckled or mottled appearance, equally characteristic of the state of the blood as the buffy coat itself, as shown by Schroeder Van der Kolk."†

The separation of the liquor sanguinis from the red corpuscles, which occurs in healthy blood kept fluid by a neutral salt, and which appears to be owing really to subsidence of the red corpuscles, is no illustration of the way in which the separation takes place in inflammatory blood; as in the former case, the separation proceeds much more slowly than in the latter. By the action of the neutral salt the red corpuscles are contracted and rendered specifically heavier; but it is to be remembered that the liquor sanguinis will at the same time be rendered specifically heavier also, in consequence of the salt dissolved in it. That the specific gravity of serum, at least, is increased in greater proportion than that of the red corpuscles, by admixture with a neutral salt, seems to be proved by the experiment of taking two portions of a mixture of serum and red corpuscles, and adding sulphate of soda to the one and none to the other. In that to which no salt has been added, the corpuscles subside both more quickly and more completely. It is, moreover, to be remarked, with Hewson, that the red corpuscles more readily subside in inflammatory blood from the surface of the whole mass of blood than they will afterwards do from the surface of a mixture with the serum alone.

In an experiment on two portions of a mixture of corpuscles and serum—the one from blood on which no buff formed, the other from blood on which there was a very thick buff—I found that the corpuscles subsided more rapidly in the latter than in the former, though much less rapidly than the separation of the corpuscles from the liquor sanguinis in the formation of the buffy coat takes place.

* Medical Gazette, vol. xxvii. pp. 477-689. Both Mr. Addison and Dr. Barry appear to be unaware that the existence of colourless corpuscles in the buffy coat is no new observation.

† Alison, Outlines of Physiology, Edinburgh, 1839, p. 89.

Hewson's observations led him to remark, that "something more than merely a lessened disposition to coagulate is necessary for the forming of the crust or size." The opinion he entertained was, that the buffy coat probably depended solely upon a change in the coagulable lymph, which, in inflammatory blood, he supposed to be attenuated and specifically lighter; hence, allowing the red corpuscles to subside more rapidly. Dr. John Davy* agrees with Hewson in supposing that the formation of the buffy coat depends chiefly on a greater tenuity of the coagulable lymph. It is to be observed, however, that though blood on which a buffy coat afterwards forms, appears thinner than natural, this is not owing to any increased tenuity of the coagulable lymph, or, more properly speaking, liquor sanguinis, but to a diminution in the number of red corpuscles. Certainly the liquor sanguinis is not less viscid than natural; and, notwithstanding Dr. Davy's inference that there is no necessary connexion between the quantity of fibrin in the blood and its tendency to exhibit the buffy coat, it is now generally admitted that the blood which exhibits the buffy coat in a well-marked manner contains proportionally more fibrin than healthy blood. The opinion of Hewson and Davy thus appears to be untenable, except as regards certain cases in which the blood is unusually thin, from a diminution in the quantity of fibrin, and in which the red corpuscles readily subside, leaving a liquor sanguinis at the top, which yields no consistent buffy coat by coagulation, but merely flakes of fibrin suspended in serum, like moss in water.

In blood in which the buffy coat forms in a well-marked manner there is not only an increase in the quantity of fibrin, but also an increase in the quantity of liquor sanguinis in general, and a diminution in the number of red corpuscles. This relatively greater amount of liquor sanguinis, with its increased proportion of fibrin, is no doubt a condition contributing in some considerable degree to the development of a well-marked buffy coat, but not exactly, as has been supposed, by promoting subsidence of the red corpuscles.

Dr. Alison (*ut supra*, p. 88) is of opinion that the formation of the buffy coat depends on an unusual tendency to separation between the fibrin and corpuscles; but considers it doubtful whether this is owing to increased aggregation among the particles of each, or to a peculiar repulsion between the two. That it is principally owing to an increased aggregation of the red corpuscles, will be shown in the following pages. Here I would observe that any increase in the aggregation of the particles of fibrin would, if it regarded rapidity, lead only to more speedy coagulation; and, if it regarded closeness, to the formation of a more firm coagulum, but not to separation. Besides, before any aggregation at all of the particles of fibrin is manifested, separation of the liquor sanguinis from the red corpuscles has already taken place to the extent of constituting the condition, visible to the naked eye, for the formation of the buffy coat. As to the question of a repulsion between the fibrin, or rather the liquor sanguinis, and red corpuscles, that will be considered below.

The minute process leading to the formation of the buffy coat was, I believe, first explained by Professor Hermann Nasse, of Marburg, and has since been noticed by Professors Rudolph Wagner and Henle. More recently, I have made some observations on the point.

Before entering upon an explanation of the subject it is necessary to call attention to some appearances presented by newly-drawn healthy blood under the microscope. If a drop of such blood spread out by having a thin plate of glass gently laid over it, be quickly transferred to the microscope and forthwith examined, the corpuscles are observed dispersed confusedly about in the liquor sanguinis. In the course of half a minute, however, they are seen to overlap each other, then, rising up on edge, to become fully applied side to side. By this arrangement, like coins in rolls, they occupy less space than when they are irregularly aggregated. The consequence is that the field of the microscope, which was at first uniformly scattered over with corpuscles, now presents spaces containing nothing but liquor sanguinis, with perhaps a single red or colourless corpuscle floating about in it.

* Researches, Physiological and Anatomical, vol. ii. p. 48.—London, 1839.

These spaces represent, as it were, the meshes of an irregular network, formed by the rolls of corpuscles. When the red corpuscles run together into rolls, one may *occasionally* be observed here and there single, but the colourless corpuscles always remain isolated, exhibiting not the slightest attraction for the red corpuscles. After the corpuscles have continued aggregated for a minute or two, a heaving to and fro is usually observed among the rolls, which thus become broken up, and ultimately the corpuscles are more or less detached from each other.

This arrangement of rolls in a *network* is exhibited only when the blood is thinly spread out as above described. When the blood is examined under the microscope in the form of a coagulated drop, the rolls are observed to be disposed in every direction, and thus constitute a *spongework*, in the interstices of which the liquor sanguinis is contained.* A larger quantity of blood, a cupful for example, examined with the naked eye when coagulating, presents on the surface an appearance like jasper; the red mossy-like part being represented by masses of aggregated corpuscles, the transparent instertices by the liquor sanguinis.

What is the nature of the force which causes the mutual approach of the corpuscles and their aggregation into rolls?

A repulsion between the liquor sanguinis and corpuscles, whereby the former tends to separate itself from the latter on a principle similar to the separation of oily fluids from moist substances, has been suggested; but to this it may be replied, that were the force solely of this nature, its effect would be merely to cause an irregular aggregation of the corpuscles, instead of the remarkably regular arrangement which obtains, nor would the corpuscles become detached again from each other, and be promiscuously dispersed through the liquor sanguinis.

The conclusion which a consideration of all the circumstances leads to is, that the force which causes the mutual approach of the corpuscles and their aggregation into rolls, is, in addition to a want of attraction or actual repulsion between the liquor sanguinis and red corpuscles, a special attraction which the latter have for each other, but whether of a vital or physical nature it is not necessary for our purpose to stop here to inquire. It may, however, be remarked that the activity of this attraction is capable of being very much modified by the vital state of the corpuscles on the one hand, and the composition of the fluid in which they are suspended on the other.

The condition on which Dr. Nasse was the first to show that the separation of the liquor sanguinis from the red corpuscles in the formation of the buffy coat principally depends, is an increase in the natural disposition of the red corpuscles to run together.

In that state of the blood in which the buffy coat appears, there is then, together with the known diminution in the quantity of red corpuscles, an exaltation of their natural disposition to run together into rolls, and these again to form a spongework, whereby, the corpuscles being aggregated together more closely, the liquor sanguinis, which is in such cases proportionally increased in quantity, is in a greater measure pressed out as if from a sponge, and of course collects at the top; the corpuscles readily subsiding on account of their greater specific gravity being favoured by closer aggregation.

The view here stated may be illustrated by a sponge soaked in melted tallow—the sponge representing the aggregated corpuscles, the melted tallow the liquor sanguinis. The sponge, let it be supposed, is capable of being drawn together by an intrinsic force, instead of requiring to be pressed together by a force from without.

If, then, before the melted tallow concretes, the sponge be not drawn together, the tallow when concreted will be wholly contained in the meshes of the sponge, though perhaps in greater quantity towards the surface than below. Such is the clot of healthy blood—the spongework formed by the rolls of red corpuscles is not drawn together very closely before the coagulation of the liquor sanguinis takes place,

* This has been pretty well represented in the less magnified of the two figures of a drop of coagulated blood, given by Sir E. Home in the Phil. Trans. for 1818, but the meshes he absurdly describes as being produced by and filled with the carbonic acid gas, which he erroneously supposed was given off by the blood in the act of coagulating.

the coagulated fibrin of this, therefore, is wholly dispersed throughout the meshes, formed by the aggregated corpuscles, but in greater quantity towards the top; hence the greater firmness of the clot there than at the bottom. If on the contrary, before the melted tallow concretes, the sponge be drawn together, a greater or less quantity of the melted tallow will be pressed out and collecting above the contracted, and therefore specifically much heavier sponge, will, when concreted, form a more or less thick cake. Such is an humble illustration of the mode in which the buffy coat is developed. The spongework formed by the rolls of corpuscles is drawn together so closely that the liquor sanguinis is pressed out in more or less considerable quantity, and having collected at the top coagulates, and thus gives rise to the buffy coat. In this case, in consequence of the very great determination of the liquor sanguinis towards the top, the bottom of the mass of blood contains very little of it; hence, after coagulation, the bottom part of the crassamentum is soft and easily broken up from the deficiency of fibrin.

The following cases exemplify the minute process leading to the formation of the buffy coat.

CASE I. Blood drawn from the arm of a young man labouring under pericarditis. By the time a small drop of blood could be transferred to the microscope and observed, the corpuscles had run together into rolls, and these again had become arranged in a netlike manner. The meshes of the network formed by the rolls were very large, and contained nothing but liquor sanguinis. The heaving to and fro of the rolls occurred sooner, and was to a much greater degree than is observed in healthy blood. The disruption of the rolls here and there which ensued was not followed by separation of the corpuscles from each other, but by closer aggregation into isolated masses. The individual corpuscles appeared distinctly thinner than usual, but whether this was connected with their closeness of aggregation—a closeness so great that the corpuscles appeared almost as if fused together—or was owing to a real diminution in thickness, it is impossible to decide, as they were not seen singly in consequence of aggregation into rolls having taken place before any microscopical examination could be made.

The blood drawn from the arm was received into a glass. A thick stratum of liquor sanguinis soon rose to the top, and remained liquid some time. It then coagulated, and formed a thick and well marked buffy coat.

CASE II. The blood drawn from a young man, æt. eighteen, labouring under peritonitis. As the blood was flowing from the vein a minute drop was caught, quickly transferred to the microscope, and immediately examined. The corpuscles had run together into rolls, leaving in the field of the instrument large spaces of liquor sanguinis. The corpuscles appeared thinner and softer than natural, and as if fused together. Heaving to and fro of the rolls, well marked with eventual disruption here and there, and aggregation into heaps. Some of the blood thinly spread out exhibited very distinctly to the naked eye the mottled appearance described by Schroeder Van der Kolk. In the course of some minutes after abstraction, the blood presented a pretty thick buffy coat.

CASE III. The blood of a woman in the last month of pregnancy: aggregation of the corpuscles took place with rapidity, and became very close, leaving large spaces of liquor sanguinis. A buffy coat formed.

CASE IV. Blood drawn from a man with pneumonia of two days' standing. The corpuscles ran together very quickly. They formed here and there very closely aggregated masses of rolls, separated by large intervening spaces. The transverse line of demarcation in the rolls, between the corpuscles, was very indistinct, the corpuscles appearing as if fused together. The corpuscles were more pliable, and appeared as if viscid on the surface. Liquor sanguinis rose to the top of the blood in the vessel, very quickly and in large quantity. After continuing fluid for some time it coagulated, and gave rise to a very thick and firm buffy coat.

But not to multiply cases—let it suffice to say that the numerous observations I have made all agree generally with what has now been stated:—relative diminution in the number of red corpuscles and increase in the quantity of liquor sanguinis;—more rapid and closer aggregation of the red corpuscles into rolls, and these again eventually into masses with large intervening spaces, containing li-

quor sanguinis;* greater thinness and increased pliability of the red corpuscles, with an appearance frequently of viscidity of their surface, and as if they were fused together: these states have all presented themselves to a degree directly in proportion to the thickness and firmness of the buff which has afterwards formed on the blood, and the extent to which it has become cupped.

Relative diminution in the number of corpuscles and an increased quantity of liquor sanguinis, richer in fibrin, are facts well known regarding inflammatory blood. The more rapid and closer aggregation of the red corpuscles into rolls, and these again eventually into masses with larger intervening spaces, containing liquor sanguinis, now fully ascertained as microscopically characteristic of that state of the blood in which the buffy coat appears, is evidently merely an exaltation of the same attraction which exists in the healthy state. In reference to it, can the increased quantity of fibrin in the liquor sanguinis be looked upon in any other light than as a collateral circumstance? That the fibrin of the liquor sanguinis has no essential influence on the mutual approach of the red corpuscles, and their aggregation into rolls in the healthy state is shown by the fact that the phenomenon takes place in serum after the fibrin has been removed by coagulation or by beating. In buffy blood the corpuscles retain the disposition to run together after the fibrin has been removed longer than in healthy blood. That the increased disposition to aggregate, however, is connected with the increased quantity of fibrin in the liquor sanguinis, would appear from the circumstance that red corpuscles of fresh healthy blood, and even red corpuscles which have had their tendency to run together very much diminished from the length of time the blood has been drawn, run together perhaps somewhat more quickly, when they are mixed with a little of the liquor sanguinis, which has risen to the top to form a buffy coat. To this it may be added, that the red corpuscles, as shown by Nasse, aggregate very rapidly and closely in mucilage of gum, containing a little salt in solution, but this aggregation is irregular and confused in comparison of the regular arrangement of the corpuscles in liquor sanguinis. The material changes observed in the red corpuscles themselves, no doubt, have a direct and intimate connexion with the rapidity and closeness of their aggregation. Besides the changes we have above mentioned, Dr. Nasse says, that the darker the corpuscles are in colour the more quickly do they unite. But the question here rises—what connexion is there between these material changes in the red corpuscles and the material changes in the liquor sanguinis? The appearance of the red corpuscles is known to be very much influenced by the nature of the liquid in which they are suspended, therefore the changes exhibited by the red corpuscles of inflammatory blood may be to some extent owing to the changes in the liquor sanguinis; but, on the other hand, may not the changes in the liquor sanguinis be owing in some degree to the state of the red corpuscles?

It has been observed that the meshes of the network, represented by the peculiar arrangement of the rolls of corpuscles, are larger than are presented by healthy blood under the same circumstances, and that by and by, in consequence of the heaving to and fro, disruption of the rolls takes place here and there, followed by their running together into heaps separated by large intervening spaces of liquor sanguinis. Lest this should lead to the supposition that the spongework arrangement of the rolls of red corpuscles in blood with the buffy coat, must have larger meshes than are found in healthy blood, it is to be remembered, that the large spaces of liquor sanguinis seen under the microscope in a thin stratum of blood are owing to the relative fewness and to the closeness of aggregation of the corpuscles, and that they would in a mass of blood be filled up by other rolls running in all directions, and this very closely, like the fibres of a compressed sponge. The

* In cases in which the blood is thin and watery, large spaces left by the aggregation of the corpuscles may be observed under the microscope, and as mentioned at p. 591, separation of the red corpuscles and liquor sanguinis may take place. This separation, however, appears to be principally the effect of gravity favoured by the little tendency to coagulation, for the changes in the red corpuscles above described are not observed to exist. This circumstance therefore deserves to be well considered in any microscopical examination of the blood, made with a practical view.

result is, as already explained, the meshes of the spongework represented by the peculiar arrangement of the rolls of corpuscles, are in inflammatory blood really very small, and hence contain comparatively little liquor sanguinis. It is however to be remarked that the spongework represented by the aggregated rolls of red corpuscles, in becoming more condensed, is cleft by large fissures, which become filled with liquor sanguinis,—a circumstance tantamount to a sponge being divided into several pieces, but each piece continuing in its compressed state. This, which may be seen on examination before the surface of the blood becomes wholly covered over with the liquor sanguinis, is a higher degree of the state giving rise to the jasper-like appearance above described in healthy blood in the act of coagulating.

The greater size of the spaces between the rolls of red corpuscles in a thin film of blood on which the buffy coat is to form, than in healthy blood, is the cause of the mottled appearance presented to the naked eye, already referred to as having been signalized by Schroeder Van der Kolk, as equally characteristic of the state of the blood as the buffy coat itself. The reason why it is so, it will now be perceived, is, that the appearance is owing to the same cause.

The minute process leading to the separation of the liquor sanguinis from the red corpuscles—the visible condition for the formation of the buffy coat—consists then in an exaltation both of the rapidity and closeness with which the red corpuscles naturally aggregate into rolls, and these again into a spongework, thus squeezing out the liquor sanguinis from among the corpuscles, and allowing the greater specific gravity of the latter to come more fully into play, whereby the liquor sanguinis, which in such cases is in relatively greater quantity, collects at the top, and coagulating, gives rise to the buffy coat.

With a practical knowledge of the appearances above described, it is in our power to infer from the examination of a minute drop of blood drawn from a prick of the finger, as much at least of the state of the blood as can be done from the presence or absence of a buffy coat.

Physiology of the Corpuscles of the Blood. The common way of viewing the blood merely as a fluid has been a great obstacle to the establishment of clear notions regarding its vitality; but if the blood be viewed as a fluid containing suspended in it regularly organized solids, the question of its vitality becomes much more precise, simple and intelligible. The organized corpuscle may as easily be conceived to possess the essential attributes of vitality as any organ in the body; but the liquor sanguinis in which it is suspended is not organized, and can therefore be looked upon merely in the light of a chemical solution,—a solution, however, depending on nicely-balanced affinities kept in play by the vital influence of the corpuscles, and the compositions and decompositions incessantly going on in it. Though not organized and living, the liquor sanguinis, or more properly speaking some of the matters contained in it are strongly disposed to become so under certain conditions. The blood then may be viewed as consisting of organized and living solids, and of a fluid containing in solution matters highly susceptible of organization and life.

The organized and living solids of the blood are the corpuscles, red and colourless.

Nature and Uses of the red Corpuscles. The red corpuscles, according to the best physiologists, are not expended immediately for the purposes of nutrition and secretion. It is from the liquor sanguinis only which permeates the walls of the capillaries, that are derived the materials for nutrition, growth, and the various secretions.

A large number of observations have lately been brought together, however, in defence of the view that the red corpuscles are the material out of which the tissues are directly formed; but Dr. Martin Barry, the author of these observations, has, unfortunately, mistaken changes in the blood-corpuscles, arising from decomposition and from mechanical and chemical agencies for natural vital changes, and has confounded blood-corpuscles with other corpuscles, quite different in their nature. And he has not only failed to demonstrate the link in the chain of evidence

required to establish the view he advocates, but has equally failed to adduce any valid arguments against the opposite view.*

If the red corpuscles do not immediately contribute to nutrition, growth, and the secretions, what then is their function, seeing that their presence is as necessary as that of the liquor sanguinis? Do the red corpuscles act merely as "carriers of oxygen,"—or do they in addition to this maintain by their presence the excitability of the organs?

The red corpuscles considered as carriers of oxygen. The circumstance that change of tint of the red colour of the corpuscles is the only visible manifestation that the blood has lost or acquired oxygen, has led to the opinion that they are the medium through which that gas is carried to all parts of the system. But there is no reason to suppose that the liquor sanguinis less readily absorbs, and is less a carrier of oxygen than the corpuscles. Moreover it is to be remarked that the absorption of oxygen by the red corpuscles might be looked upon as accessory to some peculiar function performed by them, rather than as being solely for the purpose of distributing the oxygen to the different parts of the system.

Though the red corpuscles may not be mere carriers of oxygen, they still bear a relation to its consumption. Though not themselves expended in nutrition, the red corpuscles are intimately connected with its activity, and that in a manner which it will be endeavoured to explain below. Now as the activity of nutrition has a relation to the amount of oxygen consumed, so also must the activity of the function of the corpuscles.†

* As an example of Dr. Barry's observations, the following may be adduced. He figures and describes muscle in the act of being developed from blood-corpuscles. The subject of his observation was *pressed out with mucus from the fallopian tube of a rabbit* killed ten hours *post coitum.* The red corpuscles, "new cells," were arranging themselves to form muscle. "It is not needful," says he, "to refer to the observations of others, since the objects figured by myself *were obviously muscular fibres* (the future fasciculi) in the earliest stages of formation. There is therefore, it appears, a direct transition of blood discs into the elementary parts of muscle." (Phil. Trans. 1840, Part II, p. 605, Pl. xxx, figs. 14 to 17, inclusive.)

The explanation of the appearance observed, but so grossly misinterpreted, is this:—when blood is mixed with certain muculent secretions, the red corpuscles tend to arrange themselves, as usual, in rolls, but at the same time becoming somewhat distended by the absorption of fluid they appear like rows of beads. Thus if a minute drop of blood drawn from a prick of the finger be mixed with a drop of urethral mucus, and the whole covered with a thin plate of glass and examined under the microscope, the red corpuscles are observed to have become somewhat distended, and to be arranged in many places in single rows, like beads, for the most part parallel. The rows are exactly like what Dr. Barry has delineated, but the most remarkable phenomenon attending this state of the red corpuscles Dr. B. does not appear to have observed,—it is a locomotive power exhibited by the rows of corpuscles. They are observed to move across the field of view somewhat like worms, but very slowly. Even single corpuscles move onwards with a sort of vermicular motion, or like a polygastric infusorium when moving very slowly. This movement appears, however, not to be owing to any contractile power within the corpuscles, but to be determined by attraction for each other and for the aggregations of corpuscles towards which their movements tend. The apparent peristaltic motion appears to be owing to the flaccidity of the corpuscles. A partially filled bladder moving along any surface would present the same appearance. The flacid state of the corpuscles, it is to be remarked, is a necessary condition for its progression, for when a reagent, such as a solution of salt, is applied, the corpuscle shrinks, and is arrested in its movements.

The mistaking of red corpuscles distended by fluid, and arranged in rows like beads, for muscular fibres is, supposing all the attending circumstances of the case abstracted, a conceivable error; but to suppose that blood-corpuscles effused into and mixed with the mucus of the Fallopian tube should there form muscle, is a most extraordinary illusion. For what possible purpose, it may be asked, could muscular fibre be formed in the mucus of the Fallopian tube?

† The relation of the activity of the function of the corpuscles with the amount of oxygen consumed, referred to in the text, it will be seen is indirect, but as in the course of the performance of their function the corpuscles absorb oxygen, and as the oxygen thus absorbed may be accessory to the function of the corpuscles, a direct relation is also to be inferred.

In addition to being carriers of oxygen do the red corpuscles maintain by their presence the excitability of the organs? The presence of blood is necessary to maintain the excitability of the organs, but whether the red corpuscles are in this case the sole and direct agents is a question not decided.

John Hunter has remarked that the red corpuscles are connected principally with the strength and vigour of the animal—less with nutrition than with action. But action presupposes nutritive change. The fact appears to be that to "maintain the excitability of the organs," is simply to minister to the nutritive changes which are incessantly going on, and which cannot be stopped without stopping action. Hence, as has been said above, in regard to their relation to the consumption of oxygen, the red corpuscles maintain the excitability of the organs, only inasmuch as they contribute to nutrition. The mode in which they do this, as yet merely alluded to, I now proceed to investigate.

The red corpuscles considered as glandular cells. Numerous well-known circumstances combine to show that a process of elaboration goes on within the blood-vessels, whereby matters fitted for assimilation and secretion are prepared from the raw materials entering the blood. As regards the secretions, indeed, some physiologists suppose that they are formed independently in the blood, and are merely separated therefrom by the glands as filters.

The elaboration which goes on within the blood-vessels is partly of a chemical and partly of a vital nature. New chemical compounds are formed, and matters without undergoing any appreciable chemical change are rendered more highly organizable.

What are the agents of this elaboration? While the red corpuscles of the blood have been looked upon as mere carriers of oxygen, or as agents for maintaining the excitability of the organs, the elaboration of the liquor sanguinis out of the various matters poured into the blood-vessels, has generally been attributed to the lungs by those who have justly apprehended some elaboration necessary for the production of the liquor sanguinis—the liquid whence the materials for nutrition and secretion are immediately derived. But the great and perhaps sole function of the lungs is to serve as the medium through which oxygen is taken into, and carbonic acid gas excreted from the blood.

A view has been of late years gaining ground that the special agents of the secretory process are the nucleated corpuscles which constitute the epithelium of the interior of the cells and canals of glands.*

The view just referred to has led to the conjecture that the red corpuscles are the agents of the elaboration of the liquor sanguinis. Wagner remarks that the red corpuscles might be presumed to bear the same relation to the plasma and its normal composition, as the cells of secreting glands do to the secreted fluids;† and in his excellent volume on General Anatomy, Professor Henle, of Zurich, calls the red corpuscles *swimming glandular cells.* It is only necessary to compare for a moment the red corpuscles of the blood with the epithelium corpuscles of glandular surfaces to detect a striking similarity in structure and relations, nor does it require much reflection to perceive the likelihood of an analogy in function.

I agree with Henle in supposing that the red corpuscles draw from the raw materials of the liquor sanguinis a matter, elaborate it, and when elaboration is perfected give back the matter, becoming at the same time melted down in the liquor sanguinis, thus disappearing like the epithelium-cells of glands. In short, as the secretory corpuscles of glands elaborate the secretions from the liquor sanguinis poured out amongst them, so from the new matters constantly entering the blood, the red corpuscles elaborate the liquor sanguinis. The secretory corpuscles of glands are constantly being thrown off, or resolved into a part of the secretion, but are as constantly reproduced; in like manner the red corpuscles of the blood are constantly being melted down into certain of the materials of the liquor sanguinis, but are as constantly being reproduced.

* Purkinje. Report of the Meeting of Naturalists at Prague in 1837. Isis, 1838, No. 7. Dutrochet had well observed that all cells are, properly speaking, secretory organs.

† Physiology, by Dr. Willis. Part II, p. 448.

It is a question how fibrin is formed in the animal body. The true starting-point in nutrition, says Liebig, is albumen. The various protein compounds used as food are, by digestion, all resolved into albumen. In lymph and chyle, some fibrin presents itself, but it is in the blood that that proximate principle is first formed in any considerable quantity, and endowed with the strong and peculiar tendency to become organized. The more peculiar object of the elaboration supposed to be performed by the red corpuscles is probably the conversion of one protein compound into another—albumen into fibrin—a less into a more highly organizable proximate principle.

We have seen that the quantity of fibrin is increased in blood, which shows the buffy coat, and the number of red corpuscles diminished; and we have seen reason to believe from the changes exhibited by the red corpuscles that their action is increased in inflammation, and those other states of the system in which the buffy coat forms on the blood. The result of the increased action of the red corpuscles here assumed, I consider to be the augmentation of fibrin in the liquor sanguinis. This augmentation of fibrin is at the expense, not only of the albumen of the serum but also of the red corpuscles themselves; for by their increased action on the albumen of the serum, the red corpuscles are themselves more quickly exhausted and resolved, therefore, in greater quantity into fibrin than in health.

The Colourless Corpuscles considered in their Relations as one of the Components of the Blood. Leaving out of view the origin and ultimate destination of the colourless corpuscles, it is purposed to consider them here only in their relations as one of the components of the blood. As in blood examined out of the body the colourless corpuscles appear very insignificant, and as an idea of their importance is to be obtained perhaps only by viewing them in the blood as it circulates in the transparent parts of living animals, I would direct attention to the condition of the colourless corpuscles in, and their mode of passage through the minute arteries, the capillaries, and radicles of the veins.

Condition of the colourless corpuscles in, and their passage through the minute arteries, the capillaries, and radicles of the veins. When the circulation in the web of the hind-foot of the frog is carefully observed under the microscope, the colourless corpuscles are seen accumulated at the inner surface of the wall of the vessels, along which they move very slowly in comparison of the red corpuscles, which occupy the axis of the current. Besides the difference in rapidity, there is a difference in the mode of progression of the colourless and red corpuscles. Whilst the red corpuscles are carried directly onwards with the liquor sanguinis, the colourless ones roll along over and over like round pebbles at the bottom of a stream of water; sometimes only are they pushed or carried along without rolling. Frequently, when the general current of blood is slow, a number of colourless corpuscles is observed to be stationary, giving to the vessel an appearance as if it were lined with an epithelium of globular corpuscles; a few of which are every now and then becoming detached from the rest and roll along. In the minute arteries when the velocity of the stream of blood is great, the colourless corpuscles are mingled and carried along with the red ones like stones in a rapid current of water; but if the velocity of the stream be diminished, the colourless corpuscles are observed to extricate themselves from among the red ones, and as stones seek the bottom when the force of a current is diminished, come in contact with the wall of the vessel along which they now slowly roll. Through the smaller capillaries the colourless corpuscles pass one by one indiscriminately with the red ones. It is principally in the radicles of the veins that they accumulate in such numbers as actually to line the walls of the vessel like an epithelium.

The peculiar relation of the colourless corpuscles to the walls of the vessels suggested to Poiseuille, (who, it is to be remembered, however, appears not to have perceived any distinction between the colourless and red corpuscles,) the idea that, like what was shown by Girard to take place when a fluid passes through a tube of small diameter, the current of blood is less rapid towards the wall of the vessel, and the stratum in immediate contact with it altogether stationary. This, however, is not altogether a correct view of the phenomenon, as will immediately be seen.

Attractions and repulsions of the red and colourless corpuscles. From the facts stated in the preceding part of this paper it may be admitted as fully established that the red corpuscles have an attraction for each other, but none for the colourless corpuscles. The accumulation of the colourless corpuscles at the sides of the vessels proves, as already shown by Ascherson and Weber, the existence of an attraction between them and these walls. The circumstance that the red corpuscles under the ordinary natural circumstances never adhere to the walls of the vessel, is a pretty sure indication of an absence of attraction between these parts, if not of the existence of actual repulsion. The red corpuscles keep together in the axis of the stream by virtue of the attraction they have for each other, but this attraction does not operate within the vessels to so great an extent as is observed in blood just abstracted, otherwise stagnation of the blood would infallibly take place. The cause of this I am inclined to believe is, that when by virtue of attraction contact takes place between the corpuscles, repulsion ensues just as when two bodies which by reason of their being in different states of electricity attract each other, are repelled immediately on contact. The breaking up in the course of a few minutes of the rolls into which the red corpuscles aggregate immediately when drawn, above described, is owing, perhaps, to an imperfect exertion of the same repulsion, or at least cessation of attraction between the red corpuscles here supposed to supervene on contact.

A knowledge of the attractions and repulsions just mentioned appears calculated to throw some light on the circulation in the capillaries including the terminations of the arteries and radicles of the veins.

Circulation of the blood in the capillaries, including the terminations of the arteries and radicles of the veins. From the circumstance that the liquor sanguinis passes through the walls of the minute vessels by imbibition, it is to be inferred that there is an attraction between it and these walls. This being the case, the liquor sanguinis of the circulating blood in contact with the walls of the vessels will be retarded in its course, just as takes place in the passage of water through narrow tubes of glass. The red corpuscles, by virtue of their attraction for each other and repulsion, or want of attraction, for the walls of the vessels, keep in the axis of the stream, whilst the colourless corpuscles, by virtue of their attraction for the walls of the vessels, and their want of attraction, or repulsion, for the red corpuscles, apply themselves to the walls. Being thus in the less rapid stratum of liquor sanguinis they are either not at all or very slowly carried along. The rolling over and over mode of progression which they so often exhibit, appears to me to be caused by the onward movement of the string of red corpuscles aggregated in the axis of the stream, acting in the same way as a log of wood does in carrying along with it the balls or rollers placed underneath, in order that it may be moved more easily. It is indeed probable that the colourless corpuscles do actually in this way facilitate, or at least offer less obstruction to the course of the stream of red corpuscles than if, considering their attraction for the walls of the vessels, they had to have been pushed along. It is scarcely necessary to remark that the mode of progression of the colourless corpuscles under consideration explains the slowness of their course in comparison of that of the stream of red corpuscles.

It has been stated that in the minute arteries, when the velocity of the stream of blood is great, the colourless are mingled and carried along with the red corpuscles. In this case the attraction between the colourless corpuscles and walls of the vessels is overcome by the force of the current, in the same way as rapid waters overcome the force of gravitation, by raising up from the bottom, suspending, and carrying along even very large stones. As when the force of the stream of water subsides, the stones by virtue of the attraction of gravitation again seek the bottom, so when the force of the stream of blood is diminished by any cause, the colourless corpuscles, by reason of their want of attraction for the red ones, are extricated from among them, and by virtue of their attraction for the wall of the vessel are brought into contact with it.

Mode in which arrestment of the circulation takes place in the capillaries. The absence of attraction or the existence of actual repulsion between the red cor-

puscles on the one hand, and the walls of the vessels and colourless corpuscles on the other, is a most important fact to keep in view. Without this absence of attraction or existence of actual repulsion the passage of the blood through the small vessels would have been impossible. Indeed it is a change in the attractions and repulsions among the red corpuscles which appears to be the cause of inflammatory congestion.

When any irritating substance, a solution of common salt for example, is applied to the web of the frog's hind-foot, or when the part is wounded, the congestion which supervenes on the temporarily accelerated circulation, is observed under the microscope to commence by the red corpuscles agglomerating together and applying themselves here and there flat against the wall of the vessel, and adhering to it.* Other red corpuscles apply themselves to those already adherent and complete stagnation ensues. The blood in the lungs of the frog is observed to be arrested in the same way in the vessels when the part of the lung under observation is touched with solution of salt, or, as I have also found on making the experiment, *when a stream of carbonic acid gas is directed against it.*

The stoppage of the circulation in the capillaries which occurred in Mr. Blake's experiments of injecting different salts into the blood is to be attributed to the same change in the attractions and repulsions of the red corpuscles which is here considered as the immediate cause of the stoppage of the capillary circulation in the cases above described. The stoppage of the circulation in the capillaries of the lungs in asphyxia, it may be inferred from what is above stated of the action of carbonic acid gas, is owing to the same cause.

The colourless contrasted with the red corpuscles in their relations to the nutritive process. In contemplating in their relations with nutrition, &c., the phenomena just described, the first thing that strikes us is the *distended* and *globular* colourless corpuscles, rolling *slowly* along the walls of the minute vessels, whilst the *collapsed flattened* red corpuscles proceed *rapidly* onwards in the axis of the stream. The very natural inference from this is that, as Weber has already observed, there is some reciprocal relation between the colourless corpuscles, and the parts outside the vessels in the process of nutrition; whilst, as I have above endeavoured to show, the red corpuscles have no direct relation with the parts outside the vessels, but are more concerned in the elaboration of the liquor sanguinis.

Cause of the variations in the capillary circulation. It is interesting to consider the different capabilities for endosmose and exosmose, in reference to the liquor sanguinis, possessed by the red and colourless corpuscles, as indicated by their different states of distention, and to compare this with the difference in the attractions and repulsions they exhibit. The changes constantly going on in the blood is attended with variations in the capabilities of the corpuscles for endosmose, and in their attractions and repulsions. These appear to be the cause of the variations which are constantly occurring in the capillary circulation. The force of the heart alone, and not any action of the capillaries, determines the general passage of the blood from the arteries into the veins, but it is to the attractions and repulsions of the corpuscles that the varied peculiar movements of the blood in the capillaries are owing. In considering the circulation, through the capillaries, in short, it is always to be remembered that the blood is not a mere inert fluid, but one containing, in suspension, innumerable organized and living corpuscles endowed with peculiar attractions and repulsions.

The view now given of the nature and uses of the corpuscles of the blood appears calculated to guide to a more correct explanation of many obscure points in physiology and pathology. Without necessitating us to give up any of the arguments of solidism, it puts into our hands all the valuable ones of humorism.

* The observation of Weber that red corpuscles sometimes adhere to the walls of the vessels and are changed into colourless ones, has not been confirmed. All my observations are against it.

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